was fired from his position as a professor at Kharkov State University. In protest, Shubnikov immediately withdrew from teaching at the university, where he had been a professor of physics since 1935 and had organized the Soviet Union's first laboratory course in low-temperature physics. Consequently, he and Landau were accused by the Soviet officials of being anti-Soviet, and the NKVD suspected both of them of belonging to a counterrevolutionary conspiracy. Landau then fled from Kharkov and moved to Moscow to join Pyotr Kapitsa's Institute of Physical Problems.

During the summer of 1937, while Stalin's regime of terror was in full force, Shubnikov took a vacation with Landau. On returning to Kharkov on 6 August, though, he was immediately arrested, along with two other UPhTI laboratory heads, and held at the local NKVD prison. Late on the night of 5 October, the NKVD confronted him with Alex Weissberg, an Austrian physicist who had worked at the institute and had been arrested in March.[3] Shubnikov was forced to repeat his "confession"—extorted from him after two months of incessant interrogation and torture—that he had refused to be recruited by Weissberg as a German spy only because he was already a German spy. According to the same confession, his friend Landau was the head of a "counterrevolutionary organization."[4]

When Trapeznikova asked the NKVD about the fate of her husband, she was told that he had been sentenced on 28 October 1937 to "10 years imprisonment without right to correspondence."[1] For two decades, she appealed again and again to the Soviet authorities to review the sentence, but to no avail. Finally, during the Khrushchev era of political thawing, her request was granted: On 11 June 1957, Shubnikov's sentence was quashed by the Supreme Court of the Soviet Union as being unfounded, and Shubnikov was posthumously rehabilitated.[5]

The following month, Alexei Abrikosov presented his now-famous paper on type II superconductors at a meeting in Moscow, and he cited Shubnikov's achievements, becoming the first person to do so in two decades. Nevertheless, except for Shubnikov and Lazarev's observation of nuclear magnetism, Shubnikov's work went unmentioned in the review article "40 Years of Soviet Physics" that appeared in the November 1957 issue of *Uspekhi Fizicheskikh Nauk*, the scientific organ of the Soviet Academy of Sciences. Not until 1966 did the first Soviet acknowledgement of Shubnikov's great contributions appear in print.

What had happened to Shubnikov himself? In 1957, his widow had received a document declaring that he had died in prison on 8 November 1945 as a result of "heart failure"[5] However, it was not until 1991, after she had appealed to the Politburo, the still-existing chief policymaking body of the Communist Party, that she finally learned what had happened to her husband—and when. According to the recently opened archives of the KGB (successor to the NKVD),[5] Lev Shubnikov had been executed by a firing squad on 10 November 1937, three months after being arrested and twelve days after being sentenced to "10 years imprisonment without right to correspondence" (clearly an NKVD euphemism for the death penalty). When he died, he was 36 years old.

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Smithsonian Official Tells Why *Enola Gay* Exhibit Was Shot Down

In his review of Martin Harwit's account of the *Enola Gay* exhibition controversy (PHYSICS TODAY, June, page 79), my former colleague at the University of California, Berkeley, John Heilbron, regrets the decision I made to stop the exhibition that Harwit had planned for the National Air and Space Museum. That decision, which came after months of deliberation, was the toughest I have had to make since taking up my responsibilities as secretary of the Smithsonian Institution. Making it involved the recognition that I was no longer operating solely within the conditions of an academic environment.

It does not surprise me that Heilbron, who has stayed largely within the academy, finds it difficult to understand the particular circumstances and obligations of a public institution in the nation's capital. But I was surprised that Harwit, both as director and author, never understood how far the museums on the Mall are from being a university campus. For many Americans, the proposed exhibition had not only intellectual but also symbolic importance.

Naively handled as they were, Harwit's negotiations led to greater antagonism on all sides and therefore upped the political stakes. By the end, positions had hardened and trust had evaporated. The exhibition had lost much of its potential to inform rather than incite, and the well-being of the Smithsonian Institution itself was at risk. I made the decision I felt I had to make.

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Entrenched Teacher Ponders Sokal Hoax and Student Beliefs

My belated appreciation to Silvan Schweber for his reasoned discussion of the Alan Sokal matter (PHYSICS TODAY, March, page 73). Sokal's action was more than a hoax. It was a deeply damaging attack on the entire basis of intellectual publishing. That basis is the assumption that most of what we read is written for disclosed reasons. Sokal's deliberate breaking of trust should be punished, not celebrated. Steven Weinberg's defense of Sokal in the *New York Review of Books* (NYRB) was shocking, suggesting that the arrogance and ignorance of our spokespeople are profound.

Herewith a view from the science education trenches. Many of the students I teach come to my classes without any of the assumptions that Weinberg and Sokal hold self-evident. Some are fundamentalist Christians and some are Native Americans. Others are just ignorant. If I were to wait until all of my students abandoned their angels and demons, my classes would be canceled. Further, if I debated them, using my expensive East Coast education to humiliate them, again, I'd be out of a job. And rightly so. My job is to teach, not lecture. I am proud of the fact that I've taught radioactive age dating to fundamentalist Baptists. How did they reconcile "Rock of Ages" with the age of rocks? Who cares? They learned the material, and it's not my job, apart from exercising my curiosity, to judge my students' cosmogenic explanations.

