

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
Grand Forks, North Dakota

5/89

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
Johnston, Rhode Island

10/89

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 ago¹: "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
Paris, France

10/89

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

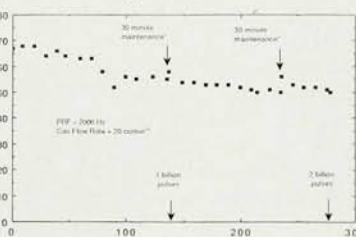
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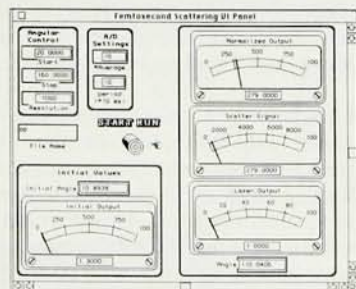
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other literature more than a few years old. We are *not* standing on the shoulders of giants the way earlier generations of scientists did. Can we help this situation? Sure, but we won't. Years ago Conyers Herring called for the obvious: We must pay and reward good scientists with perspective to pull together magisterial reviews. That cannot of course compensate for the ignorance explosion that has engulfed us. We are condemned by our finite brains and finite time to know a smaller and smaller fraction of the relevant science that is known.

▷ The present US system of disbursing research money must go. Its effect is the same as the one above: to waste one-quarter to one-third of the time of our most active, productive scientists. No one contests this fact about this bloodletting against US science; but not enough protest, some even claiming that there is no alternative. I have proposed a system of systematic peer review of *performance* (not of essays called proposals) based on students educated, papers published and so forth.¹ It yields a much more humane distribution of funds: Everybody who is "playing" wins something. It has no step-function discontinuities, it requires nearly zero time for preparing and reviewing, it encourages innovation and risk taking, and finally it would save not less than 25% of the net time of scientists.

▷ The third source of time loss is the meetings racket. I have plotted the person-days spent at meetings by the faculty of our lab, and it has an ominous exponential shape. When are we supposed to work? Pity you if you are in a "hot" field—superconductors, diamond films, what have you—there's a meeting a week. And as Mermin wisely observes, attendance is thrust upon one by the "competition" and a defensive strategy. This growth too must and will end. Again only ameliorative strategies are available, but in the end science will lose out to bureaucracy.

Each of us must be inventive about ways to conserve time. I am currently diverting most invitations asking me to travel to present seminars on my research to the alternative of using a satellite-TV link with interactive questions and answers. It doesn't make sense to travel 1.5 days for 2-3 hours of talk and contacts. The cost is also less. Next I hope to introduce this approach into professional society meetings—starting probably with some sessions at the Materials Research Society meeting this fall. The meeting or particular sessions

could be picked up for no more than registration fees, saving travel money, but most obviously time.

On second thought, don't fire Mermin; give him the first Mermin Award for Good Sense. I am trying to locate 23 colleagues to write letters supporting him.

Reference

1. R. Roy, *Minerva* **22**, 316 (1985); *Sci. Technol. Human Values* **10**, 73 (1985).
RUSTUM ROY
Pennsylvania State University
1/89 *University Park, Pennsylvania*

As David Mermin points out, any original apparent fairness in the system of prizes, honors and awards in physics has been overwhelmed by the practice (which I refer to as "teaming") in which these prizes are systematically sought after, sometimes individually but more often by groups of scientists working in collusion (spoken or unspoken) in organized campaigns that routinely consume phenomenal amounts of time and effort. This type of effort benefits and continues to benefit those in collusion; and once they start down the path, the process itself tends to acquire a kind of momentum, in which each success leads to a more frenzied, albeit usually covert, further teaming effort for the more prestigious prizes, honors and awards. Meantime, those in the profession who have not participated in this manifestly unfair but widespread activity are left out in the cold and are rarely recognized with honors. This process clearly contributes to the prevalence and magnitude of the so-called Matthew effect in science, which "consists in the accruing of greater increments of recognition for particular scientific contributions to scientists of considerable repute and the withholding of such recognition from scientists who have not yet made their mark."¹

