FREE EXCHANGE ON GREEN BANK VS MAX PLANCK

I read with some amazement the statement by Bernard Burke of MIT quoted in the April 1989 PHYSICS TODAY (page 56): "In fact as an observing complex [the 140-foot and 300-foot telescopes in Green Bank, West Virginia, were] scientifically superior in many respects to the Effelsberg (Bonn) observatory despite the fact that radioastronomers there have a modern 100-meter radiotelescope. People came from Bonn to use the Green Bank facilities."

In response to this, I would comment that over the past ten years, more than 20% of the observing time on the 100-m telescope went to American observers. In fact the majority of very-long-baseline interferometry proposals specifically require the use of the 100-m telescope as part of the network.

International free exchange of observing time on ground-based telescopes is an essential part of astronomy. To quote one half of the truth as an argument for the scientific superiority of the National Radio Astronomy Observatory or of American radioastronomy—whatever the motives are—is not only highly unfair but could turn out to be detrimental to the system of free exchange of observing time. My immediate reaction after I heard of the collapse of the 300-foot telescope was neither a sigh of relief nor a sentiment of *Schadenfreude*: I picked up the phone and offered the director of NRAO the support of the Max Planck Institute for Radioastronomy to help American astronomers get over this critical period until a replacement for the 300-foot telescope is in sight.

International cooperation is too important to the long-range goals of science to jeopardize it by statements aimed at day-to-day politics.

P. G. MEZGER
Max Planck Institute for Radioastronomy
Bonn, FRG
5/89

BURKE REPLIES: It was certainly not my intent to be unfair to the Max Planck Institute for Radioastronomy in my comments, although I can understand P. G. Mezger's protest. I regret any implication of unfairness on my part.

I certainly agree with Mezger that the 100-meter telescope is an essential part of most VLBI programs, my own included. The superior aspects of Green Bank that I had in mind were frequency coverage, superb receivers and the freedom from man-made interference that Green Bank enjoys. The flexibility of having two large instruments, one especially well suited to survey work, was another significant advantage, and is now lost.

Fortunately, a successor instrument in the 100-meter class will be built at Green Bank. The Effelsberg–Green Bank combination, together with several of the other large paraboloids in the world, will give powerful new possibilities for weak-source VLBI, and many of us look forward to future collaboration.

BERNARD F. BURKE
Massachusetts Institute of Technology
Cambridge, Massachusetts
10/89

Radon: Paradigms and Precipitation

Anthony Nero's contribution "Earth, Air, Radon and Home" (April 1989, page 32) is a fine summary of the tremendous progress that has been made in this field during this decade and of the problems remaining. Since leaving the field a few years ago, I have been struck by the extent to which advances were correlated with a "paradigm shift": About 1980–82, building scientists and geologists with their approaches supplanted health physicists in this area. The health physicists had developed measurement tools and inventoried materials as sources. The geologists built on the serendipitous discovery of predominant external sources, and the building scientists examined the mechanisms of radon introduction from the soil.

