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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.

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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.
Svavilov, 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-

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league of Vavilov's who shared the 1958 Nobel Prize with Čerenkov and Igor Tamm, as well as Vavilov's own reminiscence entitled "Recollections of Physicists." (The essays are available in *Soviet Physics—Uspekhi*, an English-language version of the Russian journal published by the American Institute of Physics.⁶)

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Swann's Way Helped Shape Lawrence's Work on Cyclotron

Liverpool and Berkeley: The Chadwick-Lawrence Letters" by Andrew Brown (May, page 34) includes some interesting but fragmentary comments concerning the construction of Ernest Lawrence's first cyclotron. However, like so many other accounts of the early days of the cyclotron, the article makes it sound as though both Lawrence and the cyclotron emerged from a vacuum. They did not.

When Lawrence was a graduate student in the 1920s, he gained much from having as his mentor a professor of physics named W. F. G. Swann. Formerly at the Carnegie Institution of Washington in Washington, DC, Swann was already teaching at the University of Minnesota in 1923, when Lawrence enrolled there to study for a master's degree.

In the next two years, Lawrence accompanied Swann as a continuing graduate student when Swann became a professor of physics first at the University of Chicago and then at Yale University. Throughout their association, Swann made calculations of the effects of static and oscillating electric and magnetic fields upon the energies and momenta of the cosmic-ray parti-

cles. It was at Yale that Lawrence completed work on his PhD, which included preparing a dissertation on the photoelectric effect in potassium vapor. He stayed on as a research fellow and assistant professor, but then, in 1928, accepted an offer to become an associate professor at the University of California, Berkeley.

From 1946 to 1959, I worked at the Bartol Research Foundation in Swarthmore, Pennsylvania, and had many discussions concerning developments in physics with the foundation's long-time director—Swann. Those conversations became intense between 1954 and 1959, at teatime on Saturday afternoons. Swann was forever seeking fresh viewpoints and new concepts that might lead to advances in physics. Consequently, he often reviewed the procedures that he and Lawrence had developed and he discussed some of the studies they had undertaken back in the 1920s.

For example, he described how they had asked Irving Langmuir, then at General Electric's research laboratory in Schenectady, New York, for information on high-vacuum techniques and lowest attainable pressures. From those data, they had calculated the lengths of mean free paths of particles. The estimates of path lengths were very small in comparison with the distances to be traversed by particles experiencing an appreciable increment of energy in the accelerating electric field. In other words, attainable kinetic energies would be much limited by collisions of the accelerated ions with neutral molecules pervading the entire region of the magnetic field. Consequently, Lawrence and Swann had shelved the idea of creating a particle accelerator.

During my teatime conversations with him in the 1950s, Swann maintained that he had always emphasized to Lawrence that a particle could be brought to a high kinetic energy either by applying one great "push" to the particle, as was later done by Robert J. Van de Graaff, or by supplying many "small pushes," which is what Lawrence did.

Lawrence certainly went to Berkeley in 1928 with the idea of developing the cyclotron very much in mind. And it is clear that the calculations, thinking and discussions he engaged in with Swann must have provided him with a strong foundation and impetus for going forward with the design and construction of his first cyclotron. Swann, on the other hand, turned his attention again specifically to cosmic rays.

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