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PROBABILITY IN QUANTUM THEORY†

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Abstract: For some sixty years it has appeared to many physicists that probability plays a fundamentally different role in quantum theory than it does in statistical mechanics and analysis of measurement errors. It is a commonly heard statement that probabilities calculated within a pure state have a different character than the probabilities with which different pure states appear in a mixture, or density matrix. As Pauli put it, the former represents "Eine prinzipielle *Unbestimmtheit*, nicht nur *Unbekanntheit*". But this viewpoint leads to so many paradoxes and mysteries that we explore the consequences of the unified view, that all probability signifies only incomplete human information. We examine in detail only one of the issues this raises: the reality of zero–point energy.

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INTRODUCTION: HOW WE LOOK AT THINGS

In this workshop we are venturing into a smoky area of science where nobody knows what the real truth is. Such fields are always dominated by the compensation phenomenon: supreme self–confidence takes the place of rational arguments. Therefore we shall try to avoid dogmatic assertions, and only point out some of the ways in which quantum theory would appear different if we were to adopt a different viewpoint about the meaning and functional use of probability theory. We think that the original viewpoint of James Bernoulli and Laplace offers some advantages today in both conceptual clarity and technical results for currently mysterious problems.

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How we look at a theory affects our judgment as to whether it is mysterious or irrational on the one hand; or whether it is satisfactory and reasonable on the other. Thus it affects the direction of our research efforts; and *a fortiori* their results. Indeed, whether we theorists can ever again manage to get ahead of experiment will depend on how we choose to look at things, because that determines the possible forms of the future theories that will grow out of our present ones. One viewpoint may suggest natural extensions of a theory, which cannot even be stated in terms of another. What seems a paradox on one viewpoint may become a platitude on another.

For example, 100 years ago it was a much discussed problem how material objects can move through the aether without resistance. Yet a different way of looking at it would have made the mystery disappear without any need to dispense with the aether. One can regard material objects, not as impediments to the "flow" of aether, but as *parts of* the aether ("knots" in its structure) which are propagating through it. On this way of looking at it, there is no mystery to be explained. As a student at Princeton many years ago, I was fascinated to learn from John Wheeler how much of physics can be regarded as really only geometry, in this way.

Today we are beginning to realize how much of all physical science is really only *information,* organized in a particular way. But we are far from unravelling the knotty question: "To what extent does this information reside in us, and to what extent is it a property of Nature?" Surely, almost every conceivable opinion on this will be expressed at this workshop.

Is this variability of viewpoint something to be deplored? Well, eventually we should hope to present a unified picture to the rest of the world. But for the moment this is neither possible nor desirable. We are all looking at the same reality and trying to understand what it is. But we could never understand the structure of a mountain if we looked at it only from one side. The reality we are studying is far more subtle than a mountain, and so it is not only desirable, but *necessary* that it be examined from many different viewpoints, if we are ever to resolve the mystery of what it is. Here we present one of those viewpoints, which we think has not been sufficiently recognized in the recent literature.

First we note a more immediate example of the effect of how we look at things, to support by physical arguments what is suggested later from probability considerations.

HOW DO WE LOOK AT GRAVITATION AND QED?

In teaching relativity theory, one may encounter a bright student who raises this objection: "Why should such a fundamental thing as the metric of space and time be determined only by gravitational fields—the weakest of all interactions? This seems irrational." We point out to him a different way of looking at it, which makes the irrationality disappear: "One should not think of the gravitational field as a kind of preexisting force which 'causes' the metric; rather, the gravitational field is the main observable *consequence* of the metric. The strong interactions have not been ignored, because the field equations show that the metric is determined by all the energy present." According to the first viewpoint, one might think it a pressing research problem to clear up the mystery of why the metric depends only on gravitational forces. On the second viewpoint this problem does not exist.

But then if the student is very bright, he will be back the next day with another criticism: "If the gravitational field is only a kind of bootstrap effect of the other forces, it raises the question whether the gravitational field should be quantized separately. Wouldn't we be doing the same thing twice?" Thus different ways of looking at what a gravitational field is, might lead one to pursue quite different lines of research.

A similar issue arises in electrodynamics, making a thoughtful person wonder why we quantize the electromagnetic (EM) field. The following observations were made by Albert Einstein, in two lectures at the Institute for Advanced Study, which I was privileged to attend in the late 1940's. Needless to say, very careful transcripts of his words were made.

He noted that in contemporary quantum theory we first develop the theory of electrons via the Schrödinger equation, and work out its consequences for atomic spectra and chemical bonding, with great success. Then we develop a theory of the free quantized EM field independently, and discuss it as a separate thing. Only at the end do we, almost as an afterthought, decide to couple them together by introducing a phenomenological coupling constant 'e' and call the result 'Quantum Electrodynamics'.

Einstein told us: "I feel that it is a delusion to think of the electrons and the fields as two physically different, independent entities. Since neither can exist without the other, there is only *one* reality to be described, which happens to have two different aspects; and the theory ought to recognize this from the start instead of doing things twice."

Indeed, the solution of the EM field equations is, in either classical or quantum theory,

$$A_\mu(x) = \int D(x-y) J_\mu(y) d^4y \ . \tag{1}$$

In quantum theory $A_\mu(x)$ and $J_\mu(y)$ are operators; but since the propagator $D(x-y)$ is a scalar function, the $A_\mu(x)$ in (1) is not an operator on a "Maxwell Hilbert Space" of a quantized EM field – it is an operator on the same space as $J_\mu(y)$, the "Dirac Hilbert Space" of the electrons.

Conventionally, one says that (1) represents only the "source field" and we should add to this the quantized "free field" $A_\mu^{(0)}(x)$ which operates on the Maxwell Hilbert space. But fundamentally, every EM field is a source field from somewhere; therefore the free field is *already* an operator on the Dirac Hilbert space of distant sources; so why do we quantize it again, thereby introducing an infinite number of new degrees of freedom for each of an infinite number of field modes?

The pragmatic distinction between the "free field" and the "source field" does not lie in whether we think the free field is or is not an operator; what matters functionally (*i.e.* what makes a difference in the results of our calculations) is whether it does or does not commute with the local matter variables $J_\mu(x,t)$ at equal times. At first one would think, on grounds of the relativity principle, that these quantities must commute; any theory in which they do not is simply wrong. However, later we shall see that the situation is not quite that simple; it depends on some subtle points about the physical meaning of a propagator, which are not discussed at all in current QED.

One can hardly imagine a better way to generate infinities in physical predictions than by having a mathematical formalism with $(\infty)^2$ more degrees of freedom than are actually used by Nature. Stated differently, then, the issue is: should we quantize the matter and fields separately, and then couple them together afterward; or should we write down the full classical theory with both matter and field, the field equations in integrated form; and quantize it in a single step? The latter procedure (assuming that we could carry it out consistently) would lead to a smaller Hilbert space.

The viewpoint we are suggesting is very different in spirit, but nevertheless very similar in its pragmatic consequences, to the Wheeler–Feynman electrodynamics, in which the EM field is not considered a "real" physical entity in itself, but only a kind of information storage device invented by us. That is, the present EM field is a "sufficient statistic" which merely summarizes all the *information* about past motion of charges that is relevant for predicting their future motion.