I believe that this change began when Andreas George of the Depart-

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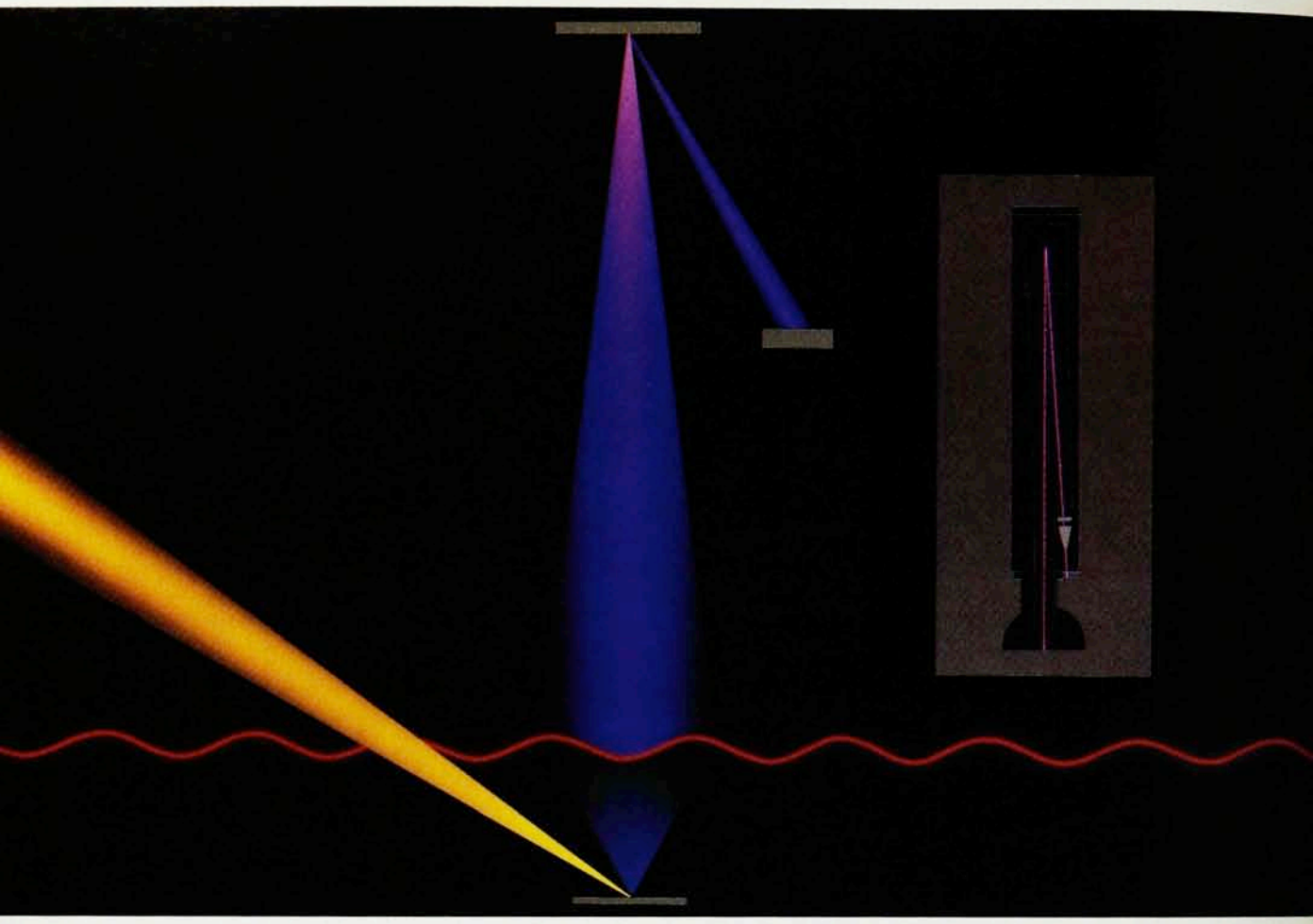

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ment of Energy Environmental Measurements Laboratory first sought a geologist to examine anomalous data gathered by Pennsylvania Power and Light Company in a 1980–81 study of employee houses. An analysis of these data that strongly suggested the role of geology in explaining patterns of high values was presented at the International Indoor Air Pollution Symposium and later published by the Princeton group.[1] Nero alludes to but does not cite this and later reports. Statistical, geochemical and structural geological tools are being fruitfully applied to the problem by workers at many institutions, as illustrated by a recent symposium[2] (which took place after the deadline for Nero's article).

The need to decouple the building interior from the surrounding soil—a classic buildings science problem—was also discovered in the early 1980s. This work probably began with the engineering consultants (particularly Arthur G. Scott, working for the Canadian Atomic Energy Control Board). Early, limited mitigation and source strength work was also carried out at Princeton.[3]

Clearly radon is an important problem, and these points are not meant to detract from Nero's summary and its call for better data, models and public communications. I hope that he will recall that not all the pioneers are at Lawrence Berkeley Laboratory, although it has more than its share.

References

1. H. M. Sachs, T. L. Hernandez, J. W. Ring, Environment Int. 8, 97 (1982).
2. A. E. Gates, L. C. S. Gunderson, eds., Symp. on Geologic Controls on Radon, Ann. Meeting of Northeastern Section of Geological Society of America, New Brunswick, N. J., in *Abstracts with Programs*, vol. 21, no. 2, Geological Society of America, Boulder, Colo. (1989), p. 48.
3. H. M. Sachs, S. Gross, "Regional (Location) and Building Factors Determinants of Indoor Radon Concentrations in Eastern Pennsylvania," PU/CEES report 146, Princeton University, Princeton, N. J. (1982).

HARVEY M. SACHS
Cranbury, New Jersey

6/89

NERO REPLIES: Harvey Sachs might have noted that the references in my article were primarily *reviews* of important topics, rather than papers from the primary literature, which for the question of geology alone consists of dozens of articles. Not citing all these (and the many hundreds of nongeological articles) in the short reference list of an article for PHYSICS TODAY does not constitute a slight of either his work or that of the tens, and now even hundreds, of scientists who have been working on indoor radon. But he might also recall that even before the Princeton–Environmental Measurements Laboratory project he writes about, several major geologically oriented efforts had begun, principally in Sweden, in Canada (witness the work of Arthur Scott) and in the United States, where, for example, our group at Lawrence Berkeley Laboratory had even hired a geologist specifically to investigate the connection between indoor radon and geological factors. This is not to mention the United States Geological Survey, which has a long history of examining the sources and transport of radon (though not specifically into houses).

