Szilard’s Inventions Patently Halted FREE

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The equivalent mathematical underpinnings between time-dependent diffusion and harmonic diffusion-wave equations tend to be glossed over by many researchers who assign to the latter periodic disturbances properties of propagating (hyperbolic) wave fields that properly belong to the former. Corngold's argument that the telegrapher's equation (otherwise known as "second sound,"[5]) is an improvement over the infinitely fast diffusive propagation is correct; however, this has seen little experimental use within the (harmonic) diffusion-wave communities. I guess the main reasons are that the telegrapher's equation is an ad hoc generalization of Fourier's linear diffusion equation, and that there is no real problem in interpreting data by means of simpler diffusion-wave equations satisfying simultaneity rather than causality; and that the time-delayed expressions introduce relaxation times that are short compared to, say, conduction heat transfer times. This large time-scale difference between conductive transport and second-sound-type relaxation time tends to minimize any perceptible differences between instantaneous and time-delayed responses, offering mostly imperceptible exactitude at the expense of additional mathematical complexity.

I agree with Corngold that the introduction of the concept of neutron waves in the 1950s preceded the tremendous growth of diffusion-wave applications in, for example, the photoacoustic and photothermal communities in the last quarter century. Nevertheless, for a primarily experimental field, neutron waves do not seem to have been as important to neutron diffusion science as their more conventional time-resolved counterparts (Corngold's ref. 1 and ref. 2 chap. 4.2). My bias against counting time-resolved diffusion as "diffusion waves" has led me to conclude that only recently has progress occurred in the diffusion-wave area, to which Corngold has objected. I am grateful, however, that he insightfully pointed out the need for more sophisticated analytical approaches to diffusion-wave applications as they spread across many disciplines, such as charge-carrier-wave dynamics and diffuse photon density waves. I agree with his exhortations for cross-fertilization between current diffusion-wave groups and workers in the broader transport physics areas. To date, the opportunity for cross-fertilization remains largely unexplored, exciting, and potentially fruitful territory, especially in the limiting case in which periodic diffusion lengths become commensurate with mean free paths of random microscopic and mesoscopic motion.[6,7]

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Valentine L. Telegdi wonders (Physics Today, October 2000, page 25) why Leo Szilard abandoned his early patent applications on the linear accelerator, cyclotron, betatron and synchrocyclotron. Perhaps, he suggests, Szilard "lost interest in pursuing them," or the patent examiners "may have raised questions of novelty" if they knew the work of Gustaf Ising or others.

This has been one of the mysteries of Szilard's life. Why did he seemingly abandon so many astonishing and career-making inventions? Was it erratic and eccentric behavior, as is usually assumed?

In my talk at the Szilard Centenary in Budapest in 1998,[1] I argued just the opposite—that Szilard was a logical and determined man who has been misjudged. I published an account of his refrigeration inventions with Albert Einstein,[2] and I am grateful to Telegdi for this occasion to discuss Szilard's accelerators.

The German patent examiner's response to Szilard's 1928 application on the linear accelerator still exists. Szilard gave a copy to Einstein, and it is preserved in the Einstein Archives. The examiner rejected the invention as unpatentable with this classic statement:

> Patents can be given only for inventions that permit a commercial use. However, the submitted procedure apparently has only a scientific value. Whether, in accordance with the invention, any commercially useful material can be produced by accelerating artificially-produced positively-charged corpuscles, appears from our present knowledge ruled out. In the whole application, no hint is found that the applicant has produced, or can produce, such material. Obviously the yield would be so tiny, as with atomic disintegration from the natural alpha rays of radioactive substances, that even in the future the prospect of using the invention in commerce has the highest degree of improbability.[3]

Priceless! What was Szilard to do? To prove the patent office wrong, he needed to build the devices. But without a patent, what company would support such a project? Szilard turned to his friend Dennis Gabor, as Szilard recalled in an unpublished letter:

> It was my intention to build some of the machines and I turned over my patent applications to a colleague, Dr. D. Gabor, who at that time was with the Siemens Company and who thought that he might enlist the support of that company for this task. Nothing came of this, however.[4]

Szilard could have stopped there, but he did not. Telegdi notes that in 1934, after fleeing Germany, Szilard filed an application in the UK on betatron and synchrocyclotron designs that were even more sophisticated. Telegdi suggests that this was Szilard's last work on accelerators, but that is not so.