It is not enough to reply: "The present QED procedure must be right because it leads to several very accurate predictions: the Lamb shift, the anomalous moment, *etc.*" To sustain that argument, one would have to show that the act of quantizing the free field actually plays an essential role in determining those accurate numbers (1058 MHz, *etc.*), and that, had we dealt with the free field differently, those numbers would be different. But their calculation appears to involve only the Feynman propagators; and mathematically, the propagator $D(x-y)$ in (1) is equally well a Green's function for the quantized or unquantized field (albeit with strange boundary conditions).

The conjecture suggests itself, almost irresistibly, that those accurate experimental numbers come only from the local source fields, which are coherent with the local state of matter. That is, the experimental numbers come only from mathematical expressions of the form $\int\int J_\mu(x)\, D(xy)\, J_\mu(y) dxdy$. Terms such as $\int J_\mu(x) A_\mu^{(0)}(x) dx$ involving the quantized free field in its ground state cancel out because it is uncorrelated with the local state of matter. This has been confirmed in part by the "source–field theory"* which arose in quantum optics some years ago [Milonni *et al* (1973), Senitzky (1973), Allen & Eberly (1975)]. It was found that, at least in lowest nonvanishing order, observable effects such as spontaneous emission and the Lamb shift, can be regarded as arising from the source field which we had studied already in classical EM theory, where we called it the "radiation reaction field". Some equations illustrating this are given below; it is easy to make simple models in which one can prove from the exact solutions that the analogs of spontaneous emission and the Lamb shift are determined entirely by the local source fields.

In these quantum optics calculations it seems that the quantized free field only tags along, putting an infinite uncertainty into the initial conditions (that is, a finite uncertainty into each of an infinite number of field modes) and thus giving us an infinite "zero–point energy", but not producing any observable electrodynamic effects. One wonders, then: *If we don't use it, do we really need it?*†

HOW DO WE LOOK AT BASIC QUANTUM THEORY?

Let us back off for a moment for a more general view of these things. Current thinking about the role of information in science applies to all areas (and in particular to biology, where perhaps the most valuable results will be found). But the most tangled area in present physical science is surely the standard old 1927 vintage quantum theory, where the conceptual problems of the "Copenhagen interpretation" refuse to go away, but are brought up for renewed discussion by every new generation of physicists (much to the puzzlement, we suspect, of the older generation who thought these problems were all solved). Starting with the debates between Bohr and Einstein over sixty years ago, different ways of looking at quantum theory persist in making some see deep mysteries and contradictions in need of resolution, while others insist that there is no difficulty.

How can scientists of unquestioned competence be in sharp disagreement about such things? It must be that we have different unstated premises (hidden assumptions) in the back of our minds. If so, then until we bring out into the open just what those premises are, there would be no possibility of resolving the issue.

Defenders of the Copenhagen interpretation have displayed a supreme self–confidence in the correctness of their position, but this has not enabled them to give the rest of us any rational explanations of why there is no difficulty. Richard Feynman, while defending the QM formalism on grounds of its practical success, at least had the honesty to admit: "Nobody knows how it can be that way."

While we doubters have not shown so much self–confidence, nevertheless for all these years it has seemed obvious to me – for the same reasons that it did to Einstein and Schrödinger – that the Copenhagen interpretation is a mass of contradictions and irrationality and that, while theoretical physics can of course continue to make progress in the mathematical details and computational techniques, there is no hope of any further progress in our basic understanding of Nature until this conceptual mess is cleared up.

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* Not to be confused with Schwinger's 'source theory' which is quite different in outlook and premises, although of course related to, the 'source–field theory' under discussion, in a way not yet entirely clear.

† Indeed, it has long been standard practice in QED calculations that in the Hamiltonian for a field mode, $\hbar\omega(a^\dagger a + 1/2)$, one starts by throwing away the $1/2$, which represents the ZP energy, on the grounds that it produces no dynamical effects because it commutes with all other variables.

Because this position seems to arouse fierce controversy, let me stress our motivation: if quantum theory were not successful pragmatically, we would have no interest in its interpretation. It is precisely *because* of the enormous success of the QM mathematical formalism that it becomes crucially important to learn what that mathematics means. To find a rational physical interpretation of the QM formalism ought to be considered the top priority research problem of theoretical physics; until this is accomplished, all other theoretical results can only be provisional and temporary.

This conviction has affected the whole course of my career. I had intended originally to specialize in Quantum Electrodynamics working with J. R. Oppenheimer; but this proved to be impossible. Whenever I look at any quantum–mechanical calculation, the basic craziness of what we are doing rises in my gorge and I have to stop and try to find some different way of looking at the problem, that makes physical sense. Gradually I came to see that the resolution cannot be found within the confines of the traditional thinking of physicists; the foundations of probability theory and the role of human information have to be brought in, and so I have spent many years trying to understand them in the greatest generality.

The failure of quantum theorists to distinguish in calculations between several quite different meanings of 'probability', between expectation values and actual values, makes us do things that don't need to be done; and to fail to do things that do need to be done. We fail to distinguish in our verbiage between prediction and measurement. For example, the famous vague phrases: 'It is impossible to *specify* $\cdots$'; or 'It is impossible to *define* $\cdots$' can be interpreted equally well as statements about prediction or statements about measurement. Thus the demonstrably correct statement that the present formalism cannot *predict* something becomes perverted into the logically unjustified – and almost certainly false – claim that the experimentalist cannot *measure* it!

We routinely commit the Mind Projection Fallacy: supposing that creations of our own imagination are real properties of Nature, or that our own ignorance signifies some indecision on the part of Nature. It is then impossible to agree on the proper place of information in physics. This muddying up of the distinction between reality and our knowledge of reality is carried to the point where we find some otherwise rational physicists, on the basis of the Bell inequality experiments, asserting the objective reality of probabilities, while denying the objective reality of atoms! These sloppy habits of language have tricked us into mystical, pre–scientific standards of logic, and leave the meaning of any QM result ambiguous. Yet from decades of trial–and–error we have managed to learn how to calculate with enough art and tact so that we come out with the right numbers!

The main suggestion we wish to make is that how we look at basic probability theory has deep implications for the Bohr–Einstein positions. Only since 1988 has it appeared to the writer that we might be able finally to resolve these matters in the happiest way imaginable: a reconciliation of the views of Bohr and Einstein in which we can see that they were both right in the essentials, but just thinking on different levels.

Einstein's thinking is always on the ontological level traditional in physics; trying to describe the realities of Nature. Bohr's thinking is always on the epistemological level, describing not reality but only our information about reality. The peculiar flavor of his language arises from the absence of all words with any ontological import. J. C. Polkinghorne (1989, pp. 78–79) came independently to this same conclusion about the reason why physicists have such difficulty in reading Bohr. He quotes Bohr as saying:

> "There is no quantum world. There is only an abstract quantum physical description. It is wrong to think that the task of physics is to find out how nature *is*. Physics concerns what we can say about nature."