There is also a more recent and significant example of forgetfulness that is relevant in an ironic way to Sachs's complaint: Since 1985 members of the geological community have been much more involved than before in the question of indoor radon, but—judging from their papers—one wonders if they have read the substantial earlier literature on indoor radon (including Sachs's first reference). In fact the Geological Society of America (whose abstracts from one meeting Sachs refers to) recently published a "fact sheet" on indoor radon that is anything but, presenting as it does a view of the general problem (and even its geological aspects) that is substantially uninformed and misleading. The same difficulty, unfortunately, also pertains to the Environmental Protection Agency, which has often acted as though it were ignorant of past work. Interest in indoor radon seems to occur in cycles, and each needs a memory of past cycles. The literature should serve to convey this memory, but only works if it's read.

ANTHONY V. NERO JR
Lawrence Berkeley Laboratory
Berkeley, California

10/89

My experience with radon monitoring in a cold climate demands that I respond to the photo on the cover of your April 1989 issue. We do have radon in our area: Recently the University of North Dakota Radon Monitoring Facility (of which I am director) identified a home with 440 picocuries per liter of environmental radon in the master bedroom, and approximately 80% of the homes tested in our area had radon levels greater than 4 pCi/l, so I can speak with some credibility. The description of the cover photo implies that it shows a proper installation, but what about freezing rain entering

continued on page 97

continued from page 15

the open exhaust as the temperature is dropping? Wouldn't an inverted outlet or some type of exhaust cap be appropriate?

GLENN I. LYKKEN
University of North Dakota
5/89 *Grand Forks, North Dakota*

SCOTT CHAPIN (SHOWN ON THE APRIL 1989 COVER) REPLIES: Our main objective in designing radon mitigation systems is to reduce the risk to the occupants of any building of exposure to the effects of radon. Rain caps are installed for the deflection of rain, which may cause a problem in severe cold weather. However, these caps also serve to deflect the concentrated exhausting radon back onto the roof of the structure, to possibly reenter through ridge vents, open skylights or even bedroom windows. The real problem is not water entering from outside the system, but moisture from the ground traveling through these systems. When properly designed and calibrated, a radon exhaust will have enough air velocity to deflect most of the rain that would potentially enter a 2-, 3- or 4-inch pipe. Until someone designs a cap that will allow the radon to dissipate vertically above the house and at the same time deflect rainwater from entering, an uncapped system is the better choice.

SCOTT CHAPIN
Chapin Environmental Inc
10/89 *Johnston, Rhode Island*

How 'Frustration' Set In

It was a challenge to survey the full story of spin glasses for PHYSICS TODAY, and one cannot but admire the visionary breadth of Philip W. Anderson's coverage.

Perhaps two small comments will be useful:

▷ A superficial reading of "Spin Glass V" (July, page 9) has led some people to believe in a rivalry between Phil and me about the paternity of the term "frustration." I am happy to report that there is not any dispute. Here is an extract from a text I wrote ten years ago[1]: "One last anecdote: In the summer of 1976, I attended a lecture by P. W. Anderson on spin glasses in the patio of the Aspen Center of Physics; I was not actively working in this field, things appeared pretty esoteric to me, but one sentence which remained as graffiti in one corner of the blackboard struck my imagination: 'The name of the game is frustration.' When I looked for a word which would evoke both the effect of contradiction and an analogy with percolation, it came back to my mind."

▷ In all fairness, the names of Jean Vannimenus and Marc Mézard should have also appeared in the list of the main Paris–Rome actors.

Reference

1. G. Toulouse, *Modern Trends in the Theory of Condensed Matter* (Springer Lecture Notes in Physics, vol. 115), Springer-Verlag, New York (1980).

GÉRARD TOULOUSE
Ecole Normale Supérieure
10/89 *Paris, France*

Prizes and Their Problems

You really must do something about this chap David Mermin. First he has the gall to write a column suggesting—on the basis of the spelling of "Lagrangian"—that no one reads the journals. And now in the January 1989 PHYSICS TODAY (page 9) he has the nerve to suggest that we should forgo the true purposes of existence (such as nominating people for prizes, writing letters supporting nominations and sitting on committees to review them) just to get back to doing physics.

The fact is that what Mermin, bless him, is doing with sarcasm and wit needs to be attended to with great seriousness and clear-headed analysis by the scientific community. Out of my own dark night of the soul of a science administrator I have studied and had my students in my science policy classes analyze similar problems. Here is my list of the most serious problems facing science (at least American science) that we brush under the rug. They all deal with work time.

▷ The "literature" has become meaningless as a usable resource. I remember Peter Debye saying at a conference on *Chemical Abstracts*, when asked what he does about the literature, "I just [pronounced *chust*] ignore it." He explained that he knew all the top labs and kept up with their work: To do a thorough literature job would take away from doing his own science. That was 20–25 years ago. Today I *feel* the same way—but I know better. New work is coming out all over the world, and the paper count is growing exponentially. I cannot read more than a tiny, tiny fraction of it. The citation business must become more and more meaningless because we cite a small circle of friends (and ourselves of course) and innocently (perhaps) ignore most foreign and all