At Oxford University, while searching for an element that might sustain a nuclear chain reaction, Szilard collaborated with James Tuck to build such a betatron. Frederick Lindemann, director of Oxford's Clarendon Laboratory, agreed to fund betatron construction, and plans were moving forward when history intervened. Donald Kerst, who built the first successful betatron, later called the Szilard–Tuck design the "most promising and most complete in technical detail" of early designs. Kerst believed it "would surely have succeeded were it not for the war in Europe."[5]

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Elegance in Crystal Symmetry

In N. David Mermin's column in the March 2000 issue of PHYSICS TODAY (page 11), I tasted the disappointment that the community of crystallographers has not en masse embraced his proposed description of periodic and aperiodic crystals, although it is so elegant. The explanation is simple. Mermin's approach, using only reciprocal (or Fourier) space, was not as new as he claimed. Early in the study of modulated structures, the symmetry was described using irreducible representations of space groups. This is, in fact, a formulation in reciprocal space. The phase factors appearing there are exactly the gauge transformations Mermin discusses.

In contrast, the description that has become standard uses either reciprocal space and three dimensions or direct space and more dimensions. These two formulations are equivalent, but sometimes one is more natural than the other. For example, the positions of atoms or the properties of tilings are more easily discussed in direct space than in reciprocal space. Mermin's approach is very close to the reciprocal space formulation of the standard approach. To apply his approach to the Penrose tiling, one must first calculate the Fourier transform—an unnecessary detour. Therefore, in my opinion, Mermin's approach is certainly elegant, but the standard approach proposed earlier is even more so.

The reason that a serious commission struggles a long time with the nomenclature is simple. The higher-dimensional space groups studied are not used only for electron wavefunctions of aperiodic crystals. Structures, atomic positions, and especially deviations like phason strains are more easily visualized in direct higher-dimensional space, and are described there by space groups. Furthermore, higher-dimensional space groups are relevant not only for physics and crystallography. Quasicrystals have inspired a strong and interesting development in mathematics, for instance, in which problems concerning model sets, diffracting sets, tilings, and other objects are studied using symmetry arguments. Other mathematical topics such as the characterization of Lie groups or spaces of constant curvature use higher-dimensional space groups. Therefore, it is useful to have a nomenclature that satisfies the needs of the different users—mathematicians, physicists, and crystallographers—and that is understood by these groups. Developing such a nomenclature is time-consuming and requires a broad perspective.

I agree with Mermin's statement about the role of elegance in science, but I think that at least one of his examples is not very well chosen.

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MERMIN REPLIES: I'm glad that Ted Janssen sees some elegance in the approach to crystal symmetry that my collaborators and I developed for aperiodic crystals, and I'm pleased to return the compliment to the standard approach of him and his collaborators.

As far as I know, the precise connection was only recently spelled out between our phase (gauge) functions and the rather different phases (factor systems) that appear in the venerable theory of space-group representations.[1] This link offers a more direct and elementary route from crystal symmetry to some of the major physical applications of space-group representations.[2]

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Authors Amend Article on Strontium Ruthenate

Because of an unfortunate miscommunication among the authors, the following corrections were not made to the published version of our article "The Intriguing Superconductivity of Strontium Ruthenate," which appeared in the January 2001 issue of PHYSICS TODAY.

In the cover caption on page 5, the alpha and beta sheets are misidentified: The alpha sheet is orange; the beta is white.

In the last sentence of the first paragraph on page 42, the partially filled d-bands referred to parenthetically should be those of ruthenium or copper ions and not those of strontium or copper ions.

Yoshiteru Maeno's title at Kyoto University is associate professor of physics, not professor of physics. He is also affiliated with the Japan Science and Technology Corp's Core Research for Evolutional Science and Technology program.

On page 44, figure 3 is based on data from S. E. Barrett, *Physics Review B*, volume 41, p. 6283, 1990.

In the figure in the box on page 46, the unit on the axes is the pixel number, not the actual length (each pixel measures 7.5 mm × 7.5 mm).

On page 47, a phrase was omitted from the last sentence of the third paragraph under "Phase-sensitive probes." The sentence should begin: "Josephson tunneling of pairs between Sr_2RuO_4 and conventional superconductors consistent with the proposed *p*-wave state has been reported."

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Correction

December 2000, page 26—The bomb shown in the photograph is a B-61, not a B-83. ■