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A Lost Alternative to Dirac's Equation

Lev Landau and Yakov Frenkel, two friends so well remembered in separate articles by Alexander Akhiezer and Rudolf Peierls in your June 1994 issue (pages 35 and 44, respectively), share critical roles in a remarkable but largely forgotten episode in the early history of the quantum mechanics of the relativistic spin-½ electron. It is today a common consensual belief that Dirac produced, in 1928, a uniquely well-suited descriptive equation for spin-½ particles.[1] But is this belief wholly valid? A retrospective analysis shows a much richer situation. Apart from Dirac's seminal 1928 paper, only two independently proposed foundational theories appeared contemporaneously—one by Frenkel,[2] then one by Dmitrii Iwanenko and Landau[3] (in which the inspiration of Frenkel is acknowledged). Frenkel's paper was a sketch of a theory; Iwanenko and Landau developed their arguments much further and explicitly referenced Dirac's first publication. By my reckoning, however, it is highly unlikely that Iwanenko and Landau (whose paper may not have been refereed) saw Dirac's paper before the very last stages of completion of their paper.

Unlike Dirac, Iwanenko and Landau used antisymmetric tensors of various orders ("recalling the example of the EM field") for wavefunctions. They employed an elaborate Lagrangian to build their equation set (Dirac used a Hamiltonian) and so could use standard variational methods to get density, current and so on. Lastly, Iwanenko and Landau explicitly proposed multielectron extensions, regarded by them as a favorable point of comparison with Dirac. But Iwanenko and Landau's paper also has many similarities to Dirac's: They emphasized the broad foundations of a satisfactory theory; their equations are clearly Lorentz invariant; they put no a *priori* constraint on the effective number of components of their wavefunctions; their wave equations are first-order linear in $\partial / \partial \chi_\mu$; they obtained conditions on coefficients and equation form by using classical equations as limiting cases; and the electron magnetic moment falls automatically out of their equations. And their ultimate published result is effectively that of Dirac: a Klein–Gordon-like equation with added terms in the electromagnetic fields **E** and **H** due to spin, which they show, analogously to Dirac, produces as a first approximation the 1927 results of Charles Darwin[4] for the hydrogen spectrum, the best spin-½ theory that had been proposed up till then. This is precisely the same initial agreement that gave Dirac confidence in his results. In short, Iwanenko and Landau's paper appears to cover similar ground to Dirac's, with comparable results. Indeed, Iwanenko and Landau, in comparing their work with Dirac's, took pains to assert that "both theories, apart from their complete difference of methods and equations, *appear to be equivalent* [emphasis added], although their detailed connections are unclear for us." They had no illusions, however, about the need for deeper analysis to test the validity of this assertion definitively.

The reaction to the Russian paper has a curious history, mostly of silence. The proposal was not even commented on in any way—praise or ridicule—in Pauli's extensive correspondence[5] of the time, although Dirac's was, especially in the Pauli–Heisenberg and Pauli–Dirac exchanges. I suggest that the Dirac equation has flourished, while the striking Iwanenko–Landau proposal at once fell into oblivion, because the latter proposal was typographically and aesthetically unpleasing and excessively cumbersome. Nor is their equation as conducive (even, as it turned out, for their creators) to a great range of detailed applications: Only Dirac's equation prompted an explosion of results. The elegant, spare, intrinsic simplicity of the Dirac theory plays *the* decisive, clearly identifiable operational role; actual new thought experiments are wholly convincing here.

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The Artful Interferer

I enjoyed Daniel Kleppner's Reference Frame column "Some Small Big Science" (October 1994, page 9). It reminded me of an issue related to measuring small amplitudes of which even many experimenters seem unaware.

Kleppner, in discussing measuring a transition amplitude of strength 10^{-11}, states: "Observing this by brute force . . . is out of the question: The rate, which depends on the square of the amplitude, would be 22 orders of magnitude smaller than for a normal transition." The non-brute-force technique, described by Kleppner, is to have a larger but well-known and controllable amplitude interfere with the very small one. Choosing the best value of the interfering amplitude is, according to Kleppner, "one of the secrets of the experimental art."

Thus, as it is commonly stated, the direct transition is too small to observe, and with interference one produces an observable signal. The point here is that this may or may not be true; if true, it is worth elucidating what it is that's relevant to the "art" of experimental science.

Let us consider a small amplitude that we want to measure, which we will call B, using Kleppner's notation. It is desired to measure this as accurately as possible. We will choose another amplitude A with which B will interfere, and we will assume that A is well known. Then we will observe an event total, given by

$$R = F\,|A + B|^2$$

Here F can be taken to represent the total incident particle flux for the experiment. Now the sensitivity to B will be given by the derivative dR/dB. What is relevant is the statistical significance of a departure of the measured R value for a small change in the unknown amplitude B. Since R is a number of counts, its fluctuation will be given by $\sqrt{R}$. (This argument assumes that the number of counts is large enough that Gaussian statistics applies. It also assumes that A and F do not need independent measuring and that A and B maximally interfere. These assumptions are approxi-