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p. 38, line 13

for

"taking ψ_f = const gives $\psi_f = \ldots.$"

read

"taking ψ_f = const gives the classical equations, while quantizing the action $S = \hbar/i = 1/i =$ const. gives $\psi_f = \ldots..$"

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FIXED PAST AND UNCERTAIN FUTURE:*

An Exchange of Correspondence Between

Pierre Noyes, John Bell, and Thomas Phipps, Jr.

ABSTRACT

The quantum mechanics of particles of finite mass, when coupled to special relativity, produces dynamical effects due to the appearance and disappearance of particles. It was proposed that this fact could provide the basis for a covariant quantum particle dynamics. This proposal, and the reactions to it by John Bell (a critic of conventional quantum mechanical interpretations) and Thomas Phipps (whose novel interpretation of quantum mechanics originally suggested this approach) are presented in the temporal sequence in which they occurred.

(Submitted to Foundations of Physics)

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* Work supported in part by the U. S. Atomic Energy Commission.

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FIXED PAST AND UNCERTAIN FUTURE: A SINGLE-TIME COVARIANT QUANTUM PARTICLE MECHANICS*

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ABSTRACT

A covariant quantum mechanics for systems of finite mass particles at finite energy follows from interpreting the quantum fluctuations, needed by Phipps to understand measurement theory and by Gyftopoulos to understand the second law of thermodynamics, as the Wick-Yukawa fluctuations in particle number. The dynamical one-variable equations require as input the N-1 particle transition matrices and an N-N vertex, or coupling constants at three-particle vertices.

* Work supported in part by the U.S. Atomic Energy Commission.

The Phipps derivation[1] and interpretation[2,3] of quantum mechanics restores to physics a conceptually unique past history of particulate events while retaining the uncertainty needed to describe the individually random but statistically convergent results obtained in particle scattering experiments. By identifying the random element as the Wick[4]-Yukawa[5] fluctuations in particle number we arrive at transition amplitudes T_{BA} connecting any system of $N(\leq N_A \leq N_B)$ finite-mass particles of finite energy to any system of N_B particles similarly restricted. These equations are uniquely specified by postulating the observable amplitudes $T_{nn'}$ $(n, n' \leq N_B - 1)$ and an elementary T_{BA}. If we add the Wick-Yukawa postulate, our equations contain as parameters only the coupling constants and the masses of the particles we choose to include in the dynamical system. Conventional results are guaranteed for the quantum electrodynamics of charged leptons to order e^4, and probably to order e^6, but interesting differences can be expected in the properties of hadrons.

The Hamilton-Jacobi equations are the geometrical optics of a wave theory whose covariant wavelength was determined by de Broglie[6] by taking the invariant phase-space volume of the normal modes to be Planck's constant. Phipps[1] noted that as operator equations acting on some state ψ_f, taking ψ_f = const. gives the classical equations, while quantizing the action $S = \hbar/i = 1/i$ = const. gives $\psi_f = \phi(\underline{x}_n, t) \exp(-i\Sigma_n \underline{P}_n \cdot \underline{X}_n)$ where ϕ is the conventional Schrödinger wave function. Thus the Hamilton-Jacobi equations have a correspondence limit in both classical and quantum mechanics but also have a more general class of solutions in which neither ψ_f nor S are constant; this third class has not received much attention subsequent to Phipps' original investigation.[1] Since the $\underline{P}_n$, $\underline{X}_n$ obey Poisson bracket (commutation) relations in the classical (quantum) limits, they also obey the uncertainty relations in the quantum limit.

Phipps[2,3] sees his unconventional phase factor as representing the values of $\underline{P}_n$ and $\underline{X}_n$ which result when any (unobservable and time-reversible) process is completed and joins the fixed past. These values retain their classical significance as constants of the motion between interactions, but their uncontrollable change at the time of each interaction provides that severance of phase connections which is needed to explain the random results obtained experimentally in individual measurements while leaving intact the statistical phase connections contained in the observable amplitude ϕ. This interpretation simultaneously cuts the Gordian knot within two tangled skeins in natural philosophy. When describing experiments one need no longer postulate the "collapse of the wave function" or some metaphysical "act of observation". That a counter fired or that an ion cascade leading to a bubble or a developable grain of emulsion occurred was due to a unique sequence of past events which can be partially retrodicted from subsequent observation. Yet the randomly varying phases whose time history describes this fixed past are not "hidden variables" in Bell's[7] sense because the future remains only statistically predictable following ordinary quantum mechanical usage. Thus the successful experimental refutation of most hidden variable theories[8] offers no barrier to the acceptance of the Phipps formalism. By giving a unique interpretation to the direction of "time's arrow" as the motion of the interface between the fixed past and the uncertain future, the Phipps interpretation also identifies at the quantum level the origin of the irreversibility implied by the second law of thermodynamics; the necessity for some mechanism of this precise type has been discussed in detail by Gyftopoulos and Hatsopoulos. [9]

For any system of particles whose momenta $\underline{P}_n$ and energies ϵ_n define the scalars $m_n = (\epsilon_n^2 - P_n^2)^{\frac{1}{2}}$ invariant under proper Lorentz transformations (with additional discrete labels for distinguishable particles of the same mass [10]) we assume that if we measure (see below) these momenta and energies for some system of N_A particles, and then made a measurement of all the particles we can find in the system at some subsequent time, the N_B particles so measured will again have uniquely defined scalar masses; the sums of their momenta and energies will add up to the sums of the initial values, but otherwise will be random variables. Nevertheless, if we repeat the same experiment many times the distribution of these values will converge, in the sense of the law of large numbers, to a unique function which can be computed [11] from a unique Lorentz scalar $(\epsilon_1 \ldots \epsilon_{N_B})^{\frac{1}{2}} T_{BA}(\epsilon_1 \ldots \epsilon_{N_A})^{\frac{1}{2}}$. Because of their reliance on one of the "Copenhagen" interpretations of quantum mechanics, Goldberger and Watson [11] are forced to an elaborate wave-packet construction in order to justify their connection between the transition amplitude and observables, but the same prediction for the future stemming from a unique past follows immediately from the Phipps interpretation of the free-particle wave functions once we have postulated some transition amplitude at some (fixed but unknown) time in the past and summed over all on-shell fluctuations allowed up to the present by the uncertainty principle [12]. Convergence to scattering boundary conditions is obtained by a factor $\int_{-\infty}^{t} dt' \exp \eta t'$ and gives the usual stationary state formalism in the limit $\eta \to 0^+$, as in Lippman and Schwinger [13], except that it is on-shell t-matrices or Yukawa-type vertices which are unknown, rather than an unmeasurable short-range interaction. Thus Phipps can talk of a unique past, while Goldberger and Watson can only discuss a statistical ensemble of pasts with no way of distinguishing the one which happens to be ours, if indeed that question has any meaning for them. Clearly at this level the two theories are observationally indistinguishable, and will differ only in the dynamical equations used to compute T_{BA}.

