Conceiver and First Director of Magnet Lab Was Clearly Lax

On page 60 of your May issue celebrating PHYSICS TODAY's first 50 years, you ran a brief excerpt from a September 1960 story. It announced the founding of a "national magnet research center" at MIT and revealed the important role that Francis Bitter would play in designing and constructing the facility.

As current staff members at what is now the Francis Bitter Magnet Laboratory (so named at his death in 1967), we would like to paraphrase the rest of the 1960 story for your readers to give credit to Benjamin Lax, then head of the solid state division of MIT's Lincoln Laboratory. It was Lax who conceived the idea of the new lab and led the effort to get it funded. Subsequently, he served as the lab's director for its first 21 years.

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More on Chances of Recent Physics PhD's Getting Faculty Slots

Ben Yu-Kuang Hu, in the July issue of PHYSICS TODAY (page 92), suggests that a good problem to put on a physics PhD qualifier is to figure out the odds of securing a faculty appointment. Like many qualifier questions, this one is not as simple as it seems at first glance and may be more challenging to faculty members than to students, who by and large know the score at this point.

Although crude assumptions need to be applied to even the best published statistical data on yearly faculty replacements, the conclusion is essentially the same: The odds are very low for newly minted physics PhD's. Of the approximately 400 US physics department faculty hires in 1995–96, only about 50 from the postdoctoral pool were hired by PhD-granting physics departments for tenure-track positions, and a comparably small number of new PhDs or postdocs were hired by BS- and MS-granting institutions.[1]

The remaining positions were either filled temporarily or filled with more senior people, many of them coming from industry and government laboratories.

By comparison, there were about 1400 new physics PhDs produced in that same academic year, and there were 1000 starting postdocs from physics departments in both the US and other countries. The odds of a postdoc getting a tenure-track position were less than 10%. Interestingly, the odds were no better for those graduating from the most highly rated graduate schools than for the others.

One may quibble about complicating factors such as physicists being hired by nonphysics departments, physicists finding positions abroad, the effects of underreporting and ambiguous reporting and a forthcoming bubble of academic retirees, but it remains very difficult to nudge the odds significantly upward.

With that said, it should be noted that the employment opportunities for young physicists are currently strong for those willing to go outside the academic and basic research arenas. Responses to a survey of the job-market perceptions of young APS members indicate a high level of satisfaction among young physicists who have taken nonacademic positions.[1]

Reference

1. S. Preische, APS News, February 1998, p. 4. The article is also available at http://www.aps.org/apsnews/feb98.html.

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How Gamow Dismayed Los Alamos by Taking on a Nuclear Test

In his review of Joseph Albright and Marcia Kunstel's *Bombshell: The Secret Story of America's Unknown Atomic Spy Conspiracy* (PHYSICS TODAY, September, page 61), Lawrence Badash refers to the "domestic controversy over who played the critical role in the development of the Soviet bomb—the scientists or the spooks. . . ." The following anecdote may contribute to that controversy.

In the summer of 1948, I took two courses at Ohio State University taught by a visiting professor, George W. Gamow. A leading theoretician who had trained in the Soviet Union, Gamov had defected to the West in 1934, but had not had any part in the Manhattan Project. One day he announced there would be no lectures the following week. When he returned to class, he told us that he had gone to Los Alamos to take a test. Unfortunately, he indicated to us, he had passed. The test had been to see whether a Soviet-trained scientist who had access only to unclassified material and the open literature could describe how to build a successful atomic bomb.

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Debate on Estimating Asian Nuclear Test Yields Isn't Artless

I am surprised and dismayed to read in PHYSICS TODAY (July 1998, page 45) that so much ignorance continues to exist about the proper procedures for estimating the yields of the Indian and Pakistani nuclear tests conducted last May. It is especially troubling to read David Albright's incorrect statement that "Determining the yield of a nuclear test from seismic data is an art, not an exact science."

In numerous articles, most particularly one that I cowrote with Gerald Marsh and that was published in this very magazine (August 1987, page 36), I made it perfectly clear that accurate yield estimates can be extracted from seismic data, if only people will take the trouble to do the analysis properly.

A note of mine in *Physics and Society* (October 1998, page 10) explains how to achieve accurate yield estimates of the Indian and Pakistani explosions (India said its largest one was 43 kilotons, Pakistan said its was 18 kt; my estimates are 46 kt and 19 kt, respectively).

Thus, it simply is not true that the Comprehensive Test Ban Treaty is endangered by the inability of the US to make accurate estimates of yields.

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ALBRIGHT REPLIES: I stand by my quote. It was made in the context of the Indian and Pakistani nuclear tests, for which scientific information has been scarce regarding the subsurface geology of the test sites and depth of placement of the explosions. In such situations, skill and expert judgment are critical in estimating the yields of tests and assigning a credible uncertainty range to such estimates. These activities are what I was referring to as "art."

Without meaning to do so, Evernden supports my point. His (mean?) yield estimates for the largest Indian and Pakistani tests are considerably higher than those produced, for exam-