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An Approach to the Realistic Explanation of Quantum Mechanics.

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In the late 1920's and early 1930's, many physicists felt that the phenomenological quality of quantum mechanics was a temporary difficulty, which would soon disappear. Scientists of profound insight, such as EINSTEIN, SCHRÖDINGER and DE BROGLIE felt convinced, even in the 1950's, that physics would have to return someday to the concept of an external world existing in a definite (if unobserved) state and to the concept of observers obeying the same laws as the observed ones. EINSTEIN could accept strongly the philosophical notion of a meaningful God existing, but not the notion that there is no external universe at all, or, even worse, the notion of phenomenology. The sheer complexity of quantum mechanics, in Fock space, was enough to persuade most physicists of the value of trying to «explain» it, a few decades back.

In the meantime, the structure of quantum mechanics has grown more and more complex. We are reminded of medieval astronomy; the direct idea of the Sun and stars revolving about the Earth seemed simple enough, to begin with: phenomenal appearances seemed to be a «natural» basis for astronomy. But, as new facts accumulated, the phenomenological approach grew hopelessly complex, and left us with little capacity, *either* mathematical or intuitive, for coming to grips with new data. Nowadays, as hundreds of unexplained particles show up all across the spectrum, and as the quantum formalism has not found a solid mathematical base in any area but that of electrodynamics, the analogy grows stronger and stronger. The philosophical objections to phenomonology are just as compelling now as in 1930; the physical limitations of quantum mechanics are far clearer than they were in the time of EINSTEIN; therefore physicists should, in principle, be *more* interested now in realistic explanations than ever before. Yet in the United States, the idea of explaining quantum mechanics in realistic terms has become extremely unpopular.

There is one and only one justification for this change in attitudes: the experience of many physicists, *trying* to construct a realistic explanation of quantum mechanics, *unable* to get anywhere; if one begins to believe that a real explanation has a low probability of ever being found, then one logically attributes a low probability that such an explanation would ever be found true. If such an explanation should again seem possible, however, logic would demand that we attribute a high probability of truth to this explanation.

My objective in this article is to show that the failure of physicists to construct a realistic explanation for quantum mechanics lies in a consistent naivete in their assumptions; I intend to describe a new approach to the explanation of quantum mechanics, which could bring us into a new era of realism and vigor.

Let me begin by analyzing the difficulties which beset prior attempts at explanation. In the case of helium, one can deduce the proper spectrum, in quantum mechanics, by solving for the wave function ψ of six variables—the two sets of three co-ordinates for each electron. A classical approach might consider this success as a grand coincidence, and hope to duplicate it by « guessing the right formula »; the success probability of such an approach would seem extremely low! One might argue, like SACHS in the US, that the six-dimensional character of ψ is not essential to the actual prediction; he has proposed a theory of his own, which yields the Hartree-Fock « approximation » as the true spectrum. However, HYLLERAAS [1] has shown, in the case of helium, that the higher-order terms—going beyond the Hartree-Fock approximation—can be calculated, and are necessary to an accurate prediction of the spectrum. (de Broglie's theory would appear to have similar problems, though the question of spectral calculation is glossed over in that theory.) To *explain* this success of a six-dimensional ψ, in realistic terms, we have only two alternatives: i) to treat ψ as a purely statistical function of possible real (particle) states, a statistical function obeying statistical laws analogous to the Fokker-Planck equation (Einstein's view); ii) to treat the six-dimensional space, or $(3n+1)$-dimensional space-time, *as the real world*, and to treat ψ as the one and only field in the world (Everett's approach—a bit doubtful, by Occam's razor.)

However, classical statistical explanations for quantum mechanics have consistently failed to work. Why? At least up to 1960, we can find one thing in common among quantum mechanics, rigorous classical mechanics and observed microscopic phenomena: *time symmetry*. But classical statistical mechanics is totally *asymmetric* with respect to time. The latter is therefore incapable of regenerating any of the former. (Also, a *single* real probability function—not a complex one—is sufficient in classical physics; ψ is fundamentally complex.) Let me give a simple example of this. Consider the case of an accelerated proton, coming near a neutron, turning into a neutron by an « exchange reaction »; it seems clear [2] that the neutron emits a charged pion, absorbed later by the proton. (These reactions are also extremely common.) From a classical point of view, it is easy to understand how a neutron may emit pions from time to time, in random directions. It is easy to understand how the probability of a pion existing somewhere in space-time may depend on the existence of a neutron nearby to « emit » the pion. However, it seems an act of magic that the pion « knows » to leave just at the time when a proton comes nearby.

