courages people from writing out their talks and then reading them, with which I fully agree. For those who are not native English speakers, however, it is a good idea to write out the talk—and then throw it away. That way one has to think over the grammatical structures, synonyms and so on and saves oneself (and the audience) the embarrassment of knowing what to say but not knowing how. Besides, this method also works as a length check. (You quickly get used to converting pages to minutes.)

The credit for this advice goes to Thomas Timusk of McMaster University, to whom I have been grateful ever since.

KATALIN KAMARÁS
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9/91 *Budapest, Hungary*

The article by James C. Garland on advice to physics speakers was so good that I made it required reading for my geology graduate students. I'd add a corollary, however, to his advice "Never, ever speak past your allotted time." If the speaker before you has gone over, the audience will be sympathetic to your plight, but will nevertheless be impatient and resentful just on general grounds, even if you don't compound the previous speaker's error. In other words, you inherit some of the resentment caused by the previous speaker. Therefore if you *really* want to make friends and get an audience behind you, find a way to shorten your talk enough to put the session back on schedule. If your presentation seems slightly shaky as a result, you will still be viewed as so generous and professional that all will be forgiven.

JUDITH TOTMAN PARRISH
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The Mythical 'Golden Age' of Grants

N. David Mermin's Reference Frame column "What's Wrong with Those Grants" (June 1991, page 9) brings back memories. His ideal for the financing of university research is almost identical to how such research was funded by the National Research Council in Canada 30 to 40 years ago.

Then and there you could not pay student support out of your operating grant; instead scholarships, bursaries and postdoctoral fellowships were awarded directly to the recipients in a separate scheme. You could not use your operating grant to travel; there were separately administered travel grants. You could pay for equipment used in research, and expendable materials, naturally, but the university paid for postage, telephone calls and photocopying (as it existed then). There were no summer salaries from grants and no overheads to universities. Grants were awarded by peer review, but at the national, not the local, level. Needless to say, there were other sources of funding for items that did not fit into the "small science" mold.

Also needless to say, things are not the same in Canada now as they were 35 years ago, but have moved in many respects closer to the American system. The problem is this: Does this change represent progress? Or is it a "decline and fall?" Or is David Mermin looking wistfully back at a golden age before "the whole system veered off down the wrong track"—a golden age that never really existed and was never really golden?

J. M. DANIELS
6/91 *Princeton Junction, New Jersey*

Obit Selective on Shockley's Race Ideas

The June 1991 issue of PHYSICS TODAY (page 130) contained an obituary of William Shockley by Morgan Sparks, Lester Hogan and John Linville. Among fulsome praise for Shockley's scientific achievements, the authors mention, in a single paragraph, his "unpopular" views on correlations between race and social performance. They acknowledge that he devoted many years and tremendous effort to discussing these correlations. It is a pity therefore that they did not apprise readers of the nature of his views.

Shockley thought that "Nature has color-coded groups of individuals so that statistically reliable predictions of their adaptibility to intellectually rewarding and effective lives can easily be made and profitably be used by the pragmatic man-in-the-street. An urgent moral issue underlying these considerations is this: If those members of our black community with the least percentage of Caucasian genes are both the most prolific and the least intelligent, then a form of genetic enslavement is the destiny of their next generation."[1] Shockley proposed to counter such a "dysgenic trend" by making welfare payments contingent upon voluntary sterilization. He suggested that "unwed mothers can transmit genetically controlled antisocial behavior traits" and argued that this was the cause of the growth of social problems. He approved of the sterilization of "mental defectives" and did not think that a lesson to be learned from Nazi history is that eugenics is intolerable. These arguments were repeated in public over a period of at least ten years.

Shockley's views had much in common with those of Arthur Jensen, Hans Eysenck and Cyril Burt (later found to have faked his results), all of whom he cited and corresponded with. The ideas of this group were used by conservatives in the 1970s to criticize equal opportunities programs and justify government funding cutbacks. Although the tone and direction of the arguments have changed somewhat, a similar process is occurring today—for instance, under the cover of the "political correctness" issue.

Given Shockley's appalling ideas, as well as their political context then and now, I find it extraordinary that Sparks, Hogan and Linville should simply refer briefly and obliquely to his ideas as "unpopular." I should hope that they were, and remain so.