Conventional theory has great difficulty in arriving at unambiguous input for strong interaction dynamics. The two-nucleon problem requires the specification of a "potential", which cannot be taken from experiment because there are an infinite number of nonlocal models which predict the same two-body observables. Each of these will have, in principle, different consequences in the three-nucleon problem, but again there are an infinite number of ways to add three-body forces to any three-body system generated from any of these two-body models in such a way as to maintain identical predictions for all two- and three-body observables, as is easily proved in the exterior-interior representation[14] by integrating out the off-shell variables. If, instead, the two-nucleon interaction is to be computed from some Yukawa theory of the second quantized matter field, there has been no consensus for over two decades as to how to calculate even the two-pion contribution to two-nucleon scattering, let alone how to approach the three-nucleon problem. In contrast, application of Phipps' basic assumptions tells us that if we know the two-nucleon on-shell amplitudes (phase shifts), we can uniquely compute what will happen in the three-nucleon system if only these pairwise scatterings occur. Since only completed processes can have observable consequences, we simply add up all nonrepeated pairwise scatterings allowed by the uncertainty principle. This gives us integral equations of the same algebraic structure as the Alt-Grassberger-Sandhas equations[15] (or in the three-body case, the once-iterated Faddeev equations) but with on-shell kinematics, and hence only one variable (the total energy), no matter how many particles we are discussing; since all particles are on shell, we can use relativistic kinematics. If the predictions (at the level of accuracy of the input observables) fail to agree with experiment, we must add an N-body force, or (in an elementary particle theory) an additional particle (meson) whose production channel happens to be energetically closed. [16, 17] Generalizations to include spinors (using helicity amplitudes), SU_2, SU_3, SU_6 ..., or other symmetries at the vertices appear to be straightforward.

Up to this point we have discussed only scattering experiments with finite mass particles. If they are charged we can couple them to the electromagnetic field by the gauge-invariant and covariant prescription $p_n \rightarrow p_n - (e_n/c) A(\underline{x}_n, t)$, and thus measure their momenta and energies using macroscopic electromagnetic fields. But as was demonstrated by Bohr and Rosenfeld [18], any material system which (like ours) satisfies the uncertainty principle will generate the same uncertainties in the measurement of such fields which are more conventionally calculated by second quantization. Hence we know in advance that our theory will not only agree with conventional quantum electrodynamics in the nonrelativistic limit, but also for any radiative transition which does not change particle number, or significantly contain frequencies which could. If we add the postulate that there are positively charged particles of the same mass as electrons, the French-Weiskopf calculation [19] guarantees that our theory will predict the usual result for the anomalous magnetic moment of the electron, vacuum polarization, and the Lamb shift, to order e^4; the Brodsky-Roskies time-ordered e^6 calculations in the infinite momentum frame presented at the Batavia Conference in 1972 suggest that QED can also be done this way to the next order. When it comes to high energy electron-proton scattering, we anticipate new results. Our wave functions carry the covariant normalization $d^3p/(p^2 + m^2)^{\frac{1}{2}}$ and hence correspond to particles of finite size, as was pointed out by Serber in 1950. [20] As such they will have finite electromagnetic self-energies of order $e^2/(\hbar/mc) = (e^2/\hbar c)mc^2$ and can be expected to have an additional factor of $m^2/(q^2+m^2)$ in their electromagnetic form factors. Assuming dominance by a vector meson of mass m_V we therefore expect that the e-p proton form factor will be proportional to $m_V^2 m_p^2/(q^2+m_V^2)(q^2+m_p^2)$, which gives, according to Lapidus[21], a better fit to the data than either a sum of poles or a single dipole. Since we can expect the same result for any narrow resonance, the deep inelastic form factors will go as $(m_V/m_R)^2(q^2/m_R^2)^{-2}$, which Moreno[22] has shown is all that is required to insure scaling, and perhaps good results for inclusive reactions as well; thus our theory might get on without partons.

The clear gains which flow from adopting the interpretation of quantum mechanics advocated here, in addition to the conceptual gains in measurement theory and the second law of thermodynamics already discussed by Phipps and by Gyftopoulos and Hatsopoulos, are (1) one-variable integral equations for any finite set of hadronic transition amplitudes using well-defined theoretical or phenomenological parameters and on-shell particles, and (2) finite electromagnetic self-energies for all charged particles. The immediate price to be paid is to abandon both "nuclear potentials" and second quantization of the "matter field". This entails removing from the category of "absolute laws" CPT, the connection between spin and statistics, and the exact equality of particle and antiparticle masses. But in the eyes of the author, this is a gain rather than a loss, since this freedom allows us to construct theories which break these and other symmetries and compare their consequences with experiment. Most of the use made of these theorems in practice stems not from their character as <u>exact</u> laws but from the fact that they provide a simple way to insure <u>detailed balance.</u> Of course our theory must also insure this in any dynamical postulate for any elementary vertex, or we would run the risk of running afoul of well-known facts about macroscopic statistical equilibrium. Our theory explicitly does not contain macroscopic time-reversal invariance, and is indeed for this very reason in better accord (at least conceptually) with the second law of thermodynamics than conventional theories. Speculatively, this makes re-examination of the $K_L - K_S$ puzzle look promising. Since our theory is (up to a proper Lorentz transformation) embedded in a unique space-time, there is no barrier in principle to using a generally covariant frame and thus treating gravitation geometrically rather than as a field. One could then approach the quantum restrictions on gravitational measurement along the lines of Bohr and Rosenfeld, as Oppenheimer[23] suggested long ago. This would also provide a natural way to investigate extremely high energy behavior, that is particles with energies comparable to the rest-energy of the measuring apparatus (or earth, or galaxy, or receding galaxies, ...) if one wishes to reintroduce Mach's principle into physics. Whether such spectulations will bear fruit must be left to the uncertain future to decide.

The greatest intellectual debt which the author must acknowledge is to Thomas Phipps. His determination to develop a new approach to quantum mechanics in spite of the weight of conventional opinion against such innovation made this covariant generalization possible. Less obvious, but even more significant, was the author's increasing reliance on an evolutionary, historical, and dialectical approach. James Young, Franz Gross, and W. Ebenhöh provided helpful and creative criticism of the evolving theory during the final stages of preparation. My final acknowledgement must be to my wife, whose surprise when I told her that the idea of a fixed past and uncertain future — which for her is obvious common sense — is foreign to current theories, gave me the needed encouragement to present this obvious theory to my colleagues.