One is tempted to conjure up elaborate « explanations » for the phenomenon, including mysterious fields to inform the neutron about the coming proton. One can admit that the presence of a pion depends *directly* on a neutron on its own world-line in the past, but one cannot see how its presence could depend *directly* on the presence of a proton in its future. The latter fact is precisely the mirror image of the former fact through time; yet the latter fact is inconsistent with the « common sense » assumptions built into *all past classical statistical models*. It is no wonder, then, that the classical theories have been unable to fit the facts.

Now: how could we hope to construct a rigorous statistical model, *without* falling into time asymmetry?

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(1) E. U. CONDON and G. H. SHORTLEY: *Theory of Atomic Spectra* (Cambridge, 1957).
(2) E. SEGRÈ: *Nuclei and Particles*, Sect. **10**·6 (New York, 1965).

Following PLANCK, we can begin by using a bit of temporary scaffolding, and keeping our minds open. We need to find some way of expressing the idea that past and future somehow reach an equilibrium, each « responding » to the other. To express the idea that the entire space-time continuum can *approach* an equilibrium, we must postulate an extra dimension—hypertime—in which this approach is to be made. If we begin by imagining that the true statistical dynamics of the Universe flows forwards in hypertime, then the symmetry of forwards and backwards time becomes automatic. (With such a symmetry, we also escape from the class of theories ruled out by the experiment proposed by BELL and SHIMONY. Asymmetry on a small scale can be re-introduced easily enough, for the few phenomena which display it; however, such phenomena are well beyond the scope of this paper.)

Before pressing ahead with the mathematics of this approach, I should emphasize that it is *not* in any way far-fetched. Admittedly, Occam's razor tells us that this theory would be rather fanciful, if compared with a successful four-dimensional theory. But there *are no* successful realistic four-dimensional theories. Everett's theory is the best alternative against which a theory like this can be compared. Surely a five-dimensional theory, *local* in five dimensions, is much more palatable and much closer to classical four-dimensional ideas than assuming that the Universe is a Fock space run by laws nonlocal in that space, with inexplicable tendencies to look « local » with respect to four-dimensional space instead. Some groups of people, opposed to the materialist faction of realist philosophy, have praised quantum mechanics as a theory fuzzy enough to *allow for* phenomena, such as ESP and precognition, whose existence they believe to be an empirical fact. However, present formulations of quantum mechanics do not yield such phenomena; new formulations, cited by such people as KOESTLER, require an extra dimension *anyway*, to allow the mutual accomodation of past and future. If one were willing to postulate an extra dimension in any case, surely it would be better to pick a realistic theory. Or, to put it another way, my own approach to realism is not only simpler than Everett's; it also allows the possibility of treating a larger range of phenomena.

Now: mathematics.

We may compare the equilibrium of the space-time continuum, *between* two time slices, with the equilibrium state of an iron rod, between two cross-sections. From one cross-section, one *cannot* predict the other cross-section, except in a probabilistic way. To make full use of our knowledge, in thermodynamics, we start with the problem of predicting the thermodynamic state (*i.e.* probability distribution) *between* the two cross-sections, given our knowledge of the boundaries. In a simple-minded system, the probability of any configuration in this region equals the *product* of the independent probabilities of the thermodynamic states at each point. The entropy of a configuration equals the logarithm of its probability; therefore, the entropy may be written as the sum of local entropies:

$$S = \int_{x_0}^{x_1} s(T(x))\,\mathrm{d}x\,.$$

PRIGOGINE (³) has elaborated at length on the reasons why this formula can be used for the probability *distributions* of simple-minded systems. However, in an iron rod, we must remember that the probability of an electron pointing up is *dependent* on the prob-

(³) I. PRIGOGINE: *Thermodynamics of Irreversible Processes* (New York, 1967).

ability of nearby electrons pointing up; therefore, the entropy looks like

$$S = \int_{x_0}^{x_1} s\left(M, \frac{\mathrm{d}M}{\mathrm{d}t}\right)\mathrm{d}x \quad \text{(usually a quadratic function).}$$

