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Comments on Leiter's Paper on Non-Linear Wave Mechanics

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This note is in response to a recent article in this journal by Leiter (1970). My comments have to do with both the conceptual and the mathematical aspects of his article. From his own discussion, it is difficult to ascertain Leiter's philosophical stand. But it seems to me that he quite misunderstands the approach that I have been taking in my work when he compares Bohr's approach to the theory of measurement with the stand that I have been investigating.

The elementary interaction theory, upon which my research program has been based, was first applied to predictions about atomic systems in a paper with Schwebel in 1961 (Sachs & Schwebel, 1961). Since then, the philosophical aspects of this approach have been spelled out in detail in several articles (Sachs, 1964, 1967, 1968, 1970). The starting point of this theory is the contention that a full exploitation of relativity theory necessitates the use of the closed system as an elementary entity. That is to say, with this approach, the closed system cannot, in principle, be decomposed into distinguishable parts. The interacting components of this closed system could be a large measuring apparatus and an electron or the electron–proton system, or the whole universe! But whatever model one wishes to start with (for convenience in the problem at hand) it is the single closed system—without actual parts—that one is investigating here. It is in this sense that this entity is referred to as 'elementary'.

The elementary interaction is then described in terms of a field (mapped in one space-time) that solves a set of coupled nonlinear field equations. *In principle*, it is only after the solutions are obtained that one can take the asymptotic limit (corresponding to weak energy-momentum transfer between the components of the closed system) in which they appear (as a mathematical approximation) as the solutions of a quantum mechanical-like formalism, describing distinguishable parts, in interaction.

In Bohr's philosophy, the particle is still an elementary entity, even though his view denies that anything can be said about this particle without also incorporating the description of the measurement (by a macroscopic apparatus)—in an intrinsically probabilistic way. In my approach, there is no fundamental distinction between 'macroscopic' and 'microscopic', as it is so in Bohr's view.

The Copenhagen school adopts the philosophical stand of positivism, while the elementary interaction theory of this author takes the philosophical stand of realism. In the latter, it is the elementary interaction, describing in fundamental terms the *closed system*, that is the universal from which one wishes to derive the particulars (i.e. the consequences of experimentation). It is an important difference in these two approaches that the elementary interaction relates to a single entity that is without actual parts while Bohr starts with an ensemble of elementary things (but without a description in terms of predetermined dynamical variables, as in classical mechanics).

The mathematical consequences of these two views are also different, in principle. Quantum mechanics is a fundamentally linear theory while the elementary interaction theory is fundamentally nonlinear. Even though the two formalisms *approach* each other in the proper limit, it is important to note that features of the solutions of nonlinear equations, generally, that predict physical consequences, do not necessarily vanish as these solutions approach the linear limit—so long as the actual limit is not reached. There are consequences of the nonlinear features of this author's equations that were shown in earlier publications to predict some properties of charged particle systems that have not been rigorously predicted by quantum field theory (or any other theory). Two important derivations were (1) the physical consequences of the Pauli principle (Sachs, 1963) and (2) the Planck spectral distribution for blackbody radiation (Sachs, 1965). The latter derivation was based on the result of this theory that while there is no actual physical property of a true vacuum, the space occupied by matter in any experimental observation is populated with particle–antiparticle pairs in a particular bound state [which Schwebel and I called the 'annihilation state'—even though there is no actual annihilation here (Sachs & Schwebel, 1961)]. In his article, Leiter also referred to this 'annihilation state' that we have derived, but he does not do anything with it.

I am not quite certain which of the philosophical stands Leiter is actually taking since his language seems to mix contrasting ideas from the Copenhagen approach and those of this author. For example, Leiter still tries to maintain (in words) the Heisenberg uncertainty principle, as a fundamental ingredient in his field theory. But if his theory is actually nonlinear, as he claims, then this is not possible. He claims (in a footnote) that the Schwarz inequality (which is used to derive the uncertainty relations) is not dependent on the linearity of the field formalism. This is not true. The derivation of this inequality and its application to the predictions of a field theory is based on the assumption that the solutions of the field formalism are the (square integrable) elements of a linear function space (Messiah, 1966).

The field theory that I have been studying (as in the formalism of Einstein's general relativity) does not have this mathematical structure and therefore cannot incorporate the uncertainty relations for this mathematical (as well as conceptual) reason—even though the asymptotic limit, in which the formalism of quantum mechanics is approached, does incorporate these uncertainty relations *as a mathematical approximation.*

The field equations that Leiter writes down in the second section of his paper are close to this author's formalism, with two exceptions. First, a spinor interaction appears in the latter equations for electrodynamics (in a natural way) from the analysis [that was originally carried out by Sachs & Schwebel (1961)]. It was this part of the formalism that we showed to give a prediction of the Lamb shift, in very good quantitative agreement with the data for the measured values on the fine structure of hydrogen. The second difference between Leiter's formalism and ours is in his introduction of the retarded and advanced potentials in a nonsymmetric way. The reason that these are introduced symmetrically in our approach [as in the Wheeler–Feynman theory (Wheeler & Feynman, 1945)] is the requirement here that it should make no difference in the description of the fundamental interaction as to the labelling on each of the interacting components. To introduce these potentials nonsymmetrically, as Leiter does, seems to me to destroy the 'elementarity' of the measurement—which Leiter claims he starts with as a fundamental building block.

Another comment is that Leiter assumes the existence of stationary state solutions and a fundamental linearity in their description—implying that the fundamentally nonlinear function space of his formalism can generally be reduced to a linear function space. It is conceivable that this could happen accidentally as a consequence of the imposition of some very special conditions. But it is very difficult for me to accept this assertion as a general feature of the equations that he starts with.

Finally, I might comment that from my own research program, I would answer Leiter's question: Can atomic processes be described with nonlinear wave mechanics? with an affirmative conclusion. For the results of this research have already led to a derivation of the entire hydrogen spectrum (Sachs & Schwebel, 1961), the derivation of effects that are normally associated with pair annihilation–creation (Sachs & Schwebel, 1961; Sachs, 1968) and generally, to the containment of the quantum mechanical formalism as a mathematical approximation (in the appropriate limit) for the nonlinear formalism that follows from the elementary interaction field theory.

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