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(Letter from John Bell to Pierre Noyes — original handwritten)

Geneva — 7 April 1973

Dear Pierre,

I wish I could respond more intelligently to your very intriguing paper. But first you have to help me with Phipps. I have already tried to understand his proposals, because I like very much his reservations about ordinary Q. M. Now I have tried again, re-reading his "Time asymmetry, etc." of which I have a preprint. But I am still quite mystified. First, how does one choose between "macroscopic", "atomic", and "nuclear" domains? Do the laws applicable for 10 or 10^{10} particles no longer apply for 10^{23}? Take the "atomic" domain. Then I understand that the past was a sequence of events each with coordinates

$$(x_k, \ p_k, \ T) \quad k = 1 \ \dots \ 3n$$

and that the present is described by a wavefunction

$$\psi = e^{-i\Sigma \, x_k p_k} \phi(x_k, t)$$

where ϕ satisfies the Schrodinger equation. I understand that ψ can be used somehow to give a probability distribution for (x, p, T) of the <u>next</u> event, but what exactly is the formula? <u>And what happens to ϕ</u> (as distinct from the phase factor) after that next event? I really cannot find the answers in the paper; can you help me?

With warm regards,

John

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August 6, 1973
(Note added August 23, 1973)

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Dear John:

It was bad luck that you were in Ireland while I was in London and Geneva in late May. It would have been much easier to discuss Phipps' approach to quantum mechanics, and my own ideas which stem from thinking about his work, face to face. You are one of the few people who has expressed any interest in the deeper implications of this approach, and I hope we will have time for extended discussion before too long. In the meantime I will try to keep your interest alive by describing how I interpret Phipps' work and propose to extend it.

I had two long discussions with Tom Phipps (who is an old friend) in Washington this April, and obtained a much clearer view of how he views the situation than had been possible by correspondence, let alone by reading his published or preprinted but rejected papers. I may not do him justice in what follows, so I am sending a copy of this letter to him for comment, clarification, correction, and objection; you should let him explain his own views in his own words, rather than accepting this commentary at face value. As I understand it, he does not claim to have presented a specific theory, or even a fully articulated interpretation of quantum mechanics, but simply a set of <u>equations</u>

which are badly in need of interpretation. These equations are the Hamilton-Jacobi equations of classical mechanics reinterpreted as operator equations. In the classical limit in which the wave function (operand) is constant, they reproduce the classical results. But as we know, this Hamilton-Jacobi theory can be considered to be the geometrical optics limit of a wave theory, and specifically if the action is quantized by the recipie $S \rightarrow \hbar/i$ = const., the wave theory has the same formal structure as conventional quantum mechanics, except for the exponential factor whose phase is the summation of $\underline{P}_n \cdot \underline{X}_n$ over the degrees of freedom. Thus the minimal accomplishment of the Phipps equations is to preserve the same number of parameters (degrees of freedom) in both the classical and the quantum limits. In 1960 he showed that there is a consistent transition theory in which neither the wave function nor the action are constant, and exhibited a covariant example based on the one-particle Dirac equation. He has also attempted to interpret these equations over the years, and I find the most significant formulation to be that the Schroedinger wave function describes virtual and reversible processes, while the phase factor (constants of the motion in the classical limit) represent the initial state (preparation of the system); the discontinuous change in phase is interpreted as an event (observed or not) which terminates a sequence of virtual processes and defines the moment at which the uncertain future joins the fixed past.

These equations have, for me, a number of attractive features. I have never been able to understand what is meant by the severance of the phase connection or the collapse of the wave function in discussions by the Copenhagen school, while Phipps' identification of this transition with a quantum event is not mysterious. The fact that this interpretation of all quantum events as irreversible immediately supplies an interpretation of the second law of thermodynamics, and avoids the quagmire of ergodic theory, unifies two problem areas in physics in a simple way. Over the last two or three years I have tried to discuss these aspects of Phipps' work with some of our colleagues who are concerned about the foundations of our discipline; they were always interested, and always ignorant of what Phipps has written in these subjects. I therefore decided to attempt to get a short paper on his work published in such a way as to broaden the audience receptive to these ideas. This resolve was stimulated by a conversation with Tom in 1971 in which he asked my advice as to useful literature on the relativistic two-body problem.

Once I took up the task seriously, I was soon struck by the thought that the Wick-Yukawa mechanism might provide a quantized description of the discontinuous processes implied by the Phipps interpretation, and at the same time support a covariant formalism that would not be restricted to one or two particles. The paper you have received (THP-3, "Fixed Past and Uncertain Future: A covariant quantum particle mechanics") was the end result, achieved after many false starts, and submitted for publication in late August 1972. I also enclose a paper on "Three Body Forces" which contains some of the same material and which was presented at the UCLA conference on Few Body Problems that same month. Over the past year I have discussed these ideas with a number of physicists, and have reached the conclusion that the most compelling theoretical idea for me is that it might be possible to rest relativistic quantum particle mechanics directly on the basic ideas of finite (quantized) mass values with a smallest unit and the fluctuations in particle number which seem to be required if one accepts both quantum mechanics and the mass-energy relation. Thus I am working on a "Democritean" mechanics which contains only (for hadrons) particles and the void, but in which particle number can fluctuate. Thus my starting point now is different from that of Phipps, but retains some points of contact. I will try to spell out my understanding of Phipps' work in this letter, and defer discussion of Democritean particle quantum mechanics to another occasion.

If one is willing to specialize discussion to the case of a finite number of particles with finite masses, and for clarity restrict the discussion to the case in which these mass values m_n ($n = 1, \ldots, N_A$) and their associated momenta $\underline{P}_n$ are known from past observations, and similar information can be obtained about the system at the present time (i.e. conventional scattering boundary conditions), I believe that the content of the Phipps equations in the quantum limit can be articulated with considerable precision. We assume throughout that the energy of each particle is related to its momentum and mass by $\epsilon_n(\underline{P}_n) = +\sqrt{m_n^2 + P_n^2}$ and that the total energy of the system at any time is equal to the sum of the energies of the particles at that time (and similarly for the total vector momentum). Then the Phipps equations* for one such system have the form

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* Note these are a very special case of the general Phipps equations; in what follows I will use "Phipps equations" only in this restricted sense.

$$\sum_{n=1}^{N_A} \left(m_n^2 + p_n^2\right)^{\frac{1}{2}} \Psi = \frac{1}{i} \frac{\partial}{\partial t} \Psi$$

$$n = 1, \ldots N_A \begin{cases} \underline{p}_n \Psi = \frac{1}{i} \nabla_{\underline{x}_n} \Psi \\ -\underline{P}_n \Psi = \frac{1}{i} \nabla_{\underline{X}_n} \Psi \end{cases} \tag{1}$$

$$\Psi = \Psi(x_n, X_n; t); \; \hbar = 1 = c$$

These equations have the immediate solution

$$\Psi_A = \frac{1}{(2\pi)^{3N_A/2}} e^{i \sum_{n=1}^{N_A} \underline{P}_n \cdot (\underline{x}_n - \underline{X}_n)} e^{-iE_A t} \tag{2}$$

