

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 Schwarzschild'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 loop⁵ 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 Zvezdin⁶ 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 similar 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 reported³ 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

field, thereby providing unequivocal evidence for mesoscopic quantum tunneling of the magnetization in Mn_{12} .

Regarding the physics, Gunther is quite correct in pointing out that the tunneling must be produced by a transverse field. In our work, we attributed the resonant spin tunneling at a fixed longitudinal field to an internal (dipolar and hyperfine)⁴ or external⁵ static transverse field (see also the theory given in reference 6). Gunther and others invoke a time-dependent longitudinal⁷ or transverse⁸ field to account for the experimental results. This issue will surely be resolved by further investigation.

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Do-It-Yourself Analysis Proposed for NCI's Data on Iodine-131

The September issue of PHYSICS TODAY contains a "Washington Reports" item (page 54) about the iodine-131 exposure resulting from US atmospheric nuclear tests in the 1950s and 1960s. The National Cancer Institute study tabulating this exposure received wide news coverage, so it is good that PHYSICS TODAY presents enough information and World Wide Web links to enable readers to evaluate the validity of the data.

The NCI map of county-by-county per capita doses (reproduced on page 55) presents a *prima facie* case that the study suffers from uncontrolled systematic errors. One can clearly see the state boundaries, with those

between North Dakota and Minnesota, and between Idaho and Oregon, being particularly noticeable.

I surmised that the unusual geographical behavior of the data might have resulted from differing economic practices across state lines (for example, the number of dairy farms), so I looked at figure TS-1 of the NCI technical summary (<http://rex.nci.nih.gov/massmedia/techsum2figts1.html>) showing "activities of I-131 deposited per unit area of ground." These data should not be sensitive to political boundaries, but, in fact, the Idaho-Oregon border shows up even more distinctly than in the dose map.

I am not trying to excuse the unconscionable behavior of the US government during its nuclear testing program. However, some of the specific conclusions of the NCI report, such as the identity of the five counties (all in Idaho and Montana) receiving the highest per capita doses, are suspect. Singling out these counties alarms some people unnecessarily and comforts other people falsely.

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Book Review Fuels Debate on History of NMR Imaging

As the writer of *The Pioneers of NMR and Magnetic Resonance in Medicine: The Story of MRI* (co-authored with the late Merrill Simon), I appreciate Paul Moran's statement in his PHYSICS TODAY review (January, page 66) that "This book is a 'must read' volume for all serious students of NMR in chemistry, physics or bioscience for its early NMR biographies." However, I take strong exception to several of his criticisms.

First, as to Moran's complaint about which pioneers received individual chapters and our alleged slighting of Peter Mansfield, if we had cut the list off at ten instead of nine, Mansfield would have been number 10 overall, number 3 for MRI alone. Even so, his important contributions are pointed out repeatedly in the book.

Second, I was perplexed by Moran's statement that "Mansfield and his colleagues' roles are attenuated by their portrayal as quaint, tea-sipping Upper Midlands academic onlookers—at least some comic relief to those who know the old Nottingham gang." Just in case I had subconsciously used such an inappropriate characterization, I searched the text for

"tea," "quaint," "Upper Midlands" and "onlookers." I found no such depiction—direct or indirect. The portrayal is Moran's.

Third, Moran's conclusions about the Damadian-Lauterbur-Mansfield controversy are also without merit. For example, although Paul Lauterbur failed in his 1973 article on zeugmatography¹ to cite Raymond Damadian's March 1971 article about NMR's usefulness in detecting tumors,² he did cite Damadian's article in his own lab notes (September 1971), in which he first recorded his idea for spatially localizing Damadian's relaxation information. I consider those countersigned notes an important clue as to what led to what in MRI.

I was surprised by Moran's statement that the T1 results reported by Damadian "were seminal for oncology and widely cited for some time, but unfortunately, they did not apply to human cancers." What then are those dark spots on T1 MRI images of humans afflicted with cancer (shown as white spots on the corresponding T2 images)? Incidentally, Damadian's 1970 findings, reported in 1971, dealt with both T1 and T2 relaxation and with both healthy and cancerous tissues.

I also take issue with Moran's statement, "Nor is there any evidence presented that Damadian had the slightest clue about how to actually build an NMR scanner until well after at least a dozen other labs were installing MRI units based on the Lauterbur-Mansfield-Ernst gradient modulation methods." Here, Moran is ignoring the book's lengthy description of Damadian's not-so-clueless building of "Indomitable" (even Mansfield collaborator Peter Morris credits Damadian's work as a "truly remarkable feat"³). Moran also overlooks the very important point that the medical NMR efforts of those other labs were precipitated by Damadian's T1 and T2 findings, and he completely misses the point, made repeatedly in the book, that Damadian did not use the gradient method to achieve the world's first whole-body MRI scan (1977), nor did Damadian use the gradient method in the world's first commercial MRI scanner (1980). Rather, he used successfully the point-by-point method proposed in his 1972 patent application.

One of the important features of the book is its exhaustive referencing. On that basis alone, the diligent soul who wants to search for the truth can do so. I believe a careful prospector will arrive at the same conclusions presented here and in the book.