

It was a pleasure and most informative to read David Gross's article on Eugene Wigner (December 1995, page 46). However, his understanding of Wigner's position on one very important issue in physics differs from mine.

Gross states that Wigner's "analysis provided a *definition* of what we mean by an elementary particle, which according to Wigner should be identified as an irreducible representation of the Poincaré group" (Gross's emphasis). On the other hand, Wigner once told me emphatically that "a particle is a point object that moves on a world line."

At that time, I did not understand his concern about the clarification of this definition, but I now believe that he was objecting to identifying a particle with a group representation. He was very interested in the foundations of physics, and the nature of the fundamental elements of quantum mechanics was an important matter for him. Is an electron a particle or a wave? A discussion of this question requires a clear understanding of what we mean by the word "particle."

The word was originally introduced into classical mechanics, where it clearly fits the definition that Wigner gave me. The original founders of quantum mechanics depended on keeping as close to classical mechanics as possible. Consequently, attempts were made to carry over such words as "particle" into the new theory, resulting in confusion as to the definition of the word.

Wigner was aware of some of the thoughts on this question that had been expressed by a number of physicists whose work has indicated that there are no particles in a properly interpreted quantum mechanics. They have found that the appearance of particle-like phenomena, such as alpha particle tracks in a cloud chamber, is due to the interaction of the wave function with the surrounding medium.¹ If this is the case, the fundamental elements of quantum mechanics are fields, not particles.

This is a very serious problem for physicists because the word "particle" pervades many of their communications. High-energy physics is often called particle physics and yet it is surely based on quantum field theory. The introduction of the term "wave-particle duality" is often confusing to students, who find it difficult to know when an electron is a wave and when a particle. In my opinion, a real effort should be made to rid our literature of this word when quantum mechanics is used.

I cannot resist closing with an anecdote that reveals important aspects

of Wigner's character. He and I were attending a civil defense conference being held at a hotel in Atlanta. As we stood at the hotel desk, a very young and neatly dressed soldier in uniform approached the desk somewhat timidly. He asked how much it would cost for a room for the night. When the clerk told him, the young soldier appeared crestfallen and turned away. Wigner quickly got the clerk's attention and said, "I will pay for half of this man's room." He knew very well that I would come up with the other half.

Eugene Wigner was a kind and patriotic man, and many of us are thankful for having had the opportunity to know him.

Reference

1. A. A. Broyles, *Phys. Rev. A* **48**, 1055 (1993), and references therein.

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Spacetime May Be Chief Source of Proton Spin

In his article "Where Does the Proton Really Get Its Spin?" (September 1995, page 24), Robert L. Jaffe reports that polarized scattering experiments have revealed that the quark spins contribute only 20–30% of the spin of a proton or neutron, and that the source of the rest of the spin remains unknown.

The spin of elementary particles manifests itself in several effects in fundamental interactions, such as the splitting of nuclear energy levels, the nondegeneracy of hadronic states in strong interactions and parity violations in weak states.

To answer the question of where does the proton or neutron acquire the 70–80% of the spin not supplied by quark spins, we suggest that spacetime has torsion.

It has long been recognized in gravitational theories that torsion is a manifestation of spin, and this inherent spin of spacetime has been recently studied by Venzo de Sabbata and Chidambaram Sivaran.¹ Torsion is to spin as curvature of spacetime is to mass. Gravity can be unified with the electroweak and strong interactions by an energy-dependent spin torsion coupling constant. In their book, Sabbata and Sivaran discuss in detail the idea that all interactions can be understood as originating in spin-curvature coupling.

They also show how torsion in spacetime could solve the problem of

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the cosmological constant. According to Sabbata and Siravan, the introduction of torsion could also resolve the deep incompatibility that exists between gravitational theories and quantum mechanics. Einstein recognized very early that torsion could represent spin; with Elie Cartan, he developed the so-called Einstein–Cartan theory,² which is a viable alternative to the theory of general relativity. This theory is indeed interesting; for instance, the electromagnetic fields do not couple to the torsion. As far as we know, the Einstein–Cartan theory has not been disproved by any experimental evidence.