$$E_A = \sum_{n=1}^{N_A} \epsilon_N(P_n) \; .$$

This solution satisfies our postulated initial boundary condition not only in the remote past but for all times. Yet we know empirically (or theoretically if we couple relativity and quantum mechanics through the Wick-Yukawa mechanism) that if we examine systems which (so far as we know) started with the same masses and momenta in the remote past, we can find systems with (the same or different masses and particle number N_B and) different momenta $\underline{K}_n$, $n = 1, \ldots, N_B$. Subject to the restriction that $E_B = E_A$ and that $\Sigma_{n=1}^{N_B} \underline{K}_n = \Sigma_{n=1}^{N_A} \underline{P}_n$, these momenta are independent of the initial values $\underline{P}_n$. However, if we repeat the experiment often enough, and select cases leading to a specific system of particles N_B, the momenta of the particles will approach (in the sense of the law of large numbers) a unique (statistical) distribution. Our theoretical problem is to connect the theoretical description of the initial system Ψ_A and all allowed Ψ_B in such a way as to preserve these "facts." For separated systems all we need do is postulate that the constants m_n, $\underline{P}_n$ and hence ϵ_n which occur in the solution (Eq. 2) of the Phipps equations are the constants which we define empirically by whatever meaning we attach to the measurement of the energy and momentum of a particle of mass m_n. The problem is to write an expression, plus rules, which can describe a system initially represented by Ψ_A but in which at later times we can sometimes find Ψ_A and sometimes some specific example of Ψ_B which, in the limit of a large number of observations will lead to a statistical prediction of the observables in Ψ_B.

In some sense, which it will be the purpose of a <u>dynamical</u> theory to make more precise, these new states Ψ_B must arise out of the initial state, and hence must be computable through some function A_{BA} which multiplies the initial state Ψ_A at the time when the transition occurs. Consider first a single fluctuation at some time t' with $-\infty < t' < t$. The general form of the new wave function will then be

$$\Psi^{t'}_{BA} = \Psi_A(\underline{x}_n, \underline{X}_n, t) + A^{t'}_{BA}\Psi_A(t)\,\theta(t-t') \tag{4}$$

$$= \Psi_A(\underline{x}_n, \underline{X}_n, t) + P^{t'}_{BA}\Psi_B(\underline{y}_n, \underline{Y}_n, t-t')\,\theta(t-t') \tag{5}$$

In general there can be many states B, and fluctuations at many times t', so that summing over these

$$\Psi_{N_B N_A} = \Psi_A(\underline{x}_n, \underline{X}_n, t) + \int_{-\infty}^{t} dt' \Sigma_B P^{t'}_{BA}\Psi_B(\underline{y}_n, \underline{Y}_n, t-t')\ . \tag{6}$$

In order that this description reduce to Ψ_A as $T \to -\infty$, we introduce the obvious convergence factor $e^{+\eta(t'-t)}$ (with the implied limit $\eta \to 0^+$ to be taken later) and with

$$\Psi_{N_B N_A} = \Psi_A(\underline{x}_n, \underline{X}_n, t) + \int_{-\infty}^{t} dt' \; e^{+\eta(t'-t)} \; \Sigma_B \, P^{t'}_{BA} \Psi_B(\underline{y}_n, \underline{Y}_n, t') \tag{7}$$

This satisfies our boundary conditions at $t = -\infty$, but there are additional conditions which must be imposed on the (so far arbitrary) amplitude fluctuation probability A_{BA}. Note in particular that interference between the two pieces of the wave function could, in general, give information about the $\underline{X}_n$ and $\underline{Y}_n$ and hence introduce hidden variables. In a particular situation which at time t leads to unique system B with specified momenta $\underline{K}_B$ we have (I believe) an irreversible transition in which the (so far virtual) processes we are describing become determinate (though only partially retrodictable) and join the fixed past. This can happen at any time t', whether recorded (observed) or not. But $\underline{P}_n$, $\underline{X}_n$ and $\underline{K}_n$, $\underline{Y}_n$ are canonical variables and, (in the quantum limit) just as subject to the uncertainty principle as the dynamical variables $\underline{p}_n$, $\underline{x}_n$ or $\underline{k}_n$, $\underline{y}_n$. Thus to satisfy our boundary condition of precisely known $\underline{P}_n$ and precisely knowable $\underline{K}_n$, we must insure that $|\Psi_{N_B N_A}|^2$ contains no reference to $\underline{X}_n$ or $\underline{Y}_n$. This can be accomplished by postulating that

$$A^{t'}_{BA} = i(2\pi)^{3N_A/2} \, e^{i\underline{K}_n \cdot \underline{Y}_n} \, \mathscr{T}_{BA}(\underline{K}_1 \cdots \underline{K}_{N_B}; \underline{P}_1, \ldots, \underline{P}_{N_A}) \; e^{-i\sum_{n=1}^{N_A} \underline{P}_n \cdot \underline{X}_n - i\sum_{n=1}^{N_B} \underline{K}_n \cdot \underline{Y}_n} \; e^{iE_A t'} \; e^{-iE_B(t-t')} \tag{8}$$

where the energy variables

$$E_A = \sum_{n=1}^{N_A} \epsilon_n(\underline{P}_n) \text{ and } E_B = \sum_{n=1}^{N_B} \epsilon_n(\underline{K}_n) \tag{9}$$

can differ because of the uncertainty principle connecting energy and time. Equivalently, because of Eqs. (5), (6), and (7), we could assume that

$$P_{BA}^{t'} = i(2\pi)^{3N_A/2} e^{\eta(t'-t)} e^{i\left(\sum_{n=1}^{N_B} \underline{K}_n \cdot \underline{Y}_n - E_A t'\right)} \mathscr{T}_{BA}(\underline{K}_1 \dots \underline{K}_{N_B}; \underline{P}_1 \dots \underline{P}_{N_A}) \, . \tag{10}$$

In order to conserve momentum we <u>also</u> postulate that

$$\begin{aligned} \mathscr{T}_{BA}(\underline{K}_1, \dots, \underline{K}_{N_B}, \underline{P}_1 \dots \underline{P}_{N_A}) = T_{BA}&(\underline{K}_1 \dots \underline{K}_{N_B}, \underline{P}_1 \dots \underline{P}_{N_A}) \\ &\delta^3\left(\sum_{n=1}^{N_B} \underline{K}_n - \sum_{n=1}^{N_B} \underline{P}_n\right) \end{aligned} \tag{11}$$

Substituting into Eq. (7) and letting $\Sigma_B \to \int d^3K_1 \dots d^3K_{N_B}$ we obtain

$$\begin{aligned} \psi_{N_B N_A} = e^{-i\left(\sum_{n=1}^{N_A} \underline{P}_n \cdot \underline{X}_n + E_A t\right)} &\left[\frac{1}{(2\pi)^{3N_A/2}} e^{i \sum_{n=1}^{N_A} \underline{P}_n \cdot \underline{x}_n} + \frac{i}{(2\pi)^{3N_B/2}} \times \right. \\ \times \int d^3K_1 \dots d^3K_{N_B} \int_{-\infty}^{t} dt' \, e^{i\left(E_A + i\eta - E_B\right)(t-t')} & T_{BA} \, e^{i \sum_{n=1}^{N_B} \underline{K}_n \cdot \underline{y}_n} \\ &\left. \delta^3\left(\sum_{n=1}^{N_B} \underline{K}_n - \sum_{n=1}^{N_A} \underline{P}_n\right) \right] \end{aligned} \tag{12}$$