So in Bohr's writings the notion of a "real physical situation" was just not present and he gave evasive answers to questions of the form: "What is really happening when $\cdots$?" Eugene Wigner (1974) was acutely aware of and disturbed by this evasiveness when he remarked:

> "These Copenhagen people are so clever in their use of language that, even after they have answered your question, you still don't know whether the answer was 'yes' or 'no'!"

J. R. Oppenheimer, more friendly to the Copenhagen viewpoint, tried to explain it in his lectures in Berkeley in the 1946–47 school year. Oppy anticipated multiple valued logic when he told us:

> "Consider an electron in the ground state of the hydrogen atom. If you ask, 'Is it moving?' the answer is 'no.' If you ask, 'Is it standing still?' the answer is 'no'."

Bohr would chide both Wigner and Oppenheimer for asking ontological questions, which he held to be illegitimate. Those who, like Einstein (and, up till recently, the present writer) tried to read ontological meaning into Bohr's statements, were quite unable to comprehend his message. This applies not only to his critics but equally to his disciples, who undoubtedly embarrassed Bohr considerably by offering such ontological explanations as "Instantaneous quantum jumps are real physical events." or "The variable is created by the act of measurement.", or the remark of Pauli quoted above, which might be rendered loosely as "Not only are you and I ignorant of x and p; Nature herself does not know what they are."

We disagree strongly with one aspect of Bohr's quoted statement above; in our view, the existence of a real world that was not created in our imagination, and which continues to go about its business according to its own laws, independently of what humans think or do, is *the primary experimental fact of all*, without which there would be no point to physics or any other science. The whole purpose of science is learn what that reality is and what its laws are.

On the other hand, we can see in Bohr's statement a very important fact, not sufficiently appreciated by scientists today as a necessary part of that program to learn about reality. Any theory about reality can have no consequences testable by us unless it can also describe what humans can see and know. For example, special relativity theory implies that it is fundamentally impossible for us to have knowledge of any event that lies outside our past light cone. Although our ultimate goal is ontological, the process of achieving that goal necessarily involves the acquisition and processing of human information. This information processing aspect of science has not, in our view, been sufficiently stressed by scientists (including Einstein himself, although we do not think that he would have rejected the idea).

Although Bohr's whole way of thinking was very different from Einstein's, it does not follow that either was wrong. In the writer's present view, all of Einstein's thinking – in particular the EPR argument – remains valid today, when we take into account its ontological purpose and character. But today, when we are beginning to consider the role of information for science in general, it may be useful to note that we are finally taking a step in the epistemological direction that Bohr was trying to point out sixty years ago.

But our present QM formalism is not purely epistemological; it is a peculiar mixture describing in part realities of Nature, in part incomplete human information about Nature – all scrambled up by Heisenberg and Bohr into an omelette that nobody has seen how to unscramble. Yet we think that the unscrambling is a prerequisite for any further advance in basic physical theory. For, if we cannot separate the subjective and objective aspects of the formalism, we cannot know what we are talking about; it is just that simple. So we want to speculate on the proper tools to do this.

We suggest that the proper tool for incorporating human information into science is simply probability theory – not the currently taught "random variable" kind, but the original "logical inference" kind of James Bernoulli and Laplace. For historical reasons explained elsewhere (Jaynes, 1986), this is often called "Bayesian inference". When supplemented by the notion of information entropy, this becomes a mathematical tool for scientific reasoning of such power and versatility that we think it will require Centuries to explore all its capabilities. But the preliminary development of this tool and testing it on simple problems is now fairly well in hand, as described below.

A job for the immediate future is to see whether, by proper choice of variables, the QM omelette can be seen as a kind of approximation to it. In the 1950's, Richard Feynman noted that some of the probabilities in quantum theory obey different rules (interference of path amplitudes) than do the classical probabilities. But more recently Jaynes (1989) we have found that the QM probabilities involved in the EPR scenario are striking similar to the Bayesian probabilities, often identical; and we interpret Bohr's reply to EPR as a recognition of this. That is, if we read it very carefully and sympathetically, Bohr's explanation of the EPR experiment is seen as a fairly good (albeit awkwardly phrased) statement of Bayesian inference.

Furthermore, we know that toward the end of his life, Niels Bohr showed an active interest in Information Theory. Therefore the omelette does have some discernible structure of the kind that we would need in order to unscramble it. I do not think this problem is unsolvable in principle – in fact, one can now see vaguely how it is going to work out – only nobody has yet seen how to handle the specific details.

PROBABILITY THEORY AS THE LOGIC OF SCIENCE

Let us note first an older and simpler field in which basically the same questions and the same arguments are rampant. For some 200 years a debate has been underway on the philosophical level, over this issue: Is probability theory a "physical" theory of phenomena governed by "chance" or "randomness"; or is it an extension of logic, showing how to reason in situations of incomplete information? For two generations the former view has dominated science almost completely.

More specifically, the basic equations of probability theory are the product and sum rules: denoting by AB the proposition: "A and B are both true"; and by $\overline{A}$ the proposition "A is false", these are

$$P(AB|C) = P(A|BC)P(B|C) = P(B|AC)P(A|C) \tag{2a}$$

$$P(A|B) + P(\overline{A}|B) = 1\ , \tag{2b}$$

and the issue is: What do these equations mean? Are they rules for calculating frequencies of "random variables", or rules for conducting plausible inference (reasoning from incomplete information)? Does the conditional probability symbol $P(A|B)$ stand for the frequency with which A is true in some "random experiment" defined by B; or for the degree of plausibility, in a single instance, that A is true, given that B is true? Do probabilities describe real properties of Nature; or only human information about Nature?

The original view of James Bernoulli and Laplace was that probability theory is an extension of logic to the case where, because of incomplete information, deductive reasoning by the Aristotelian syllogisms is not available. It was sometimes called "The calculus of inductive reasoning." Laplace's great contributions to science were made with the help of probability theory interpreted in this way.

But, starting in the mid–Nineteenth Century, Laplace's viewpoint came under attack from Leslie Ellis, John Venn, George Boole, R. von Mises, R. A. Fisher, M. G. Kendall, W. Feller, J. Neyman, and others down to our own time. Their objection was always to his philosophy; none of these critics was able to show that Laplace's methods [application of Eqs (2a), (2b) as a form of extended logic] contained any inconsistency or led to any unsatisfactory results. Whenever they seemed to find such a case, closer examination always showed that they had only misunderstood and misapplied Laplace's methods.

Nevertheless, this school of thought was so aggressive that it has dominated the field almost totally in this Century, so that virtually all probability textbooks in current use are written from a viewpoint which rejects Laplace's interpretations and tries to deny us the use of his methods, ignoring the success he had with them. Almost the only exceptions are found in the works of

Harold Jeffreys (1939), Arnold Zellner (1971), and G. L. Bretthorst (1988), which recognize the merit of Laplace's viewpoint and apply it with the same kind of good results that Laplace found, in more sophisticated current problems. We have written two short histories of these matters (Jaynes, 1978, 1986), engaged in a polemical debate on them (Jaynes, 1976), and are trying to finish a two volume treatise on the subject, entitled "Probability Theory – The Logic of Science" in which we demonstrate that these methods are determined uniquely by some very elementary – and nearly inescapable – qualitative desiderata of consistency and rationality in plausible reasoning. Its validity is a matter of logic, independent of all physical hypotheses. Today, in our view, nothing in any physical science has anywhere near the fundamental theoretical justification that Laplace's methods have.