Women physicists are among those physicists whose careers have suffered the most damage. This is at least in part because women physicists still are more isolated from their colleagues in the practice of "office politics" and typically are not included or are underrepresented in the informal networks that support the teaming phenomenon, as well as being neglected in teaming efforts conducted by formal structures such as departments and divisions. (The opportunities for saluting women scientists' achievements are often neglected because they, and other outsiders, are not part of the "game plan" of these teaming groups—it's to nobody's advantage but the women's if

they are honored.) Partly as a consequence of this, we still find that women physicists are underrepresented at the podium and that too few women scientists receive merited prizes, honors and awards (apart from specialized honors like the American Chemical Society's Garvan Medal). There is also a pattern of omission of women physicists from fellowship in The American Physical Society.

What are we going to do about this situation that Mermin has had the courage to speak of? Mermin has suggested that we relieve ourselves of this glory game. In principle I concur with him, and in practice I agree that it will not happen. Hans Selye, the physiologist and pioneer in stress research, has written that scientists are motivated not by fortune but by fame.² We work for approval and applause from fellow scientists. There is too long a tradition of honors and prizes, and it is too deeply ingrained in ourselves and our society. It starts early and answers human needs. I would suggest that a more moderate approach, and one with a greater hope of success, would be to try to curb the more frenzied excesses in pursuit of scientific honors and make every effort to extend recognition more equitably within the scientific community.

References

1. V. Kistiakowsky, *PHYSICS TODAY*, February 1980, p. 32.
2. M. Saffran, *The Scientist*, 25 July 1988, p. 14.

CAROLINE L. HERZENBERG
Argonne National Laboratory
2/89 *Argonne, Illinois*

I was more than inspired by N. David Mermin's Reference Frame article of January 1989. Mermin's exhortation that we must speak provocative truths with little or no regard for our professional reputations was most compelling. In this regard, I must speak out on an issue that pervades every aspect of what physicists do, yet is rarely mentioned anywhere in our formal education. This issue is the role of bias and subjectivity in everyday, blue-collar physics.

Before expounding on this subject, one first has to ponder the question, Why don't graduate students (of which I am one) raise issues such as this more often? To answer this question, one must consider that there are two categories of graduate students: those who have taken their qualifying exams and those who have not. Those who haven't can hardly afford to criticize workaday physicists (such as the professors at their insti-

tutions); aside from the obvious explanation, it would just plain be rude.

Those graduate students who have taken their qualifiers can't criticize physicists either—that is, of course, unless they have no intention of trying after graduation to obtain a position where other physicists are present, such as in academia or industry. At this point, the reader may be wondering whence the author himself speaks, but I prefer to keep my status undetermined. Unfortunately, this does not mean I'm not in trouble anyway.

(Allow me the opportunity to note that the prequalifier grad student exists in a state that is a linear combination of a master's program and a doctorate program; it is only the process of measurement that throws the student into one eigenstate or the other. This is one of the best examples of quantum indeterminacy at the social level that I have ever found. But I digress.)

Having thus shot myself right in the CV, I may address the problem to which I alluded several lines above. It is the problem of letting irrelevant data influence a scientific decision. Mermin highlights the problem when he states that he doesn't want to reveal whether he is an award-winner or not, for fear that this will taint the reader's assessment of him and his proposal. This raises the question, Would physicists judge his ideas differently if they knew whether he had won a prize or not? Is there anything regarding Mermin himself that forms a valid consideration? Would physicists be influenced in the least if they knew that there was a four-by-six-foot shrine to Mermin near the entrance to Clark Hall at Cornell University, and that the devout of the physics community regularly paid homage before it? These are difficult questions.

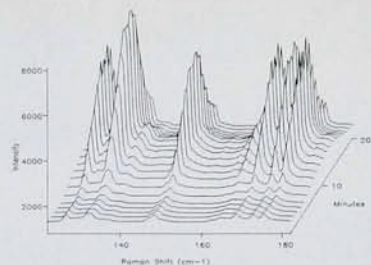
Of course, the matter can take more serious forms. In *QED*, Richard P. Feynman relates the shameful history of the measurement of the electronic charge, and how experiments were judged to be in error not based on analysis of technique but for failing to agree with Robert A. Millikan and Harvey Fletcher's value. Of course, it's important to consider past experiments and explain discrepancies, but to let prior experiments influence when an effort may be deemed a success is not a part of the scientific method. This illustrates how our biases and expectations can cloud our thinking.