Thus, by insuring that there is no way of determining $\underline{X}_n$ or $\underline{Y}_n$ if $\underline{P}_n$ and $\underline{K}_n$ can be precisely known (i. e. by postulating the uncertainty principle), and insuring momentum conservation we show that the Phipps equations for these boundary conditions yield

$$\Psi_{N_B N_A} = e^{-i \sum_{n=1}^{N_A} \underline{P}_n \cdot \underline{X}_n} \Phi_{N_B N_A} (\underline{x}_n, \underline{y}_n) e^{-iE_A t} \tag{13}$$

where $\Phi_{N_B N_A}$ is the usual stationary state wave function as given, for example, in Goldberger and Watson

$$\Phi_{N_B N_A} = \frac{1}{(2\pi)^{3N_A/2}} e^{i \sum_{n=1}^{N_A} \underline{P}_n \cdot \underline{x}_n} + \frac{1}{(2\pi)^{3N_B/2}} \int d^3K_1 \ldots d^3K_{N_B}$$

$$\frac{T_{BA} e^{i \sum_{n=1}^{N_B} \underline{K}_n \cdot \underline{y}_n} \delta^3\left(\sum_{n=1}^{N_B} \underline{K}_n - \sum_{n=1}^{N_A} \underline{P}_n \right)}{\sum_{n=1}^{N_A} \epsilon_n(\underline{P}_n) + i\eta - \sum_{n=1}^{N_B} \epsilon_n(\underline{K}_n)} \tag{14}$$

with the conceptually significant difference that T_{BA} is now an <u>arbitrary</u> function referring to fluctuations and not to interactions.*

* (Note added August 23 in response to a query from John Bell.)

Since $\Phi_{N_B N_A}$ depends on the dynamical coordinates of both the N_A and the N_B systems $(\underline{x}_n, \underline{y}_n)$, it does not (except for elastic scattering when $\underline{x}_n \equiv \underline{y}_n$) correspond to a single solution of the Phipps equation for either system but to that combination of the solutions for both systems which describe the virtual states that can arise out of the time evolution of the initial state due to momentum-conserving fluctuations. Once we probe the system (e.g. by an electromagnetic measurement) in such a way that we are certain that (at the time of probing) there are only N_B particles present, this description is no longer appropriate, and we should construct a new wave function appropriate to the new state of knowledge (new boundary condition). I would prefer to describe this as the start of a new problem rather than as the "collapse" of the old wave function. Note that, although we cannot retrodict at what space-time points $(\underline{X}_n, t')$ the N_A particles disappeared and at what points $(\underline{Y}_n, t')$, the N_B particles appeared, we are at liberty to assume that there was some such unique event when the $\Phi_{N_B N_A}$ that describes the uncertain future in fact joined the fixed past and was replaced by the Ψ_{N_B} found by the subsequent probing of the system. From this point of view the probe does not "create" ψ_{N_B}, but simply informs us that <u>from now on</u> we can make more precise (in the statistical sense) predictions of the future by constructing a new wave function incorporating the new information. Whether or not we exercise this option is a matter of choice and in no way affects the actual course that the system follows.

We would be <u>foolish</u> to ignore the possibility of using the information given by the probe for future predictions, but history reveals all too clearly that there is no law of nature that prevents physicists from being foolish.

In a subsequent letter I will develop a dynamical scheme for calculating T_{BA} starting from the basic assumption of the Wick-Yukawa mechanism, and believe I can show that the T_{BA}'s so generated are both covariant and unitary. But for the moment, I will restrict myself to this somewhat tedious "derivation" quite transparently constructed by the Lippmann-Schwinger technique in order to reproduce the conventional formalism with the overall Phipps phase factor. Note that the Goldberger-Watson "derivation" of $\Phi_{N_B N_A}$ also uses this phase factor, but in a more elaborate way as they use it to construct wave packets. I hope that my route shows that the same observable consequences (given the same T_{BA}) can be obtained simply by allowing arbitrary fluctuations in space-time, and summing up all such fluctuations consistent with momentum conservation, the uncertainty principle in momentum, and the uncertainty principle in energy.

All this preliminary work was undertaken in the hope that I can now answer some of the questions in your letter in a rather precise way. As I see it, the amplitude $A^{t'}_{BA}$ describes a non-local fluctuation in which certain specified particles m_n $(n=1,\ldots,N_A)$ which in the remote past were known to have precisely defined momenta $\underline{P}_n$ suddenly disappear at the space-time points $\underline{X}_n$, t' and a new set of particles m_n $(n=1,\ldots,N_B)$ suddenly appear at (in general disconnected) space-time points $\underline{Y}_n$, t'. The dependence of this amplitude on quantities other than the past observables $\underline{P}_n$ known from the boundary conditions, and the potential observables $\underline{K}_n$, must be constructed in such a way that no transition (recorded or not) at the present (time t) can recover any knowledge of the specific points $\underline{X}_n$, $\underline{Y}_n$. We must also sum over all such fluctuations in order to prevent any retrodictable knowledge of when between the past (when the initial momenta were fixed) and the present these fluctuations occurred. For me this formulation brings out in a very clear and specific way the essential non-locality implied by the uncertainty principle, which has been the focus of so much of your own work. Although I have restricted my treatment to precisely known momenta, I think it obvious that this basis set can be used to construct intermediate cases in which there is partial knowledge of both $\underline{X}_n$ and $\underline{P}_n$ or of E and t' consistent with the uncertainty principle. The connection to actual laboratory observables is trivial — it is the same connection with T_{BA} (assumed known) as is given by the conventional theory (e.g. the connection to cross sections given in Goldberger and Watson, including the restrictions required for covariance and unitarity). Another way of putting it is that I believe I have shown how to describe any quantum-mechanical scattering process starting and ending with a system of finite mass particles with specified momenta at finite energy. Just as Newtonian mechanics (or its covariant generalization)for such systems leaves the dynamics (other than conservation of momentum and energy) open, I believe this approach isolates the descriptive features of quantum particle scattering from the dynamical calculation of T_{BA}. I leave that to another letter, but the scheme is roughed out in the papers I have already sent you, or enclose.

