

issues; however, they did not come up in our interviews.

The article discusses family-friendly policies at length because we were struck by the disconnect between the attitudes of administrators and the needs of young faculty. If administrators were to see family-friendly policies as recruiting devices rather than as additions to ever-increasing benefits packages, they might recruit and retain a talented, diverse, and very committed faculty.

Child care is certainly of interest to all parents, and many young men have brought the issue up in our interviews. However, female physicists are much more likely than males to be married to other scientists (68% as opposed to 17%). So family-friendly policies or lack thereof are more likely to affect the careers of young women physicists.

I agree with Chris Paulse that much progress has been made for women in physics, but the very low participation by women, especially compared with that in related fields like mathematics and chemistry, remains a puzzle. I have no idea whether women are less disposed to wonder about ohms, carburetors, and quarks; I certainly find them all fascinating. Nor do I know what the “ideal” gender balance in physics might be. I do know, after 40 years in this field, that many women who are interested and talented in physics are driven out by the chilly climate. Their leaving is a loss to the people involved and to the physics community. This loss is what our project is designed to investigate and, if possible, to mitigate. We began our article with statistics because many people find statistics more compelling than anecdotal arguments. For more personal testimony, I suggest Evelyn Fox Keller’s essay “The Anomaly of a Woman in Physics” and “Never Meant to Survive, A Black Woman’s Journey: An Interview with Evelyn Hammonds” by Aimee Sands.¹

Paulse seems to suggest that improving the quantitative skills of our students and attracting more women to the field are mutually exclusive. I do not know of anyone who suggests that the level of mathematical rigor in physics should be lowered to attract more women. There is considerable evidence that the women who drop out of physics do so with as high a performance as the men who stay. See, for example, Mary Fehrs and Roman Czujko’s article (PHYSICS TODAY, August 1992, page 33) and reference 3 of

our article. That evidence indicates that lack of ability is not what causes women to leave physics.

It is my hope that the improvements in climate that we suggest will help more young women continue on to careers in physics.

Reference

1. Both essays appear in M. Wyer, ed., *Women, Science and Technology: A Reader in Feminist Science Studies*, Routledge (January 2001). E. F. Keller is on p. 9; A. Sands, p. 17.

Barbara L. Whitten
(bwhitten@coloradocollege.edu)
Colorado College
Colorado Springs

More on the Value of Ronald Richter’s Work

Having been born and raised in Argentina and educated during the government of Juan Perón, I feel compelled to comment on Friedwardt Winterberg’s letter (PHYSICS TODAY, August 2003, page 12), in which he gives his opinion of Ronald Richter. Winterberg seems to imply that Wolfgang Meckbach, his relative by marriage who was also one-time director of the Bariloche research center, helped him to get a better insight into Richter’s research in Argentina. Unfortunately, Meckbach died in 1998, so we cannot ask his opinion. I know of no written document he may have left on this topic; perhaps Winterberg does.

I met Meckbach when he first arrived in Argentina in the early 1950s, while I was working on my doctoral thesis at the Institute of Physics at the National University of La Plata. Meckbach became an assistant to my thesis adviser, P. H. Brodersen, so he and I had many opportunities to get to know each other. At the time, he knew as much about the Richter affair as everyone else did—rumors. After 1955, Meckbach moved to Bariloche. Several decades later, long after the Richter affair was closed, Meckbach became director at Bariloche. I doubt that he had access to any classified information kept in the archives of the Argentine Atomic Energy Commission—information that may have led him to conclude that Richter’s research there showed some spark of genius.

Another point Winterberg makes is that the Argentinean scientists who reported to the government on Richter’s research in Bariloche asked

advice from the wrong German scientist, Karl Wirtz, codirector of the Max Planck Institute for Physics. Had they asked advice from the right German scientist, Fritz Houtermans, their report on Richter’s research would have been different. I have a few comments about that topic.

On the Web, I found an interesting obituary note for Leopoldo M. Falicov, a brilliant Argentine scientist.¹ It contains a tantalizing reference to the Richter research on Isla Huemul in Bariloche. Also mentioned are the spectacular declarations made in March 1951 by then President Juan Perón, who claimed that Richter had obtained the first experimental confirmation of controlled fusion at Huemul. Those experiments, shrouded in absolute secrecy, were never published, even partially.

