

policymakers is to balance the two.

How will we know when gender barriers in physics undergraduate curricula have been entirely removed? Will that only occur when participation has reached the 50% level for women? Such questions are certainly worth our careful consideration.

Reference

1. C. Holden, *Science* **289**, 380 (2000).

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Barbara Whitten and coauthors point out in their article that family issues play a significant role in career decisions and suggest several steps departments can take to be more family friendly. However, they don't mention the tenure process and its effect on career choices. Although conflicts between family and career occur in nearly all professions, the problem is greatly exacerbated in academia.

To obtain tenure, young professors are often forced to work extremely long hours. Therefore, they have the least time for child rearing during precisely the same years—their late twenties to mid-thirties—when they want to start families. Undergradu-

ate women who perceive that the tenure process will force them to choose between career and family may shy away from academic careers.

Surely part of the problem arises from an overwhelmingly male academic culture that discourages women professors from having children. In my experience, many female science students, both undergraduate and graduate, are acutely aware of the disparity. They see that women professors who start families are labeled as less dedicated than their male counterparts and as having priorities incommensurate with the tenure process. I know a tenure-track female faculty member (not at my university) who worked from home after having a child and was chided by male faculty for “not spending enough time in the office.” Conversely, another female faculty member, who often brought her young child to the office, was described by the same faculty as being “distracted from her work.”

Such punitive attitudes encourage women to delay starting families until after they get tenure—often in their late thirties—even though fertility rates drop and rates of pregnancy complications rise.

I feel little progress will be made in attracting women to science until the tenure process is greatly modified and attitudes about women professors with children change.

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As a woman who has been in atmospheric science for the past 20 years, I was amazed and irritated that the article “What Works for Women in Undergraduate Physics?” concentrated on family-friendly policies and child care. Surely such topics are of concern to all parents, regardless of gender. What happened to equal pay as an issue? It seems that progress in pay equity is as dismal in academia as elsewhere.

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Whitten replies: Our article is a report on the results of a specific project, not a discussion of all issues associated with women in physics. I agree that pay equity and the tenure system are important

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.

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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