But their basis is not only theoretical; we have today a mass of demonstrated pragmatic results obtained from using them, fully confirming what the theory indicates. Yet denunciations of the "subjectivity" of Laplace, Jeffreys, and the writer for venturing to use probability to represent human information; and even more of the "subjectivity" of entropy based on such probabilities, often reach hysterical proportions. It is very hard to understand why so much emotional fervor should be aroused by these questions, since the means to resolve them (the theoretical basis and pragmatic results of both approaches) are already in the literature and available to all. Those who engage in these attacks are only making a public display of their own ignorance of recent work in this field.

But the failure of our critics to find inconsistencies or errors does not in itself prove that our methods have any positive value for science. Are there any new useful results to be had from using probability theory as logic? Some are reported in the proceedings volumes of the Annual (since 1981) MAXENT workshops; particularly the one in Cambridge, England in August 1988, wherein our present understanding of entropy leads to a generalized Second Law of Thermodynamics, applied in what we think is the first quantitative application of the second law in biology.* But unfortunately, most of the problems solvable by pencil–and–paper methods were too trivial to put this issue to a real test; although the results never conflicted with common sense, neither did they extend it very far beyond what common sense could see, or what "random variable" probability theory could also derive.

Only recently, thanks to the computer, has it become feasible to solve real, nontrivial problems of reasoning from incomplete information, in which we use probability theory as a form of logic in situations where both intuition and "random variable" probability theory would be helpless. This has brought out the facts in a way that can no longer be obscured by arguments over philosophy. One can always argue with a philosophy; it is not so easy to argue with a computer printout, which says to us: "Independently of all your philosophy, here are the facts about what this method actually gives when applied."

The "MEM" program developed by John Skilling, Steve Gull, and their colleagues at Cambridge University, England can maximize entropy numerically in a space of 1,000,000 dimensions, subject to 2,000 simultaneous constraints. The "Bayesian" data analysis programs developed by G. L. Bretthorst (1988) at Washington University, St. Louis, can eliminate a hundred uninteresting parameters and give the simultaneous best estimates of twenty interesting ones and their accuracy; or it can take into account all the parameters in a set of possible theories or "models", and give us the relative probabilities of the theories in the light of the data. It was interesting, although to us not surprising, to find that this leads automatically to an improved, quantitative version of Occam's Razor: prefer the simpler and/or more plausible theory unless the other gives a significantly better fit to the data.

* This showed that the observed efficiency of muscles, long thought by some to be violations of the second law, are on the contrary derivable *from* the second law once it is properly understood.

Many computer printouts have now been made at Cambridge University, of image reconstructions in optics and radio astronomy; and at Washington University in analysis of economic, geophysical, and nuclear magnetic resonance data. The results were astonishing to all of us; they could never have been found, or guessed, by hand methods.

In particular, the Bretthorst programs extract far more information from NMR data (where the ideal sinusoidal signals are corrupted by decay) than could the previously used fourier transform methods. No longer does decay broaden the spectrum and obscure the information about oscillation frequencies; the result can be orders of magnitude better resolution.

Less spectacular numerically, but equally important in principle, they yield fundamental improvements in extracting information from economic time series when the data are corrupted by trend and seasonality; no longer do these obscure the information that we are trying to extract from the data. Conventional "random variable" probability theory lacks the technical means to eliminate nuisance parameters in this way.

In other words, there is no need to shout: it is now a very well demonstrated fact that, after all criticisms of its underlying philosophy, probability theory interpreted and used as the logic of human inference does rather well in dealing with problems of scientific reasoning – just as James Bernoulli and Laplace thought it would, back in the 18'th Century.

Our probabilities and the entropies based on them are indeed "subjective" in the sense that they represent human information; if they did not, they could not serve their purpose. But they are completely "objective" in the sense that they are determined *by the information specified*, independently of anybody's personality, opinions, or hopes. It is "objectivity" in this sense that we need if information is ever to be a sound basis for new theoretical developments in science.

HOW WOULD QUANTUM THEORY BE DIFFERENT?

The aforementioned successful applications of probability theory as logic were concerned with data processing, while the original maximum entropy applications were in statistical mechanics, where they reproduced in a few lines, and then generalized, the results of Gibbs. In these applications, probability theory represented the process of reasoning from incomplete information. There is no claim that its predictions must be "right"; only that they are the best that can be made from the information we have. That is, after all, the most that any science could ever have pretended to do; yet some complain bitterly when cherished illusions of "objectivity" are replaced by facts.

We would like to see quantum theory in a similar way; since a pure state ψ does not contain enough information to predict all experimental results, we would like to see QM as the process of making the best predictions possible from the partial information that we have when we know ψ. If we could either succeed in this, or prove that it is impossible, we would know far more about the basis of our present theory and about future possibilities than we do today.

Einstein wanted to do something very similar; but he offered only criticisms rather than constructive suggestions. What undoubtedly deterred both Einstein and Schrödinger is this: one sees quickly that the situation is more subtle than merely keeping the old mathematics and reinterpreting it. That is, we cannot merely proclaim that all the probabilities calculated within a QM pure state ψ according to the standard rules of our textbooks are now to be interpreted as expressions of human ignorance of the true physical state. The results depend on the representation in a way that makes this naïve approach impossible.

For example, if we expand ψ in the energy representation: $\psi(x,t) = \sum a_n(t)u_n(x)$, the physical situation cannot be described merely as "the system may be in state $u_1(x)$ with probability $p_1 = |a_1|^2$; or it may be in state $u_2(x)$ with probability $p_2 = |a_2|^2$, and we do not know which of these is the true state". This would suffice to give, using classical probability theory, the QM predictions of quantities that are diagonal in the $\{u_n\}$ representation; but the relative phases of the amplitudes a_n have a definite physical meaning that would be lost by that approach.

Even though they have no effect on probabilities p_n in the energy representation, these phases will have a large effect on probabilities in some other representation. They affect the predicted values of quantities that are not diagonal in the $\{u_n\}$ representation, in a way that is necessary for agreement with experiment. For example, the relative phases of degenerate energy states of an atom determine the polarization of its resonance radiation, which is an experimental fact; so there has to be something physically real in them.†

In other words, we cannot say merely that the atom is "in" state u_1 or "in" state u_2 as if they were mutually exclusive possibilities and it is only we who are ignorant of which is the true one; in some sense it must be in both simultaneously, or as Pauli would say, the atom itself does not know what energy state it is in. This is the conceptually disturbing, but experimentally required, function of the superposition principle.

We conjecture that this is the circumstance that also deterred Niels Bohr from making ontological statememts, and forced him to use such cautious language. He would never say (as some of his unperceptive disciples did) that $|a_n|^2$ is the probability that an atom is "in" the n'th state, which would be an unjustified ontological statement; rather, he would say that $|a_n|^2$ is the probability that, if we measure its energy, we shall find the value corresponding to the n'th state.

