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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 Masahiho Inui[1] 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 those superconducting samples in which the Cooper pairs form at temperatures well above T_c.

Such infrared absorption bands were subsequently observed by Dirk Van der Marel and collaborators at the University of Groningen and were reported in a paper published in 1995.[2] Their work supported our proposal that it is the phase winding mechanism that leads to the lowering of T_c in the underdoped samples.

Another significant consequence of the phase winding mechanism discussed in our 1990 paper is the concept that phase winding is a manifestation of the tendency of Cooper pairs to localize in this region of the high-T_c phase diagram. This leads to an extraordinary sensitivity of the superconducting phase coherence in these materials to d-band blockers such as zinc, or to other localizing agents such as praesodymium. The existence of localized pairs on the insulating side of the superconductor/insulator phase boundary can explain the anomalously long ranged proximity tunneling observed recently in YBCO/PBCO multilayers. Here, regions of a high-T_c YBCO superconductor are separated by spacers that are driven insulating by replacing yttrium with praesodymium.[3] The praesodymium leads to Cooper pair localization, which is overcome by the injection of mobile Cooper pairs from the adjoining YBCO. The injected pairs can then propagate the phase of the order parameter for hundreds of angstroms through the insulating spacers, thus leading to long-range phase coherence in the multilayer samples.

References

1. S. Doniach, M. Inui, Phys. Rev. B **41**, 6668 (1990).
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Quarternionic QM Updates Provided at ASI Web Site

Readers of David Finkelstein's review of my book *Quarternionic Quantum Mechanics and Quantum Fields* (June, page 58) may want to know that I have set up a page at the Institute for Advanced Study's Web site on which I briefly describe new work on topics discussed in the book and indicate how the relevant papers can be obtained. One such paper, cowritten with Andrew Millard, addresses issues raised in Finkelstein's review concerning the trace action principle. The Web page is at http://www.sns.ias.edu/~adler/Html/quaternionic.html.

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More on Vavilov's Contributions to 20th-century Physics

Sergei Vavilov's nephew Yu. N. Vavilov, writing with B. M. Bolotowsky in your December 1995 issue ("Letters," page 11), pleads that his uncle's role in the discovery of Čerenkov radiation, as well as other scientific achievements, be more fully recognized in the West.

We agree that Sergei Vavilov was a distinguished figure in 20th-century physics, and, in particular, we support the suggestion that the term "Vavilov-Čerenkov radiation" be used more widely in the West.

What Yuri Nikolaievitch and his colleague omitted to say, but may be of interest to your readers, is that Sergei Vavilov was probably the first scientist to observe a nonlinear optic effect. In 1926, with Vadim L. Levshin, he found a reduction in the absorption of light by uranium glass with an increase of intensity of 454 nm light from a high-intensity spark source.[1] And it was Vavilov who introduced the term "nonlinear optics" into the literature, in a passage in his 1950 book *Mikrostruktura sveta* ("The Microstructure of Light").

We include a translation of that passage in our 1995 article entitled "A History of Optics and Optoelectronic Physics in the Twentieth Century,"[2] in which we discuss Vavilov's contributions. In fact, in our discussion of the discovery of Čerenkov radiation, we point out that Vavilov was the author of a 1934 paper,[3] which immediately followed the experimental paper of his student Pavel Čerenkov,[4] in *Doklady Akademii Nauk SSR* in which he correctly postulated fast electrons as the origin of the new phenomenon.

We also describe a brush that Vavilov had with Fritz Weigert in 1922, when Vavilov incorrectly disputed Weigert's claim to have observed polarized fluorescent light at a time when it was universally believed not to be polarized.

We would also like to draw your readers' attention to two useful sets of biographical essays about Vavilov that appeared in *Uspekhi fizicheskikh nauk* in 1973 and 1975.[5] The essays include two by Ilya Frank, a col-