To finish with Phipps, note that his paper in Dialectica on the question of physical size emphasizes the fact that his equations are scale invariant and contain precisely the same number of degrees of freedom as the classical equations. As I believe I have shown in this letter, in the quantum limit they imply only conventional results (with a slightly novel interpretation) if T_{BA} is computed in a conventional way. But in the intermediate cases, where neither the action nor the wave function are constant, they contain richer possibilities. I have not (and do not intend to) explore these in detail, but so far as I can see these are genuine hidden variable theories of a different type than those of the Bohm-Vigier school. On the atomic level, these are already ruled out (to some accuracy) by your theorem and the Freedman-Clauser experiment. But if the action S departs from the fixed value $\hbar/i$ over regions of nuclear dimensions, I believe that there is no current experiment which unambiguously excludes them. This is the type of theory Phipps explored in 1960, and I refer you to that paper to see how one can preserve quantum mechanics at the atomic level and yet introduce hidden variables at the nuclear level. Thus I think the repeat of the Freedman-Clauser experiment at nuclear dimensions is well worth pursuing. However, the new dimensional parameter that presumably comes in from modifying S at short distances implies new physics for which I see, at present, no experimental necessity. Thus my own future work will concentrate on restoring approximate locality (at distances larger than the Compton wavelength of the particles involved) via transitions which are localizable to the extent that the uncertainty principle and the mass-energy relation allow (i.e. via the Wick-Yukawa mechanism).

I hope that this lengthy epistle will clarify rather than confuse the issues you raise. I would be delighted to respond to any further questions.

Sincerely,

Pierre

Pierre Noyes

PN:sj

(Letter from Pierre Noyes to Thomas Phipps, Jr., personal material deleted)

August 10, 1973

Dear Tom,

* * *

I was in Europe unexpectedly at the end of May ... and hoped to see John during that time frame, but failed. However, I got a lot of useful input from various old friends during the three weeks I was abroad, and have had a great deal more from half a dozen three-body experts who happen to be at SLAC this summer. On the technical end, I am now convinced that I can formulate a (mathematically) well-defined three-body equation using only two-body phase shifts and binding energies, and am trying to rush this through. The technical point I was missing for the last few months was that, although my interpretation of your equations in the quantum limit is necessarily non-local (see enclosed letter to Bell), if I base my dynamics on the Wick-Yukawa postulate, the third particle rides through at distances larger than a Compton wavelength from the scattering pair. Thus I can use conventional Faddeev kinematics outside this region, and approximately localize my radical ideas within a pion Compton wavelength.

I am sorry you still have not heard from Bastin, but I saw him in London for a few hours with his mathematical mentor (Kilminster) and in a four-way session with the two of them and Castillejo. I have a Bastin preprint which I will duplicate for you if you want to see it. He has a scheme some years old which generates in a "natural" way starting from the possibility of rational discrimination (the dicotomic variables 0 and 1) a sequence which yields the pure numbers 3, 10, 137, $\sim 10^{38}$ and then terminates. The way I envisage connecting up this "Edingtonian" approach with physics is via the Dyson observation that Quantum Electrodynamics breaks down when there are 137 particles of the same mass within the (so-defined) unique Compton wavelength, and hence that space-time coordinates defined by electromagnetic measurements loose precise meaning within such regions. Gravitational definition is still possible down to regions which contain $\sim 10^{38}$ hadrons within a Compton wavelength, but at that point they are all within a Schwartschild radius and form a black hole with <u>no</u> particulate quantum numbers — only mass, charge, and angular momentum. The number 10 is approximately the number of pions (interacting with a coupling constant of 0.08) which can be identified as distinct particles, and the three perhaps have something to do with quarks. This leaves out weak "interactions," but since these are (approximately) quadrilinear in the particle amplitudes, they might come about by a clash between electromagnetic and gravitational effects, which fits with the intermediate number of 10^{13} as a second order effect in the primary sequence.

What this has led me to is an attempt to reformulate quantization as resting on the quantization of mass, and hence a Democritean quantum mechanics in which there are only particles and the void, but in which the number of particles can change via the Wick-Yukawa mechanism. The resulting model for elementary particles is a charge cloud surrounding a black hole (with zero spin for bosons and 1/2 spin for fermions) which shields the hole by a Coulomb barrier $\sim 137\,e^2$ (or for quarks 1/9 or 2/9 or 4/9 that value) high. I will try to spell this out in more detail later.

This also couples into a new cosmological approach due to Darwin Shannon, in which there is a static frame in which particles are at rest, but which in our observational frame has an accelerated expansion; the expansion produces not only the recession of the distant galaxies but also the gravitational attraction of neighboring bodies once energy-momentum conservation is properly imposed on the two descriptions. He also pictures a neutron as proton containing an electron inside its central black hole (thus avoiding the uncertainty principle on the coordinates of the electron, since it exists in a conjugate universe), which decays when the electron leaks out of the black hole into our universe. Discussing that model this week, I got the vague idea that neutrinos might be neutral black holes of spin 1/2, which would complete the picture up to still more broken symmetries (muons, SU_3, SU_6).

* * *

Best regards,

Pierre

NAVAL ORDNANCE LABORATORY
WHITE OAK

SILVER SPRING, MARYLAND 20910

IN REPLY REFER TO:
120:TEP:hb
20 August 1973

AIR MAIL

Professor H. P. Noyes
Stanford Linear Accelerator Center
Stanford University, Box 4349
Stanford, California 94305

Dear Pierre:

Thanks for the opportunity to add something to your excellent exposition and substantial extension of my ideas. Let me say first that you have told me three times about the latter and I am finally beginning to think I understand. There are points on which we continue to be in some disagreement, but I have at last made a connection that enables me to measure with my own yardstick the decisive importance of your contribution.

I refer to your treatment of "fluctuations" in the description of scattering. Because of its manifest non-locality, scattering has always seemed a sort of non-event that is difficult to conjoin to the rudiments of relativity, as we know the subject. (Einstein's relativity of 1905 represents as I see it a sort of last stand for Huyghenian causal thinking about electromagnetic interactions. It embodies built-in difficulties in describing the kinematics of nonlocal physical interactions. This is not the place to go into that — but I invite you to contemplate what is meant in kinematic terms when we innocently say that the Moessbauer lattice "as a whole" takes up recoil momentum.... simultaneously in what sense and in what frame, or else with what measure of nonsimultaneity?) In fact, when Bohr finally formulated his measurement theory in a way that appeared to satisfy him, he rested his whole case on nonlocality, through the dictum that "the apparatus as a whole" makes the measurement. A conception more diametrically opposed to the foundations of a relativity based on point events is hardly conceivable. Is it any wonder that relativistic quantum theory has had a few creaks in its joints?

I vaguely recognized the problem in some of my earlier writings. For instance in a paper on "Sufficiency of Equations of Motion in Q. M." (twice rejected by *Nuovo Cimento*, though probably still the most scholarly — *i.e.*,

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least readable — thing I have put together on measurement theory) I distinguished two classes of events, which I termed "phase-information preserving" and "phase-information destroying." It remains a useful distinction, I am now (as a result of your work) more firmly convinced than ever. The PID events are the "measurements" of Margenau, the "detections" of Feynman, the "happenings" of hippies, etc. That is, they are the localized true point events, conversions of pure states into mixtures, wave-function reductions, losses of "phase memory," severings of von Neumann chains, etc. The Minkowski world is structured as an irregular lattice of such events, with nothing much (but Aristotelian potentiality) in between. In Einstein's terms the space-time point events are the "elements of reality." Einstein was quite right in criticizing a mathematical theory that claimed to be about reality but that contained no parameters descriptive of its elements. If he had not been unhappy he would have been untrue to himself. Were it not that "progress" in science resembles the forward motion of lemmings, I am convinced that more physicists would have joined him.