A key question to ask is whether the geometrical character of spacetime endowed with torsion is amenable to experimental testing. Our answer is yes! The Einstein–Cartan theory predicts new physical phenomena, as explained by Sabbata and Sivarvan.

References

1. V. de Sabbata, C. Sivarvan, *Spin and Torsion in Gravitation*, World Scientific, Singapore (1994).
2. For a thorough account, based mainly on cosmological consequences, see A. K. Raychaudhuri, *Theoretical Cosmology*, Oxford U.P., Oxford (1978).

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More on Schwinger's Views on Cold Fusion

The splendid commemorative article on Julian Schwinger by Paul Martin and Sheldon Glashow (October 1995, page 40) provides a remarkably human picture of one of the more brilliant physicists of our time. It is wonderful to learn not only of Julian Schwinger's many accomplishments but also of his legacy.

I believe it is fitting, and consistent with Schwinger's concern for accuracy and truthfulness, to present some additional facts concerning his involvement in cold fusion.

First, not only did Schwinger believe in the phenomenon of cold fusion, but he resigned from the American Physical Society in 1990 as a direct consequence of the manner in which an APS journal's editorial board had dealt with the subject.¹

Second, Schwinger observed quite correctly that it is entirely possible (contrary to the situation in free space) for unexpected modes of energy transfer to occur within a solid when a region that is macroscopically small but microscopically large

"attains a state of such uniformity that it can function collectively in absorbing . . . energy."¹ (Collective motion and interaction of precisely this nature lie at the heart of the modern theory of conductivity in solids and are responsible, for example, for our understanding of electron holes and their application in modern semiconductor technology.)

Third, although Schwinger did not believe cold fusion and sonoluminescence to be directly linked, he found it helpful to use sonoluminescence to draw attention to the fact that, through coherent phenomena, it is possible to transfer energy between entities (atoms and nuclei) that possess characteristic energies that are vastly different.¹

In addition, it is worthwhile noting that Schwinger recognized that the attainment of high loadings of deuterium (D) into palladium deuteride (PdD), approaching the limit of $x \rightarrow 1$ in PdD_x, could be expected to provide the environment in which the kinds of collective phenomena suggested by his theory would apply. Published information in peer-reviewed journals² and conference proceedings³ exists that not only illustrates the reality of anomalously large heating effects in heavily deuterated PdD but also provides documentation that a necessary condition for achieving the heating phenomenon is that these kinds of loadings ($x \rightarrow 1$ in PdD_x) of D into PdD take place. It is also worthwhile noting that these kinds of conditions are both difficult to obtain (because nonequilibrium chemistry is required) and clearly were not obtained in a large proportion of the early experiments. For this reason, a large majority of early attempts to identify the anomalous heating effect were unsuccessful.

Because of Schwinger's pioneering insight into the theoretical underpinnings of much of modern solid-state physics, nuclear and high-energy physics and statistical physics, it is clear that he had great scientific vision. It is also becoming clear, as a result of the unfolding experimental situation, that this vision may have included the elucidation of a number of the factors responsible for initiating cold fusion-related phenomena. Was Schwinger correct? Time will provide the answer.

References

1. J. Schwinger, *Trans. Fusion Technol.* **26**, xiii (1994).
2. See issues of the American Nuclear Society's journal *Fusion Technology* published since 1990.
3. See, for example, *Trans. Fusion Technol.* **26** (1994), which contains the pro-

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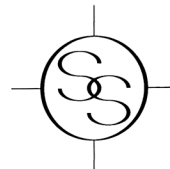
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ceedings of the Fourth International Conference of Cold Fusion; H. Ikegami, ed., *Frontiers of Cold Fusion*, vol. 5, Universal Academy Press, Tokyo (1993), which contains the proceedings of the Third International Conference on Cold Fusion; *The Science of Cold Fusion*, T. Bressani et al., eds., Italian Physical Society, Bologna (1991), issued as volume 33 of the society's conference proceedings; *Anomalous Nuclear Effects in Deuterium/Solid Systems*, S. E. Jones et al., eds., American Institute of Physics, New York (1991) [AIP Conf. Proc. 228].