But notice that there is nothing conceptually disturbing in the statement that a vibrating bell is in a linear combination of two vibration modes with a definite relative phase; we just interpret the mode (amplitudes)^2 as *energies*, not *probabilities*. So it is the way we look at quantum theory, trying to interpret $|\psi|^2$ directly as a probability density, that is causing the difficulty.

If this seems at first to be an obstacle to our purpose, it is also our real opportunity, because it shows that the probabilities we seek, which are to express the incompleteness of the information in a pure state in terms of a set of mutually exclusive possibilities (call it an "ensemble" if you like), cannot be the usual things called "probability" in the QM textbooks. The human information must be represented in a deeper "hypothesis space" which contains the phases as well as the amplitudes.

To realize this is to throw off a whole legacy of supposed difficulties from the past; the nonclassical behavior of QM probabilities pointed out by Feynman ceases to bother us because the quantities exhibiting that behavior will not be interpreted as probabilities in the new hypothesis space. Likewise, the Bell inequality arguments are seen to have very little relevance to our problem; for he was hung up on the difficulty of getting the standard QM probabilities out of a causal theory. But if they are not the basic probabilities after all, the failure of a causal theory to reproduce them *as* probabilities might seem rather a merit than a defect. So the clouds begin to lift, just a bit.

This is not an auspicious time to be making public announcements of startling, revolutionary new scientific discoveries; so it is rather a relief that we have none to announce. To exhibit the variables of that deeper hypothesis space explicitly is a job for the future; but in the meantime we can do a little job of housecleaning that is in any event a prerequisite for it. We cannot hope to get our probability connections right until we get some basic points of logic right.

The first difficulty we encounter upon any suggestion that probabilities in quantum theory might represent human information, is the barrage of criticism from those who believe that dispersions $(\Delta F)^2 \equiv \langle F^2 \rangle - \langle F \rangle^2$ represent experimentally observable "quantum fluctuations" in F. Some who pose as disciples of Bohr even claim that these fluctuations are real physical events that take place constantly whether or not any measurement is being made (although of course that does

† When these phases are taken into account, the QM formalism has the physically necessary property that the polarization follows that of the exciting radiation, independently of the direction of our axis of quantization.

violence to Bohr's position, as we have just seen). For example, at the 1966 Rochester Coherence Conference, Roy Glauber assured us that vacuum fluctuations are "very real things" and that any attempts to dispense with EM field quantization are therefore doomed to failure. It can be reported that he was widely and enthusiastically believed.

Now in basic probability theory, ΔF represents fundamentally the accuracy with which we are able to *predict* the value of F. This does not deny that it may be also the variability seen in repeated measurements of F; but the point is that they need not be the same. To suppose that they *must* be the same is to commit an egregious form of the Mind Projection Fallacy; the fact that our information is able to determine F only to 5 percent accuracy, is not enough to make it fluctuate by 5 percent! To predict observable fluctuations by a correct application of probability theory requires an entirely different calculation (Jaynes, 1978, 1983). However, it is almost right to say that, given such information, any observed fluctuations are unlikely to be greater than 5 percent.

Let us analyze in some depth the single example of EM field fluctuations, and show that (1) the experimental facts do not require vacuum fluctuations to be real events after all; (2) Bayesian probability at this point is not only consistent with the experimental facts, it offers us some striking advantages in clearing up past difficulties that have worried generations of physicists.

IS ZERO–POINT ENERGY REAL?

For many years we have had a strange situation; on the one hand, "Official" QED has never taken the infinite ZP energy problem seriously, apparently considering it only a formal detail like the infinite charge density in the original hole theory, which went away when the charge symmetry of the theory was made manifest in Schwinger's action principle formulation.

But the ZP problem has not gone away; and on the other hand as we have noted, there is a widespread belief that ZP fluctuations are real and necessary to account for all kinds of things, such as spontaneous emission, the Lamb shift, and the Casimir attraction effect. Steven Weinberg (1989) accepted the Casimir effect as demonstrating the reality of ZP energy, and worried about it in connection with cosmology. We know that Pauli also worried about this and did some calculations, but apparently never published them.

If one takes the ZP energy literally, one of the disturbing consequences is the gravitational field it would produce. For example, if there is a ZP energy density W_{zp} in space, the Kepler ratio for a planet of mean distance R from the sun would be changed to

$$\frac{R^3}{T^2} = \frac{G}{4\pi^2}\left[M_{sun} + \frac{4\pi R^3}{3c^2}\, W_{zp}\right] . \tag{3}$$

Numerical analysis of this shows that, in order to avoid conflict with the observed Kepler ratios of the outer planets, the upper frequency cutoff for the ZP energy would have to be taken no higher than optical frequencies.

But attempts to account for the Lamb shift by ZP fluctuations would require a cutoff thousands of times higher, at the Compton wavelength. The gravitational field from that energy density would not just perturb the Kepler ratio; it would completely disrupt the solar system as we know it.

The difficulty would disappear if one could show that the aforementioned effects have a different cause, and ZP field energy is not needed to account for any experimental facts. Let us try first with the simplest effect, spontaneous emission. The hypothesized zero–point energy density in a frequency band $\Delta\omega$ is

$$W_{zp} = \rho_{zp}(\omega)\Delta\omega = \left(\frac{1}{2}\hbar\omega\right)\left(\frac{\omega^2\Delta\omega}{\pi^2 c^3}\right) \text{ergs/cm}^3 \tag{4}$$

Then an atom decaying at a rate determined by the Einstein A–coefficient

$$A = \frac{4\mu^2\omega_o^3}{3\hbar c^3} \tag{5}$$

where μ is the dipole moment matrix element for the transition, sees this over an effective bandwidth

$$\Delta\omega = \frac{\int I(\omega)d\omega}{I(\omega_0)} = \frac{\pi A}{2} \tag{6}$$

where $I(\omega)$ is the Lorentzian spectral density

$$I(\omega) \propto \frac{1}{(\omega - \omega_0)^2 + (A/2)^2} \ . \tag{7}$$

The effective energy density in one field component, say E_z, is then

$$(W_{zp})_{eff} = \frac{1}{6}\,\rho_{zp}(\omega)\,\Delta\omega = \frac{1}{18\pi}\mu^2\left(\frac{\omega}{c}\right)^6 \text{ ergs/cm}^3 \tag{8}$$

and it seems curious that Planck's constant has cancelled out. This indicates the magnitude of the electric field that a radiating atom sees according to the ZP theory.

On the other hand, the classical radiation reaction field generated by a dipole of moment μ:

$$E_{RR} = \frac{2}{3c^3}\frac{d^3\mu}{dt^3} = \frac{2\omega^3}{3c^3}\mu \tag{9}$$

has energy density

$$W_{RR} = \frac{E_{RR}^2}{8\pi} = \frac{1}{18\pi}\mu^2\left(\frac{\omega}{c}\right)^6 \text{ ergs/cm}^3 \ . \tag{10}$$

But (8) and (10) are identical! A radiating atom is indeed interacting with an electric field of just the magnitude predicted by the zero–point calculation; but this is the atom's own radiation reaction field.

Now we can see that this needed field is generated by the radiating atom, automatically but in a more economical way; only where it is needed, when it is needed, and in the frequency band needed. Spontaneous emission does not require an infinite energy density throughout all space. Surely, this is a potentially far more satisfactory way of looking at the mechanism of spontaneous emission (if we can clear up some details about the dynamics of the process).

