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.

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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 colleague 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 particles. 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, Pennsylavnia, 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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