There is nothing to be happy about in a theory that claims to embody a formal "Correspondence," yet absent-mindedly mislays half the classical canonical variables in the process, then covers its nakedness with a fog of blather about "mind," which could just as well be the "God" whose sensorium provided Newton with such convenient cover in circumstances of like embarrassment. I'm pretty absent-minded myself, but when it comes to counting parameters I'll take on any performing horse (or nonperforming physicist). Aha, the defenders of orthodoxy maintain that there really is a change in the number of parameters to be used in the description of nature, as we go from "large" to "small"? Then let them say precisely at what point, or on what physical scale, the change occurs, or by what empirical criteria we are to recognize it. Such thoughts underlay my essay in Dialectica (1969) on the "relativity of physical size." To those who don't know the theory, I'm afraid the present remarks will merely serve to calibrate my degree of opinionation. That's the trouble with talking about physics instead of talking physics.

As for the phase-information preserving "events," these have a physically nugatory character because they represent "virtual processes" that lie "in between" the PID events in space-time. Why consider them at all, then? I did not have a good answer to this, before your work, because I had only trivial examples of PIP events, such as the nonlocal interaction of a photon with a specular reflector, or the "turn-around events" of the

Zitterbewegung. But by furnishing through your scattering example, Pierre, the first nontrivial actual calculation illustrating the usefulness of the concept of phase-information preserving events, you have answered the question. I don't know the details of your calculation, but feel sure that the theme is right: your "fluctuations" are my phase-information preserving or virtual events. I can hardly exaggerate my pleasure at this outcome, for it clears up the basic problem I have had from the start about scattering. I knew it was nonlocal in its physical nature, yet I badly wanted to speak of it in "event language." Now you have shown a way to do this, and thereby cleared away what is probably the last obstacle to a logically coherent post-measurement theory, which I stubbornly refuse to call a measurement theory, but insist on calling a theory of contingent events.

Among the accomplishments of this event theory, then, are the following:

(1) Restoration of objectivity, rehabilitation of the "event" (one of the casualties of Copenhagen) in quantum theory, eradication of "mind" and "observer" as occult enabling elements in physical processes, etc.

(2) Postulational purification. Equations of motion now logically suffice for physical description. No need for a heirarchy of postulates with a pecking order, such that some postulates (e.g., Projection or Selection Postulates) contradict and override others (viz., previously postulated equations of motion).

(3) Perfection of formal Correspondence — nothing to explain about where the missing parameters (classical "constants of the motion") went.

(4) "Complete" description of the individual quantum system — nothing to explain about where on the physical scale we must (allegedly) switch from single-system to statistical-ensemble mode of description.

(5) Time irreversibility of "completed processes" (events) now describable at the quantum level, despite manifest time reversibility of equations of motion descriptive of virtual processes. I have a paper emphasizing this aspect forthcoming in the fourth issue for 1973 of the Margenau-Yourgrau journal Foundations of Physics (probably not available until 1974). By the way, I should like to put in a plug for this as the only American journal that will print my work (big deal). It is in

precarious financial straits and needs the support of all physicists not spiritually wedded to the AIP's Pravda and Isvestia.

(6) Relativity of physical size. Here my Dialectica article is relevant. I was struck in reading your Bastin-stimulated thoughts, Pierre, which pictured 10^{38} hadrons up a black hole, with things popping in and out of various universes, that there could be no conceivable end to physics, unless....unless one set of equations of motion could be found that would be truly scale-independent. Of course, no scholar wants his subject to end. But I assume we are among scientists (whom I distinguish from scholars as having an instinct for the jugular — i.e., scientists bloody well want to kill the problem, not perpetuate it as an annuity for their kind....and don't mind if they have to scuff some of the hide off Absolute Truth to do it.) Anyway, physics is the art of successive approximations, and in the domain of the very small we are still groping for the zeroth approximation. Curious, is it not, with what godlike certainty editors and referees move in this remote and obscure realm their wonders to perform?

(7) Lastly, I want to bring out for special emphasis the rehabilitation of classical Newtonian mechanics accomplished through the circumstance that the Hamilton-Jacobi solutions are one class of exact solutions of the general equations of motion I proposed [Phys. Rev. 118, 1653 (1960)]. Only aficionados of existing measurement theory will fully appreciate this: Bohr always had to talk about the measuring apparatus in classical terms, and that meant that quantum mechanics, which was supposed to be a more "fundamental" discipline, was erected on foundations consisting of material of supposedly lesser worth — the noble granite fortress was built on a hunk of shale. Now we see things in quite a different light: The worth-status of classical and quantum mechanical solutions is identical. The two types of states are simply alternative classes of exact solutions of a more comprehensive set of equations of motion. Since they are both exact mathematical solutions, and are not identical, neither can represent Truth, but both must humbly represent mere approximation. The quantum-state approximation is better for describing individual events separated in time, the classical-state approximation is better for describing continuous world lines. Neither is completely True, ever. Each is only a better or worse approximation for the case in hand. Wave packets are amusing artifacts for easing the transition, but they are not True, either. The payoff from this viewpoint is immediate: The scope of faith is contracted. We no longer have to believe in the unobservable. We do not have to hypothesize that the light

billiard ball will "come apart" before the massive billiard table it sits on does. There is no empirical justification for supposing that either billiard balls or billiard tables spontaneously come apart ever. Wave packets, yes; balls and tables, no. Look around the universe. Do you see the waves of gross matter disassembled in the fashion ("spreading of the wave packet") that ordinary quantum solutions, conceived as Truth, seem to demand? The universe has been here quite awhile. Perhaps we should look harder around the edges for fuzz. Or maybe the fuzz has been in our thinking. I find this new (or old) perception of the relationship of mathematics to reality (i.e. alternate mathematical solutions as rival candidates for approximate physical description) a welcome liberation from metaphysical faith in empirically unverifiable hypotheses.

This brings me to the third class of solutions of my proposed equations of motion. I have alluded above to two classes, (1) the classical states corresponding to constant operand Ψ and variable principal function S, (2) the atomic quantum states corresponding to variable Ψ and constant $S(=-\frac{\hbar}{i})$. The remaining possibility, which you termed "transition" solutions, is that neither S nor Ψ is constant. Only this third class of solutions puts the theory to any test, since the other two classes of solutions are trivial enough to be treated as two separate mechanical theories — indeed they have been so treated historically. It goes without saying that my personal interest centers on this third class (which, coincidentally, I called Class III in my 1960 paper). If this proves nonphysical my interest will wane, regardless of the outcome of your ingenious applications of the Class II solutions, Pierre — because it will mean that my general principles, such as size relativity, are without physical significance. In other words, my instincts are wrong and I should not be in physics.