But then someone will point immediately to the Lamb shift; does this not prove the reality of the ZP energy? Indeed, Schwinger (1948) and Weisskopf (1949) stated explicitly that ZP field fluctuations are the physical cause of the Lamb shift, and Welton (1948) gave an elementary "classical" derivation of the effect from this premise.

Even Niels Bohr concurred. To the best of our knowledge, the closest he ever came to making an ontological statement was uttered while perhaps thrown momentarily off guard under the influence of Schwinger's famous 8–hour lecture at the 1948 Pocono conference. As recorded in John Wheeler's notes on that meeting, Bohr says: "It was a mistake in the older days to be discontented with field and charge fluctuations. They are necessary for the physical interpretation."

Dyson (1953) also concurred, picturing the quantized field as something akin to hydrodynamic flow with superposed random turbulence, and he wrote: "The Lamb–Retherford experiment is the strongest evidence we have for believing that our picture of the quantum field is correct in detail." Then in 1961 Feynman suggested that it should be possible to calculate the Lamb shift from the change in total ZP energy in space due to the presence of a hydrogen atom in the 2s state; and in 1966 E. A. Power gave the calculation demonstrating this in detail. How can we possibly resist such a weight of authority and factual evidence?

As it turns out, quite easily. The problem has been that these calculations have been done heretofore only in a quantum field theory context. Because of this, people jumped to the conclusion that they were quantum effects (*i.e.* effects of field quantization), without taking the trouble to check whether they were present also in classical theory. As a result, two generations of physicists have regarded the Lamb shift as a deep, mysterious quantum effect that ordinary people cannot hope to understand. So we are facing not so much a weight of authority and facts as a mass of accumulated folklore.

Since our aim now is only to explain the elementary physics of the situation rather than to give a full formal calculation, let us show that this radiative frequency shift effect was present already in classical theory, and that its cause lies simply in properties of the source field (1), having nothing to do with field fluctuations. In fact, by stating the problem in Hamiltonian form we can solve it without committing ourselves to electromagnetic or acoustical fields. Thus the vibrations of a plucked guitar string are also damped and shifted by their coupling to the acoustical radiation field, according to the following equations.

THE LAMB SHIFT IN CLASSICAL MECHANICS

Let there be n 'field oscillators' with coordinates and momenta $\{q_i(t), p_i(t)\}$, and one 'Extra Oscillator' $\{Q(t), P(t)\}$, a caricature of a decaying atom or plucked string; call it "the EO". It is coupled linearly to the field oscillators with coupling constants $\{\alpha_i\}$, leading to a total Hamiltonian

$$H = \frac{1}{2}\sum_{i=1}^{n}(p_i^2 + \omega_i^2 q_i^2) + \frac{1}{2}(P^2 + \Omega^2 Q^2) - \sum_i \alpha_i q_i Q \ . \tag{11}$$

The physical effects of coupling the EO to the field variables may be calculated in two 'complementary' ways;

(I) **Dynamic:** How are the EO oscillations modified by the field coupling?

(II) **Static:** How is the distribution of normal mode frequencies changed?

The new normal mode frequencies are the roots $\{\nu_i\}$ of the equation $\Omega^2 - \nu^2 = K(\nu)$, where $K(\nu)$ is the dispersion function

$$K(\nu) \equiv \sum_i \frac{\alpha_i^2}{\omega_i^2 - \nu^2} = \int_0^\infty K(t) e^{-st} dt, \qquad s = i\nu \ . \tag{12}$$

Let us solve the problem first in the more familiar dynamical way. With initially quiescent field modes: $q_i(0) = \dot{q}_i(0) = 0$, the decay of the extra oscillator is found to obey a Volterra equation:

$$\ddot{Q}(t) + \Omega^2 Q(t) = \int_0^t K(t - t')\, Q(t') dt' \ . \tag{13}$$

Thus $K(t)$ is a memory function and the integral in (13) is a source field. For arbitrary initial EO conditions $Q(0)$, $\dot{Q}(0)$ the solution is

$$Q(t) = Q(0)\dot{G}(t) + \dot{Q}(0) G(t) \tag{14}$$

with the Green's function

$$G(t) = \frac{1}{2\pi}\int_{-\infty}^{\infty} \frac{e^{i\nu t} d\nu}{\Omega^2 - \nu^2 - K(\nu)} \tag{15}$$

where the contour goes under the poles on the real axis. This is the exact decay solution for arbitrary field mode patterns.

In the limit of infinitely many field modes, the solution goes into a simpler form. There is a mode density function $\rho_0(\omega)$:

$$\sum_i \Big(\quad\Big) \to \int_0^\infty \Big(\quad\Big)\, \rho_0(\omega)\, d\omega$$

Then from (12), $K(\nu)$ goes into a slowly varying function on the path of integration (15):

$$K(\nu - i\epsilon) \to \int_0^\infty \frac{\alpha^2(\omega)\,\rho_0(\omega)\,d\omega}{\omega^2 - (\nu - i\epsilon)^2} \to -2\nu\,[\Delta(\nu) + i\Gamma(\nu)] \tag{16}$$

and neglecting some small terms, the resulting Green's function goes into

$$G(t) \to \exp(-\Gamma t)\,\frac{\sin(\Omega+\Delta)t}{(\Omega+\Delta)} \tag{17}$$

where

$$\Gamma(\Omega) \equiv \frac{\pi\alpha^2(\Omega)\rho_0(\Omega)}{4\Omega^2} \tag{18}$$

$$\Delta(\Omega) \equiv \frac{1}{2\Omega} P \int_0^\infty \frac{\alpha^2(\omega)\rho_0(\omega)d\omega}{\Omega^2 - \omega^2} = \frac{1}{\pi} P \int_{-\infty}^\infty \frac{\Gamma(\omega)d\omega}{\Omega - \omega} \tag{19}$$

are the "spontaneous emission rate" and "radiative frequency shift" exhibited by the EO due to its coupling to the field modes. We note that $\Delta(\Omega)$ and $\Gamma(\omega)$ form a Hilbert transform pair, a Kramers–Kronig type dispersion relation usually considered as expressing causality.† In this approximation, the general solution (14) becomes the exponentially damped solution of a linear differential equation with loss: $\ddot{Q} + 2\Gamma\dot{Q} + (\Omega+\Delta)^2\,Q = 0$.

As a check, it is a simple homework problem to compare our damping factor Γ with the well–known Larmor radiation law, by inserting into the above formulas the free–space mode density function $\rho_0(\omega) = V\omega^2/\pi^2c^3$, and the coupling coefficients α_i appropriate to an electric dipole of moment μ proportional to Q. We then find

$$\Gamma(\omega) = \Big(\frac{\pi}{4\omega^2}\Big)\cdot\Big(\frac{\mu^2}{Q^2}\cdot\frac{4\pi\omega^2}{3V}\Big)\cdot\Big(\frac{V\omega^2}{\pi^2c^3}\Big) = \frac{\mu^2\omega^2}{3Q^2c^3}\ \ \text{sec}^{-1} \tag{20}$$

and it is easily seen that for the average energy loss over a cycle this agrees exactly with the Larmor formula

$$P_{rad} = \frac{2e^2}{3c^3}(\ddot{x})^2 \tag{21}$$

for radiation from an accelerated particle. In turn, the correspondence between the Larmor radiation rate and the Einstein A–coefficient (5) is well–known textbook material.

