

LETTERS (continued from page 15)

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.

LAWRENCE H. ALLER
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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.

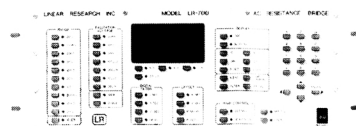
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Phase Diagram Was Out of Sync with Record

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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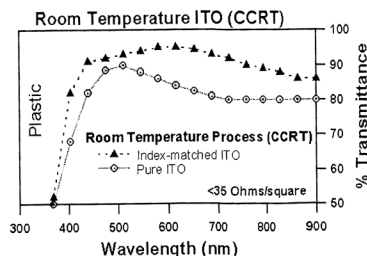
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those superconducting samples in
which the Cooper pairs form at tem-
peratures well above T_c .

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 pro-
posal 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 dis-
cussed in our 1990 paper is the con-
cept that phase winding is a manifes-
tation 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 super-
conducting phase coherence in these
materials to d-band blockers such as
zinc, or to other localizing agents
such as praeosdymium. The exist-
ence of localized pairs on the insulat-
ing side of the superconductor/insulator
phase boundary can explain the
anomalously long ranged proximity
tunneling observed recently in
YBCO/PBCO multilayers. Here, re-
gions of a high- T_c YBCO superconductor
are separated by spacers that are
driven insulating by replacing
yttrium with praeosdymium.³ The
praeosdymium leads to Cooper pair lo-
calization, 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 spac-
ers, thus leading to long-range phase co-
herence in the multilayer samples.

References

1. S. Doniach, M. Inui, Phys. Rev. B **41**, 6668 (1990).
2. J. H. Kim *et al.*, Physica C **247**, 297 (1995).
3. S. Doniach, J. Phys. Chem. Solids **57**, 506 (1996).

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Quarternionic QM Updates Provided at ASI Web Site

Readers of David Finkelstein's re-
view 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 pa-
per, cowritten with Andrew Millard,

addresses issues raised in Finkel-
stein's review concerning the trace ac-
tion 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. S-
vavilov, writing with B. M. Bolo-
towsky in your December 1995 issue
("Letters," page 11), pleads that his
uncle's role in the discovery of Čer-
enkov radiation, as well as other sci-
entific 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 sup-
port 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 Ser-
gei Vavilov was probably the first sci-
entist to observe a nonlinear optic ef-
fect. In 1926, with Vadim L. Levshin,
he found a reduction in the absorp-
tion 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 intro-
duced 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 pas-
sage in our 1995 article entitled "A His-
tory of Optics and Optoelectronic Phys-
ics in the Twentieth Century,"² in
which we discuss Vavilov's contribu-
tions. 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 fol-
lowed the experimental paper of his stu-
dent Pavel Čerenkov,⁴ in *Doklady
Akademii Nauk SSR* in which he cor-
rectly 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 fluo-
rescent 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-