The real fiends in this world are the people who make science serve power and money, those who use explanations to debase people, reduce their freedom or exclude them from participation in society. In this sense, I see fault in both fundamentalist religion and big science. And in the end, are explanations even the point? I became a scientist not to explain but to participate in the slow, careful, honest process of observation, the delight in nature for its own sake. As for absolute truth, whether rational or demon-haunted, well . . . I simply have no need of that hypothesis.

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Hysteresis Research Is a Priority Issue

I very much appreciate Bertram Schwarzchild's report (January 1997, page 17) on the recently reported observation of steps in the hysteresis curve of the molecular crystal Mn_{12}. There are, though, some important points I want to make to ensure that certain aspects of this research are clear to the physics community.

To begin, it should be noted that the studies of Mn_{12} made at Grenoble that revealed anomalous behavior in the magnetic relaxation versus applied field (but not hysteresis steps) were begun at the end of 1993 and reported on at the 1994 Conference on Quantum Tunneling of Magnetization in Chichilianne, France,[1] and at the 1994 International Conference on Magnetism in Warsaw.[2] Furthermore, it was the Grenoble researchers who first suspected that thermally activated, resonant quantum tunneling of magnetization might be occurring in Mn_{12}.

All of the experimental groups mentioned in the PHYSICS TODAY report—those at the City College of New York (CCNY), Xerox Corp, the University of Barcelona and the Louis Néel Laboratory of Magnetism in Grenoble—deserve much credit for their fine work. The CCNY group's results on steps in the hysteresis loop of Mn_{12} were published before those of the Grenoble group. However, the report creates the false impression that the work by the Grenoble group followed that of the CCNY group. In fact, the results at Grenoble didn't merely "seem to confirm" the CCNY results presented in 1995. The various groups' research reporting on the hysteresis steps was carried out independently and at around the same time.

In contrast to the CCNY experiments,[3] and also those of the Barcelona group,[4] which used a powdered sample, the Grenoble experiments were done on a single crystal. The consequence is important: The CCNY and Barcelona hysteresis loops are smeared out, as expected for a powder, whereas the Grenoble hysteresis loop[5] shows very well defined steps. I would like to suggest that your readers carefully examine figure 1 in reference 5, which shows the hysteresis curve of a single crystal of Mn_{12}. It is clear that the use of a single crystal sharpens the jumps dramatically and leads to saturation of the magnetization. As a consequence, one can test the actual shape of the hysteresis against theory.

Regarding the physics involved, the fact that steps occur at all values of m (corresponding to the level crossings) indicates that an effective transverse field is responsible for the tunneling. Although the steps occur in the absence of an applied transverse field, the CCNY group has carried out extensive experiments that display a strong dependence of the step height on an applied transverse field. Nevertheless, there are theoretical indications that the molecular spins do not tunnel simply by virtue of the presence of a transverse field that is static: As have Dobrovitski and Zvezdin[6] independently, I have recently presented a theory of hysteresis in Mn_{12} produced by the tunneling of a spin in a swept applied longitudinal field, in the presence of a static transverse field.[7] Using this theory, Bernard Barbara and I have analyzed the hysteresis experiments of Mn_{12} and found that the magnitude of the observed steps is unacceptably greater than that predicted by the dynamics of the theory, even if the transverse field, presumed to be due to intrinsic nuclear spins and neighboring Mn_{12} spins, were taken to be as large as 1000 G. We have concluded that the tunneling requires more complex dynamics.

In closing, I wish to express my agreement with those who believe that spin cluster systems similiar to Mn_{12}, but not Mn_{12} itself, are prototypes for a future molecular computer element that would bring to fruition the possibilities I expressed in my 1990 review article on quantum tunneling of magnetization.[8] May we together enjoy doing physics and share the glory of our achievements.

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SARACHIK REPLIES: Leon Gunther argues that researchers at Grenoble independently codiscovered resonant tunneling of magnetization in Mn_{12}, implying that they should be given equal priority. That is not the case: Jonathan Friedman (then a graduate student at CCNY), working with me and in collaboration with Javier Tejada of the University of Barcelona and Ron Ziolo of Xerox Corp, discovered steps in the hysteresis curve of an oriented-powder sample of Mn_{12} in the early summer of 1995, providing strong evidence for resonant tunneling of the spin. On 1 September, we submitted a paper to the *Journal of Applied Physics*[1] for publication in the proceedings of the 40th Annual Conference on Magnetism and Magnetic Materials; we submitted a similar paper to *Physical Review Letters*[2] on 1 November and reported our results at the MMM conference later that month. The Grenoble/Florence collaboration reported[3] the same phenomenon in single crystals in a paper submitted to *Nature* on 1 March 1996, a full six months after our first submission and well after our report at the MMM conference. As expected, single crystals exhibit sharper steps than powders thereby allowing more precise investigation. The essential physics, however, is precisely the same.

To be sure, the Grenoble researchers had proposed that resonant tunneling was occurring in Mn_{12}. Their astute conjecture was advanced to account for enigmatic behavior they observed near zero field. However, it remained only a conjecture until it was confirmed; we were the first to report a series of resonances at well-defined, equally spaced values of magnetic