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Henry Russell Had a Role in US Astronomy, but Not in PT Story

John Lankford and Ricky L. Slavings's article on American astronomy from 1880 to 1940 (January, page 34) is interesting but gives an inadequate summary of the 1910–40 era. It suggests that hack work was basically the order of the day in US universities and observatories.

Although many institutions were still burdened by tedious data acquisition programs during that era, forward-looking academic programs did exist at Princeton and Harvard Universities and Yerkes Observatory before 1940.

One key figure in pre-1940 American astronomy that Lankford and Slavings seem to have overlooked was Henry Norris Russell. His monumental contributions included the theory of Russell-Saunders coupling in atomic operation; a theory of stellar evolution, in which he introduced the Hertzsprung-Russell diagram; binary star analysis; and methods for quantitative chemical analysis of the Sun and stars. Russell's achievements created an observational foundation for the stellar nucleosynthesis investigations to come. Among Russell's students was Donald H. Menzel, who after a lonely stint at Lick Observatory went to Harvard to establish a graduate program in astrophysics in 1932. Menzel's group labored at the frontiers of atomic spectra and interpretation of the physics of stars and nebulae. Particularly outstanding among his disciples were Leo Goldberg, whose research and administrative prowess did so much to fashion 20th-century American astronomy, and James G. Baker, an eminent optical designer. I too was fortunate to have been one of Menzel's students.

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LANKFORD AND SLAVINGS REPLY:
Lawrence Aller misses the point of our article, which focused on the industrialization, not the overall history, of American astronomy. Far from providing a summary, we looked at a specific problem: the ways in which the production of many forms of astronomical knowledge came to resemble the production of goods and services in other sectors of industrial America.

Nor did we overlook Henry Norris Russell. He simply was not relevant to our story. Russell was arguably the most important astrophysical theorist America produced before World War II. But theoretical work and large-scale data collection are very different social activities.

In short, although Aller's historical references are correct, they have nothing to do with our topic. We were writing as social historians; Aller views the past as an intellectual historian. Although the two perspectives are not incompatible, we did not attempt to synthesize them.

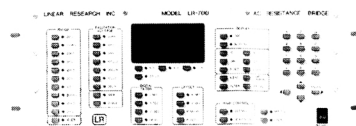
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Phase Diagram Was Out of Sync with Record

Since Newton's time (and even before), there has been a tradition in science of citing relevant antecedents to current research. In these days of millisecond publishing, this tradition regrettably seems to be going out the window. The essentials of the phase diagram for underdoped high- T_c superconductors presented in Barbara Goss Levi's "Search and Discovery" story (June, page 17) were first published in a *Physical Review B* article by me and Masahiko Inui¹ in 1990—well before the 1995 article cited by Levi.

Our article was based on the idea that the lowering of the superconducting transition temperature, T_c , in these underdoped materials is caused by quantum fluctuations of the phase of the superconducting order parameter. Furthermore, based on a quantum generalization of the Ginzburg-Landau phenomenological description of superconductivity, we made an important physical prediction. We showed that, as a result of this "phase winding" mechanism for lowering T_c in the underdoped regime, one should be able to observe infrared-active "phason bands" inside the Bardeen-Cooper-Schrieffer gap for

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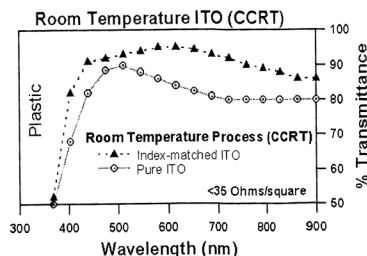
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