It is clear from this derivation that the spontaneous emission and the radiative frequency shift do not require field fluctuations, since we started with the explicit initial condition of a quiescent

† That interpretation is correct for the present classical dynamical problem, in which $K(t-t')$ clearly represents a physical causal influence. But in QED the term 'causal' has become so perverted in meaning that the violently anticausal Feynman propagator, with response outside the light cone and running backward in time, is called a 'causal' propagator! In effect, this means that the term 'causal' has become unusable.

field: $q_i = \dot{q}_i = 0$. The damping and shifting exhibited above *are due entirely to the source field reacting back on the source*, as expressed by the integral in (13).

Of course, although the frequency shift formula (19) resembles the "Bethe logarithm" expression for the Lamb shift, we cannot compare them directly because our model is not a hydrogen atom; we have no s–states and p–states. But if we use values of α_i and Ω for an electron oscillating at optical frequencies and a cutoff corresponding to the size of the hydrogen atom (that is, $\omega_{max} \simeq c/a_0$, where a_0 is the Bohr radius), we get shifts of the order of magnitude of the Lamb shift. A more elaborate calculation will be reported elsewhere.

But now this seems to raise another mystery; if field fluctuations are not the cause of the Lamb shift, then why did the aforementioned Welton and Power calculations succeed by invoking those fluctuations? We face here a very deep question about the meaning of "fluctuation–dissipation theorems". There is a curious mathematical isomorphism; throughout this Century, starting with Einstein's relation between diffusion coefficient and mobility $D = \delta x^2/2t = kT\mu$ and the Nyquist thermal noise formula for a resistor $\delta V^2 = 4kTR\Delta f$, theoreticians have been deriving a steady stream of relations connecting "stochastic" problems with dynamical problems.

Indeed, for every differential equation with a non–negative Green's function, there is an obvious stochastic problem which would have the same mathematical solution even though the problems are quite unrelated physically; but as Mark Kac (1956) showed, the mathematical correspondence between stochastic and dynamical problems is much deeper and more general than that.

Now to get our logic straight: these relations do not prove that the fluctuations are real; they show only that certain dissipative effects (*i.e.* disappearance of the extra oscillator energy into the field modes) are the same *as if* fluctuations were present. But then by the Hilbert transform connection noted, the corresponding reactive effects must also be the same as if fluctuations were present; the calculation of Welton (1948) shows how this comes about.

But this still leaves a mystery surrounding the Feynman–Power calculation, which obtains the Lamb shift from the change in total ZP energy in the space surrounding the hydrogen atom; let us explain how that can be.

CLASSICAL SUBTRACTION PHYSICS

Consider now the second, static method of calculating the effect of field coupling. One of the effects of the EO is to change the distribution of normal modes; the above "free space" mode density $\rho_0(\omega)$ is incremented to

$$\rho(\omega) = \rho_0(\omega) + \rho_1(\omega) \; . \tag{22}$$

To calculate the mode density increment, we need to evaluate the limiting form of the dispersion function $K(\nu)$ more carefully than in (17).

From the Hamiltonian (11), the normal modes are the roots $\{\nu_i\}$ of the dispersion equation

$$\Omega^2 - \nu^2 = K(\nu) = \sum_i \frac{\alpha_i^2}{\omega_i^2 - \nu^2} \; . \tag{23}$$

$K(\nu)$ resembles a tangent function, having poles at the free field mode frequencies $\{\omega_i\}$ and zeroes close to midway between them. Suppose that the unperturbed frequency Ω of the EO lies in the cell $(\omega_i < \Omega < \omega_{i+1})$. Then the field modes above Ω are raised by amounts $\delta\nu_k = \nu_k - \omega_k$, $k = i+1, i+2, \cdots n$. The field modes below Ω are lowered by $\delta\nu_k = \nu_{k-1} - \omega_k$, $k = 1, 2, \cdots i$; and one new normal mode ν_i appears in the same cell as Ω: $(\omega_i < \nu_i < \omega_{i+1})$. The separation property (exactly one new mode ν_k lies between any two adjacent old modes ω_i) places a stringent limitation on the magnitude of any static mode shift $\delta\nu_k$.

Thus the original field modes $\{\omega_i\}$ are, so to speak, pushed aside by a kind of repulsion from the added frequency Ω, and one new mode is inserted into the gap thus created. If there are many field modes, the result is a slight increase $\rho_1(\nu)$ in mode density in the vicinity of Ω. To calculate it, note that if the field mode ω_i is shifted a very small amount to $\nu_k = \omega_i + \delta\nu$, and $\delta\nu$ varies with ω_i, then the mode density is changed to

$$\rho(\omega) = \rho_0(\omega) + \rho_1(\omega) = \rho_0(\omega)\Big[1 - \frac{d}{d\omega}(\delta\nu) + \cdots\Big] . \tag{24}$$

In the continuum limit, $\rho_o \to \infty$ and $\delta\nu \to 0$; but the increment $\rho_1(\omega)$ remains finite and as we shall see, loaded with physical meaning.

We now approximate the dispersion function $K(\nu)$ more carefully. In (15) where $\mathrm{Im}(\nu) < 0$, we could approximate it merely by the integral, since the local behavior (the infinitely fine–grained variation in $K(\nu)$ from one pole to the next) cancels out in the limit at any finite distance from the real axis. But now we need it exactly *on* the real axis, and those fine–grained local variations are essential, because they provide the separation property that limits the static mode shifts $\delta\nu$.

Consider the case where $\omega_i > \Omega$ and ν lies in the cell $(\omega_i < \nu < \omega_{i+1})$. Then the modes are pushed up. If the old modes near ν are about uniformly spaced, we have for small n, $\omega_{i+n} \simeq \omega_i + n/\rho_0(\omega)$, therefore

$$\omega_{i+n}^2 - \nu^2 \simeq \frac{2\nu}{\rho_0}(n - \rho_0\delta\nu) , \tag{25}$$

and the sum of terms with poles near ν goes into

$$\sum_n \frac{\alpha_{i+n}^2 \rho_0(\nu)}{2\nu(n - \rho_0\delta\nu)} \simeq -\frac{\pi\alpha^2(\nu)\rho_0(\nu)}{2\nu} \cot[\pi\rho_0(\nu)\delta\nu] \tag{26}$$

where we supposed the α_i slowly varying and recognized the Mittag–Leffler expansion $\pi\cot\pi x = \Sigma(x-n)^{-1}$. The contribution of poles far from ν can again be represented by an integral. Thus on the real axis, the dispersion function goes, in the continuum limit, into

$$K(\nu) \simeq -\frac{\pi\alpha^2\rho_0}{2\nu} \cot[\pi\rho_0(\nu)\delta\nu] + P\int_0^\infty \frac{\alpha^2(\omega)\rho_0(\omega)d\omega}{\omega^2 - \nu^2} .$$