In the case of a system of particles in space-time, we can write the entropy of a possible probability distribution over a region of space-time as

$$S = \int_{t_0}^{t_1} s\left(p(q_1 \dots q_n), \frac{\mathrm{d}p}{\mathrm{d}t}(q_1 \dots q_n)\right)\mathrm{d}t \qquad (q \text{ is a particle location}).$$

The basic condition of thermodynamic equilibrium is that *this integral is minimized.* This is a classic problem in variational calculus. To solve it, using the Hamiltonian formalism, we get Hamiltonian equations involving *two* functions of $q_1 \dots q_n$, p and a *conjugate* variable v. This gives us two conjugate real functions, the same information as in quantum mechanics. From $p(t_0)$ and $p(t_1)$, we could calculate $v(t_0)$; from $p(t_0)$ and $v(t_0)$, we could calculate $p(t_1)$. *However*, for a p which represents *definite knowledge* of particle state at time t_0, we *cannot* deduce a similar p at t_1; *i.e.* we cannot make deterministic predictions, any more than we can from one cross-section of the iron rod to another. Therefore, with definite knowledge of $q_1 \dots q_n$ at t_0, we cannot know $v(t_0)$ in a precise enough way to predict exactly what $q_1 \dots q_n$ will turn out to be at t_1; this would lead to a principle of uncertainty about our knowledge of v, given p. (This is *not* a principle of indeterminism, however!) Also, we can explain the existence of a « Lagrangian », whose stationarity gives us the equations of quantum mechanics; we can explain the attendant *macroscopic* energy conservation. A different quantum-mechanical Lagrangian will correspond to a different entropy function. Phenomena beyond the compass of quantum mechanics may also exist, if we allow for the occasional existence of less than maximum entropy in the five-dimensional Universe. We do not expect the knowledge of $q_1 \dots q_n$ at time t_0 to incorporate all we know at time t_0 to predict $p(t_1)$, even probabilistically, because we must *also* incorporate any knowledge we might have about $(\mathrm{d}p/\mathrm{d}t)(t_0)$ to get a full prediction.

Notice that I assume that particle *positions* are real, but that momentum is not real in any fundamental way. How can I reconcile this with macroscopic conservation of momentum? The correspondence of the theory with quantum mechanics is enough, in principle, but one would want a better picture of how this works to feel sure about it. So, one can picture the world-line of a particle as a string of soap film, or even as a rubber band, stretched between two points in space-time. Surface tension, based on the positions of the particles, pulls the string tight; internal tension pulls the rubber band tight. The world-line looks like a straight line in equilibrium—implying momentum conservation on a macroscopic scale. *However*, if we look at the soap film more closely, we see that it has a foamy texture on a small scale. So, too, momentum conservation does not hold *on a small scale*, in the classical sense, according to quantum theory.

Looking back at our statistical argument, we might ask if a *four-dimensional* integral could be used as easily as the time integral we used. In fact, Schwinger's source theory (4), an elegant rephrasing of quantum field theory, depends heavily on such variational integrals. Schwinger's theory may not be the only possible formulation of this

(4) J. SCHWINGER: *Particles, Sources and Fields* (Cambridge, Mass., 1970).

kind. Superficially, Schwinger's theory might be taken to imply that *fields* should be treated as thermodynamic quantities, *not* as particle probabilities. Or, in a sophisticated view, one might imagine that *interactions*, not particles *or* fields, are thermodynamic realities; we would have to imagine (deterministic) fields conveying information between these *interactions*. In five-dimensional space, these interactions would look like simple particles moving forwards through hypertime. Once we have renormalized quantum field theory to the point where mass disappears, we would get back to a set of coupled Maxwell equations; a curious property of these equations is that « wave packets »—delta-functions moving in a definite direction—do *not* diverge like classic Heisenberg wave packets. Poisson processes in five dimensions might also be used to lead to a similar formalism. Some people here suggest that ill-posed equations in four dimensions could lead to a similar explanation.

The possibilities mentioned in the paragraph above make it clear that it would be premature here for me to single out *one* realistic explanation of quantum mechanics. As with designing a new type of engine, one discovers that the difficult part comes in defining the first example; thereafter, when *the principle of approach is clarified*, hundreds of specific examples follow. There is a huge backlog of data now in physics, to help us in singling out the best model, and even in distinguishing these models from quantum mechanics; however, this next step will require more time and effort; it leads into a wide number of topics beyond the scope of this article.