About 18 years, generously filled with busy-work, have quietly gone by since I concocted my proposed equations of motion, and I have done little or nothing to investigate the Class III solutions — preferring perhaps to leave this as my own annuity, but also aware that the Zeitgeist was not right for a really new mechanics in which the Heisenberg postulate (not satisfied by Class III solutions) is dethroned, and formally non-Hermitean operators (with imaginary momentum states) are introduced. Though I believe this discouraging prognosis is still valid and will remain so as long as physicists persist in their modern Ptolemaism (which replaces the foreknown philosophically correct descriptive element, the circle, with

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the foreknown philosophically correct descriptive element, the field, and even links together with adjustable coupling constants these paragons of the nature-mimic's art, as if physics were Fourier analysis), I will mention here a few points in favor of my alternative.

(1) If the size-relativity hypothesis is correct, and there exists one preserved form of mechanical equations of motion on all physical scales; if, as I believe to be the case, this preserved form is in exact formal Correspondence with the Hamilton-Jacobi equations; and if such equations, classically descriptive of <u>any particle</u> in the world, turn out on the atomic scale to <u>be descriptive only of the electron-positron</u> (as is in fact the case, <u>cf</u>. the <u>Dirac equation</u>); then I am inescapably forced to draw a conclusion of breathtaking simplicity: there exists only one type of particle in the world — the electron-positron. All heavier particles are composites of it, built up as complexes of the Class III real-energy, imaginary-momentum states exemplified in my 1960 paper. (All discussion of the physics was expunged from this paper during three years of refereeing, but footnote 8 of the paper gives a clue to the needed amplification.) In these states the Heisenberg postulate is locally violated, within about a Compton wavelength of the force center, but is recovered as a limiting case far from that center. This local violation explains how electrons and positrons can permanently exist in nuclei. The bound states, as noted, happen to be ones of real mass-energy and imaginary momentum.

(2) In approaching the description of a domain so remote from daily experience as nuclei and "elementary" particles, we need to look at it as children would, open directly to experience rather than burdened with presuppositions. If we do so, then the Mont Blanc that immediately meets our eye is the so-called "saturation of nuclear forces." It is this phenomenon, call it what you will, that explains why the particle does not collapse on itself. In an earlier era, if we had called the failure of the atom to collapse on itself "saturation of coulombic forces" we would have been wholly misled and semantically directed away from any perception of the need to redo mechanics. Now according to my view what is happening to prevent the "particle" from collapsing on itself is essentially a replay of what happened to save the atom from a similar fate. To accomplish the latter salvation we dropped the assumption $pq - qp = 0$ and replaced it with $pq - qp = (\text{constant})$; to accomplish the former we must drop $pq - qp = (\text{constant})$ in favor of $pq - qp = (\text{variable function}$ of distance from collective force center, which approaches

Heisenberg's constant value at distances long compared to the Compton wavelength of the complex). The variable function ($pq - qp = S$ in my theory) is not separately postulated, but is evaluated automatically (via the boundary condition just mentioned) in the process of solving the equations of motion for Class III solutions ($S \neq$ constant, $\Psi \neq$ constant). Thus I cannot agree, Pierre, with your judgment that "modifying S at short distances implies new physics for which I see, at present, no experimental necessity." It implies old physics, which must be looked at with new eyes.

(3) Once we provide ourselves with the necessary optical equipment, seeing other agreements becomes a useful and enjoyable sport. Why, for instance, are the charges of such disparate particles as the proton and positron identical? Present physics sees this as an attribute of charge. But a theory is weak in proportion to the number of unanalyzed "attributes" it assigns to nature. (Thus, before Einstein, it was an "attribute" of mass that its gravitational and inertial forms were equivalent.) How much simpler it is to see the charges as equal because a positron is contained and permanently exists within the hyper-relativistic complex known as a proton. Similarly with K-capture and beta-decay, how much simpler it is to suppose that a continuously-existing electron (positron) makes a transition between states of real (observable) and imaginary momentum, rather than to postulate something qualitatively new, a "creation" or "annihilation." State changes we know about — that is a good, old mechanical conception. But creation/annihilation is the work of the Fourier analysts. I tend to consign it to the same limbo as second quantization, a subject completely obviated if (a) one does first quantization right, (b) one finds a proper form for the relativistic many-body mechanical Hamiltonian.

The latter is unfortunately something I have yet to be satisfied about. For instance, I do not like the Hamiltonian in your Eq. (1) because it has square roots in it. Dirac taught us there is profit in explicit linearization, and I am reluctant to forget this lesson, knowing as I do the all-importance of form (preservation) in physics. (I am not criticizing — for your intended purposes the form is perfectly all right.)

I could go on with quite a list of qualitative evidences favoring the "experimental necessity" of my approach: Spins, like charges, are the same in heavy and light particles because the former are composites of the latter. The overall charge neutrality of the universe, polarization of the vacuum, etc., are readily explained by a resurrection of hole theory. (I rely on the electron as the key building block of the vacuum as well as the "heavy particle.") By this line of thinking

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one has to take Dirac's negative-energy electron sea seriously as a modern version of the electromagnetic ether. (I never was happy with E- and H-fields as mathematical vectors propagating precociously straight out of our brains and into the physical world.) The prettiest qualitative feature of the theory arises from the conception of the neutrino as the energy quantum associated with electronic transitions between states of real and imaginary momentum, just as the photon is associated with electronic transitions between different states of real momentum. But it escapes me why there should be different types of neutrinos.

So my ideas bring troubles along with opportunities for clarification, and I cannot be dogmatic about them. Maybe they will meet and join with yours on the far side of the Democritean void, or something. I have no quantitative support for my thesis that Class III solutions describe nuclei. (For instance I cannot yet calculate the binding energy of the deuteron, because it arises from the interaction of two extreme-relativistic many-body collectives. This is fundamentally far more difficult than the calculation from Schroedinger's equation of the binding energy of, say, a diatomic molecule.) My first attempt to calculate heavy-particle magnetic moments failed miserably, and led as I recall to a ridiculous value for nuclei of the order of 1/4 of a Bohr magneton. The trouble is that a prerequisite to meaningful calculations is a proper formulation of the relativistic mechanical many-body problem (which does not lean on field theoretical crutches). That sounds like a cop-out; but my ideas are in fact defeatable by observation. If Ehrenhaft's famous sub-electron, or anything resembling a quark, should ever be observed, my ideas would be utterly defeated and at once discardable. I seriously wonder, if I should be right, whether our species will ever be able to put this into the simple package it belongs in. Most of the relevant observational evidence we shall ever amass lies already before us. The clues are there. Our children are not smarter than we, merely more biased by our biases. The answer sits grinning at us, daring us to open our eyes and see it.

Ergo bibamus,

Tom

T. E. PHIPPS, JR.