But in this we recognize our expressions (18), (19) for Γ and Δ:

$$K(\nu) \simeq -2\Omega\,[\Delta + \Gamma\cot(\pi\rho_0\delta\nu)] . \tag{27}$$

As a check, note that if we continue $\delta\nu$ below the real axis, the cotangent goes into $\cot(-ix) \to +i$, and we recover the previous result (16). Thus if we again assume a sharp resonance $(\Omega \simeq \nu)$ and write the dynamically shifted frequency as $\omega_0 = \Omega + \Delta$, the dispersion relation (23) becomes a formula for the static mode shift $\delta\nu$:

$$\pi\rho_0(\nu)\delta\nu = \tan^{-1}\left(\frac{\Gamma}{\nu - \omega_0}\right) \tag{28}$$

and (24) then yields for the increment in mode density a Lorentzian function:

$$\rho_1(\nu)d\nu = \frac{1}{\pi}\,\frac{\Gamma d\nu}{(\nu-\omega_0)^2 + \Gamma^2} . \tag{29}$$

This is the spectrum of a damped oscillation:

$$\int_{-\infty}^{\infty} \rho_1(\nu) e^{i\nu t} d\nu = e^{i\omega_0 t} e^{-\Gamma|t|} \ , \tag{30}$$

with the same shift and width as we found in the dynamical calculation (13).

As a check, note that the increment is normalized, $\int \rho_1 d\nu = 1$ as it should be, since the "macroscopic" effect of the coupled EO is just to add more new mode to the system. Note also that the result (29) depended on $K(\nu)$ going locally into a tangent function. If for any reason (*i.e.* highly nonuniform mode spacing or coupling constants, even in the limit) $K(\nu)$ does not go into a tangent function, we will not get a Lorentzian $\rho_1(\nu)$. This would signify perturbing objects in the field, or cavity walls that do not recede to infinity in the limit, so echoes from them remain.

But the connection (30) between the mode density increment and the decay law is quite general. It does not depend on the Lorentzian form of $\rho_1(\nu)$, on the particular equation of motion for Q, on whether we have one or many resonances Ω, or indeed on any property of the perturbing EO other than the linearity of its response.

To see this, imagine that all normal modes are shock excited simultaneously with arbitrary amplitudes $A(\nu)$. Then the response is a superposition of all modes:

$$\int A(\nu)[\rho_0(\nu) + \rho_1(\nu)] e^{i\nu t} d\nu \ . \tag{31}$$

But since the first integral represents the response of the free field, the second must represent the "ringing" of whatever perturbing objects are present. If $A(\nu)$ is nearly constant in the small bandwidth occupied by a narrow peak in $\rho_1(\nu)$, the resonant ringing goes into the form (30).

Therefore, every detail of the transient decay of the dynamical problem is, so to speak, "frozen into" the static mode density increment function $\rho_1(\nu)$ and can be extracted by taking the Fourier transform (30). Thus a bell, excited by a pulse of sound, will ring out at each of its resonant frequencies, each separate resonance having a decay rate and radiative frequency shift determined by $\rho_1(\nu)$ in the vicinity of that resonance.

Then a hydrogen atom in the 2s state, excited by a sharp electromagnetic pulse, will "ring out" at the frequencies of all the absorption or emission lines that start from the 2s state, and information about all the rates of decay and all the radiative line shifts, is contained in the $\rho_1(\nu)$ perturbation that the presence of that atom makes in the field mode density.

Thus Feynman's conjecture about the relation between the Lamb shift and the change in ZP energy of the field around that atom, is now seen to correspond to a perfectly general relation that was present all the time in classical electromagnetic and acoustical theory, and might easily have been found by Rayleigh, Helmholtz, Maxwell, Larmor, Lorentz, or Poincaré in the last Century.

It remains to finish the Power–type calculation and show that, if one wishes to do so, simple classical calculations can also be done by the more glamorous quantum mechanical methods that Pauli dismissed as "subtraction physics". Suppose we put the extra oscillator in place and then turn on its coupling to the field oscillators. Before the coupling is turned on we have a background mode density $\rho_0(\omega)$ with a single sharp resonance, mode density $\delta(\omega\Omega)$ superimposed. Turning on the coupling spreads this out into $\rho_1(\omega)$, superimposed on the same background, and shifts its center frequency by just the radiative shift Δ. In view of the normalization of $\rho_1(\omega)$ we can write

$$\Delta = \int_o^{\infty} \omega \rho_1(\omega) d\omega - \Omega \ . \tag{32}$$

Suppose, then, that we had asked a different question: "What is the total frequency shift *in all modes*, due to the coupling?" Before the coupling is turned on, the sum of all mode frequencies is a badly divergent expression:

$$(\infty)_1 = \Omega + \int_0^\infty \omega\,\rho_0(\omega)\,d\omega \tag{33}$$

and afterward it is

$$(\infty)_2 = \int_0^\infty \omega\,[\rho_0(\omega) + \rho_1(\omega)]d\omega \tag{34}$$

which is no better. But then the total change in all mode frequencies due to the coupling is, from (32):

$$(\infty)_2 - (\infty)_1 = \Delta\ . \tag{35}$$

To do our physics by subtraction of infinities is an awkward way of asking the line shift question; but it leads to the same result. There is no longer much mystery about why Power could calculate the radiative shift in the dynamical problem by the change in total ZP energy; actually, he calculated the change in total *frequency* of all modes, which was equal to the dynamical shift even in classical mechanics.

But some will still hold out and point to the Casimir attraction effect, where one measures a definite *force* which is held to arise from the change in total ZP energy when one changes the separation of two parallel metal plates. How could we account for this if the ZP energy is not real? This problem is already discussed in the literature; Schwinger, de Raad, and Milton (1978) derive it from Schwinger's source theory, in which there are no operator fields. One sees the effect, like the van der Waals attraction, as arising from correlations in the state of electrons in the two plates, through the intermediary of their source fields (1). It does not require ZP energy to reside throughout all space, any more than does the van der Waals force. Thus we need not worry about the effect of ZP energy on the Kepler ratio (3) or the cosmological constant, after all.

CONCLUSION

We have explored only a small part of the issues that we have raised; but it is the part that has seemed the greatest obstacle to a unified treatment of probability in quantum theory. Its resolution was just a matter of getting our logic straight; we have been fooled by a subtle mathematical correspondence between stochastic and dynamical phenomena, into a belief that the "objective reality" of vacuum fluctuations and ZP energy are experimental facts. With the realization that this is not the case, many puzzling difficulties disappear.

We then see the possibility of a future quantum theory in which the role of incomplete information is recognized: the dispersion $(\Delta F)^2 = \langle F^2\rangle - \langle F\rangle^2$ represents fundamentally only the accuracy with which the theory is able to *predict* the value of F. This may or may not be also the variability in the *measured* values.

In particular, when we free ourselves from the delusion that probabilities are physically real things, then when ΔF is infinite, that does not mean that any physical quantity is infinite. It means only that the theory is completely unable to predict F; the only thing that is infinite is the uncertainty of the prediction. In our view, this represents the beginning of a far more rational and satisfactory way of looking at quantum theory, in which the important research problems will appear entirely different than they do now.

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