Because we are human, it may not be possible to prevent bias and prejudice from tainting our scientific judgment, but we can certainly work to

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keep it at a minimum. I envision a world where Mermin is free at last to proclaim from the rooftops his status as a prizewinner.

JOHN MEVISSIN
Cornell University
Ithaca, New York

4/89

I was contemplating a request for nominations for a prize in low-temperature physics when a colleague drew my attention to N. David Mermin's Reference Frame column "What's Wrong with These Prizes?" The column was a good start on a serious problem, but I hope that Mermin was not implying that refereeing on grants and publications is being carried out with any more integrity. Gang warfare is what the physics community seems to prefer to the actual study of nature's laws.

However, the source of the complaint reminds me of the beloved comedian W. C. Fields. It seems that a friend found Fields in a hospital room reading a Bible. The visitor inquired as to whether Fields's newly found religious virtue was a result of illness. Fields replied, "Just looking for loopholes."

ALLAN WIDOM
Northeastern University
Boston, Massachusetts

1/89

N. David Mermin's criticism of the multiplicity of physics prizes reminds me of a comment on the state of the German army during the last weeks of World War I. To keep up the flagging morale so many medals were being dispensed that it was said that the only way to avoid a Knight's Cross was suicide.

GEORGE WALLERSTEIN
University of Washington
Seattle, Washington

2/89

A-Bomb History in The Making

Richard Rhodes's award-winning book *The Making of the Atomic Bomb* does not really need defending, but I object to Barton Bernstein's use of the book review pages of *PHYSICS TODAY* (December 1988, page 118) to further his distorted view of the events leading up to the use of atomic bombs on Japan in 1945.

Bernstein was obviously upset that Rhodes chose not to cast the chief policymakers of the US wartime government in a sufficiently evil light. "Had Rhodes examined the various files of the Joint Chiefs of Staff," Bernstein writes, "he would also have discovered that American military planners in the summer of 1945 did

not estimate that the invasion of Japan would kill a half-million Americans (as Truman and others later claimed); rather, only about 20 000 to 46 000 fatalities were anticipated. But such pre-Hiroshima military estimates could not deter the use of the A-bomb, because it was deemed a legitimate weapon against hated enemies, who were also 'yellow'—no policymaker wanted to risk even a few thousand American lives to try to save many more Japanese lives."

Suppose that the invasion would have cost only (!) about 20 000 to 46 000 American lives, meaning at least five times that number in total casualties. Does Bernstein actually believe that there were many people, in those days of an all-out war against a hated foe, who would have been willing to forgo use of the bomb, preferring instead to let their sons, husbands and fathers pay the resultant price? It is easy to imagine the feelings of the sons, husbands and fathers themselves, who were waiting for redeployment in the US, the Pacific theater and Europe. (My being among the European contingent may just have colored my opinions.)

Yes, the enemy was "yellow," as were the peoples of Asia who were enslaved by the Japanese and were awaiting liberation. To therefore infer, as the revisionists so often do, that race difference was a significant factor in the decision to use the bomb on Japan (but not on Germany, if it had been ready earlier) severely underestimates the depth of our country's feelings toward both enemies, feelings that our government fortunately appreciated. Government leaders also appreciated the dangers involved in any invasion of the home islands of Japan.

The plans for such an invasion are described in a 24-page booklet, "Top Secret: The Story of the Invasion of Japan" (Ranger Publications, Omaha, 1985). The facts are there: 28 000 000 Japanese had become a part of the "National Volunteer Combat Force," inflamed by a national slogan, "One hundred million will die for the Emperor and nation." Trained for beach defense and guerrilla warfare, they would have been reinforced by 300 suicide submarines, 4000 motorboats and 12 000 planes, many in poor shape, but usable for almost impossible-to-stop land-based short-range suicide attacks.

Based on previous Japanese actions during their defense of their earlier conquests, is there any reason to doubt the validity of such a scenario? Bernstein must believe there is; how else can he subscribe to his *mea culpa*