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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 approximately true in many situations.) Thus the number of standard deviations N_σ that a change of ΔB in B will produce will be given by

$$N_\sigma = \frac{\mathrm{d}R/\mathrm{d}B}{\sqrt{R}}\,\Delta B$$

If we now evaluate the right-hand side using the above expression for R, we find $N_\sigma = 2\sqrt{F}\,\Delta B$, *independent* of A! What this means is that if one wants the change ΔB in the value for the small parameter B to produce, say, a two-standard-deviation effect in the experimental outcome, one can derive the necessary total flux, and the result does not depend at all on the chosen A with which B interferes. So much for the "experimental art."

Let's give an example for clarification. Let $B = 10^{-11}$ and choose $\Delta B = 10^{-12}$ to produce a two-standard-deviation effect. According to the expression above, the total flux F would be

$$F = \left(\frac{N_\sigma}{2\Delta B}\right)^2$$

giving 10^{24}. Let us first take $A = 1$; then

$$R = 10^{24}\,|\,1 + 10^{-11}\,|^2 \approx 10^{24} + 2\times 10^{13}$$

This deviates from the expression in the absence of B by 20 standard deviations, so that a 10^{-12} change in B produces a 2σ effect, as desired. Now let us take $A = 0$; then

$$R = 10^{24}\,|\,10^{-11}\,|^2 = 100$$

This deviates from zero by 10 standard deviations; a 10^{-12} change in B produces a 20% effect, or again a 2σ change in R.

Why then is Kleppner correct in stating that it is important to choose A carefully? It is simply a matter of experimental noise. With $A = 1$, one has an effect that is at the level of 10^{-11} which, depending on the particular situation, may be too small to detect. With $A = 0$, there is no background from the "A" term, but the noise may be too great to permit one to see the naked "direct transition." Thus A is chosen somewhere between 0 and 1 with consideration to the noise levels in the particular experiment, not because the pure transition is too small. Sometimes the noise may scale with the magnitude of A; other times there may be a constant noise level present independent of A.

One example showing that the "brute force" technique does work is the search for the decay of the proton. Here experiments have a sensitivity on the order of 10^{30} years; the expected signal is terribly small, but the signature is so unique that the transition can be detected directly. Another is in the measurement[1] of the small electromagnetic interaction of the neutral kaon with the electron. In that experiment, A was the much larger strong interaction of the kaon with the nucleus, which could be made to interfere with B, the K–e interaction. The experiment consisted of measuring $|A+B|^2$ and $|A|^2$ separately and thereby isolating an effect. This technique involves taking the difference of large numbers, where one has to pay very close attention to systematic uncertainty. It is possible that using the same amount of beam to detect the K–e interaction directly (with an energetic electron emerging from the target) would have produced a more significant result.

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Henry Torrey's Signal Nmr Achievement

Frederick Seitz's excellent article on World War II research on silicon and germanium semiconductors and transistor devices (January, page 22) describes Henry C. Torrey's leadership of the crystal diode work at the MIT Radiation Laboratory. It was not mentioned and is in general not well known in the physics community that Torrey also found time in 1945 to pioneer in another research direction that opened the door to a major new field of 20th-century physics, namely nuclear magnetic resonance.

In earlier work at Columbia University under I. I. Rabi, Torrey gained the background that later, at MIT, gave him unique insight into the physics of spin systems and led to improved estimates of spin–lattice relaxation time and of the rf voltage level needed to avoid saturation. This expertise made possible the design of the first successful experiment on nmr in solids, in 1945,[1] after previous workers had failed.[2] Torrey's collaborators in the experimental implementation of nmr were his MIT coworkers Edward M. Purcell and Robert V. Pound, who became well known for their later nmr research with Nicolaas Bloembergen on solids and liquids, carried out at Harvard University. The experimental skill of the MIT group, perhaps sharpened by their Rad Lab experience, is attested to by their inspired combination of an off-the-shelf oscillator, electromagnet and voltage amplifier, which produced an observable proton nmr signal with a paraffin sample on the first attempt, within the experimental parameters estimated by Torrey.

It is somewhat surprising that in 1995, the 50th anniversary of the discovery of nmr, this historic first has not received wider recognition and some form of commemoration. The detailed story of this episode, including the roots at Columbia University, the flowering at MIT and the various contributions of the participants, remains an inadequately reported chapter in the history of physics.

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Open NSF's Purse to Those Outside Academe

I wholeheartedly agree with Henry Ehrenreich in "Strategic Curiosity: Semiconductor Physics in the 1950s" (January, page 28) that it is important to protect the position of "generic," "curiosity-driven" or "basic" research within the National Science Foundation. Surely there are other agencies, such as the National Institute of Standards and Technology, that are better suited to playing the lead role in "strategic" research. That is not to say, however, that changes at NSF should not be made in light of changing conditions within the physics profession. Specifically, I have in mind the traditional rule that the NSF-sponsored single-investigator proposal, a key component of basic research, is usually limited to researchers within the university community.

In the current situation, graduating physicists who go on to careers in government, industry, nonprofit institutions, contract research and development centers and self-employment are excluded from principal-investigator status in a broad range of NSF programs directed toward basic research. This would be a majority of graduating and recently graduated PhDs. I suggest that as it is improper to deny participation based on gender or race, so too is it inappropriate to deny participation based on institutional affiliation. This nation needs to take advantage of the possible contributions of *all* physicists in this increasingly competitive world, especially in an era of ever tightening Federal budgets, when it is imperative to make the fullest use of available expertise.

There seems to be general agreement that we are producing more PhDs than there are traditional academic jobs at universities. This is not necessarily a bad thing, and some people have noted that physics training provides a rigorous background suited to a whole host of careers. If leaders within the physics community itself would set the good example of attempting to open up NSF research funding to all qualified physicists, regardless of institutional affiliation, this would provide a powerful example of the usefulness of physics training beyond traditional university research. Also, by looking more at the researcher than at his or her place of employment, I believe we would be taking a necessary step in increasing the stature of the physicist as an independent professional. Such a stature would serve well in enabling physicists to thrive outside traditional roles.

As it may be artificial to distinguish between strategic and curiosity-driven research, so too might it be artificial if not out of date to distinguish between university-based and otherwise-based researchers. And it might be wrong, too, if the purpose of Federal support for basic research is the advancement of the best possible physics.

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Can Coal Combustion Breed Pu in the Sky?

The conventional wisdom regarding plutonium in the environment is that its halflife of 24 400 years is sufficiently short that no natural-source plutonium remains in the biosphere, and any plutonium in the biosphere must have originated from breeding plutonium in uranium for nuclear weapons and reactors. This "wisdom" may be flawed, however, and we must ask if plutonium is being bred in the biosphere by natural, but unidentified, means.

The mechanism for breeding plutonium is well known: A uranium-238 nucleus plus one neutron becomes plutonium-239 after passing through some intermediate steps. Trace element analysis of coal shows significant quantities of uranium and thorium. For example, Environmental Protection Agency analysis[1] of 5000 samples of coal from varied sources gives an average uranium concentra-