The obituary also says that in 1948, the young Richter gained access to Perón and offered him a scheme to achieve, rather simply, controlled nuclear fusion and thus obtain an inexhaustible source of inexpensive energy. Perón had an enormous inclination to believe that any project undertaken by a German scientist would be successful. Due to his political disagreement with true Argentinean scientists of the stature of, for example, Ricardo Gaviola, Perón was reluctant to ask their advice on Richter’s ideas. Instead, he gave Richter a blank check and appointed him as Perón’s personal representative in the Bariloche area. The young Richter burned no less than \$300 million (mid-1950s value) in his “controlled fusion” project.¹

After the fiasco became evident, Perón appointed a technical committee of five, including José Balseiro, a former faculty member at the La Plata Institute of Physics, to report directly to him whether the Richter project should be discontinued. The group worked very hard at the Huemul facilities to reproduce the results that Richter claimed. They analyzed Richter’s so-called “thermonuclear reaction” starting from basic phenomena and concluded that the actual temperature reached in those experiments was much lower than that required to produce a true thermonuclear reaction. They reported these findings to Perón in September 1952. Soon after, the Argentine government discontinued the project.

The Richter affair caused considerable damage to the science and engineering sectors of Argentina’s

higher education system. Not only was the cost enormous, but Richter never even contacted any university in Argentina or admitted a single student into his laboratory. His contribution to developing physics in Argentina was a rather negative one. That affair would never have occurred if the government had initially asked advice from qualified local scientists.

While I was a physics student, I personally heard Richard Gans, director of the National University of La Plata Institute of Physics from the late 1940s through the mid-1950s, say that Richter proposed a thesis, at the German University of Prague, to detect “delta rays” emitted from Earth. Professor Heinrich Rausch von Traubenberg did not agree with the project. The “young genius” went to work somewhere else and graduated in a different field.

Reference

1. M. Cardona, M. I. Cohen, S. G. Louie, <http://cabbib2.cnea.gov.ar/fali/Falicov1.pdf>.

Santos Mayo
(elesan@att.net)
Rockville, Maryland

Winterberg replies: I wrote what Wolfgang Meckbach had told me after he had inspected Richter’s laboratory to see if some of the equipment there could be used for his experiments. He told me what he saw: A magnetically insulated and acoustically after-heated high-temperature electric arc. I do not need, as Mayo suggests, any “secret” unavailable information to recognize that as a credible high-temperature plasma physics experiment. Wound into a closed ring, Richter’s device becomes a tokamak-like configuration.

Richter’s claim to have achieved fusion was wrong, of course, but so was the later, also widely publicized British claim that fusion had been achieved with the Zeta device. Unlike the British team, Richter worked in a vacuum. That makes his ingenuity remarkable, because it was not even known then that plasma physics might provide a road toward controlled fusion; I doubt that any of the Argentinean physicists Mayo names knew much about plasma physics. In Germany around 1952, Otto Hahn, the discoverer of fission, was asked by reporters about the feasibility of fusion. He said that colleagues had told him it was technically impossible. And in America many years earlier, around 1935, at a time when it was already known that stars are

driven by fusion energy, Albert Einstein said the same.

Richter, of course, had grossly underestimated the technical difficulties of achieving controlled fusion, but so had everybody else. According to Mayo, Richter had spent \$300 million, but according to Juan Roederer (PHYSICS TODAY, January 2003, page 32), it was 62 million pesos, or less than \$10 million in US dollars. Regardless which figure is right, the amount is small in comparison to the expenditures for the so far unsuccessful worldwide efforts.

What, according to hearsay, Richter as a young student with little knowledge may have proposed as a PhD thesis topic is irrelevant.

Friedwardt Winterberg
(winterbe@physics.unr.edu)
University of Nevada, Reno

Somewhere Under the Rainbow

I would like to thank E. Blaise Saccocio for the beautiful picture of the double rainbow in the November 2003 issue of PHYSICS TODAY (page