Reference

1. W. Shockley, Rev. Ed. Res. **41**, 375 (1971).

WILLIAM J. SPENCE
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7/91 *Southampton, England*

An E-Mail Message from Mozart

I just got an e-mail message from Kazan containing, among other things, a request that I forward the following letter to the editor of PHYSICS TODAY:

> In a recent book review (June 1991, page 108) my good friend Philip W. Anderson says, "It is even possible for David Mermin to *complain* about the universe being boring quantum mechanics all the way down (PHYSICS TODAY, November 1990, page 9)." While Mermin does complain a lot, in this case he actually said that the success of quantum mechanics all the way down to where we've got is "a triumph." It was I who expressed disappointment at this state of affairs. Neither of us finds quantum mechanics boring.
>
> May I also take this opportunity to state as emphatically as I can that I am not now nor have I ever been a pseudonym for Neil W. Ashcroft.
>
> William A. Mozart

The author of the letter has been abroad for some time now, trying to raise funds for the SSC. He says the Tatars are wild for Waxahachie but a

little short of hard currency. He hopes, however, to persuade them to contribute rugs.

DAVID MERMIN
Cornell University
10/91 *Ithaca, New York*

Did Heisenberg Misconceive A-Bomb?

I was disappointed to see that Samuel Goudsmit's defenders (May 1991, page 13) have yielded the central point to the campaign being conducted by Mark Walker against Goudsmit's scientific and personal intelligence.

Your correspondents seem to have accepted it as a fact that Werner Heisenberg did understand that any atomic bomb would depend on the scientific principle of a fast-neutron reaction. There is a great deal of direct evidence to the contrary, starting with Heisenberg's original paper of December 1939 that mapped out the strategy of the German uranium project. This paper (now available in *Heisenberg: Gesammelte Werke*, series A, part 2 [Springer-Verlag, 1989]) suggests that Heisenberg's concept of a bomb at this stage was a reactor fueled by pure U-235 that would use an immense mass of the isotope to ensure that there would be enough time (given the length of time that slow-neutron diffusion would require) for a useful enough amount of the U-235 to be turned into energy. It was the vast amount of U-235—tons—that was needed for this reactor–bomb that in fact forestalled the German atomic project. (For a reference to this absurd conception of a critical mass of tons, see R. V. Jones's introduction to the American Institute of Physics reissue of Goudsmit's *Alsos* [Tomash, 1988]: Jones and his colleague Charles Frank actually heard the tapes of Heisenberg's Farm Hall internment conversations in 1945 and affirm that he made out the critical mass to be 4 or 5 tons.)

As to the still classified transcripts of the Farm Hall tape recordings of the German scientists' reactions to the news of Hiroshima in August 1945, all those who have had access to them—including Goudsmit himself, Jones, Frank, Leslie Groves, Paul Rosbaud and Margaret Gowing (the author of the official history *Britain and Atomic Energy 1939–1945* [Macmillan, London, 1964])—are agreed that they bear out the truth of Goudsmit's charge that Heisenberg never did understand during the war the scientific principle of the atomic bomb. Is Walker going to argue that all these people are wrong or blinded by personal losses during the war? I do not believe that Walker can continue to assert blithely that Heisenberg knew what he was doing and that Goudsmit was all at sea. At the very least, the fact that Heisenberg claimed after the war never to have made a calculation of the critical mass of an atomic bomb should ring warning bells about the danger of relying on anything he or other German scientists had to say after Hiroshima about their wartime uranium project.

If it seems farfetched that a physicist of Heisenberg's caliber should have got wrong the essential principle of the bomb, one should recall that in February 1940 hopes for a uranium bomb had been written off in Britain and Otto Frisch—the true inventor of the bomb—had himself so far missed the fast-neutron principle. It was only a brain wave that inspired him in March 1940 to consider a fast-neutron bomb and, along with Rudolf Peierls, calculate that a comparatively small amount of U-235 was needed. In the US this idea was not appreciated until the summer of 1941, as is apparent from the memoirs of Arthur Compton and Mark Oliphant and from much other documentation. Even Niels Bohr himself believed that a fast-neutron bomb would entail a large mass of U-235—that is, until his arrival in London and briefing by the British on 8 October 1943.

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WALKER REPLIES: I deeply regret that Paul Lawrence Rose has chosen to follow Jonothan Logan's and Max Dresden's lead by misconstruing gravely my words and intent. Thus I feel compelled to reiterate: I have never written or said that Samuel Goudsmit's claims should be rejected *because* he had suffered at the hands of Germans and therefore was no longer objective. Goudsmit's claims are false because of *evidence* to the contrary of those claims.

The report Werner Heisenberg wrote in 1939 is a good source for his understanding of the problem at that time, but people can change their minds. Subsequent documents such as "Die theoretischen Grundlagen für die Energiegewinnung aus der Uranspaltung" (26 February 1942), which is also reprinted in Heisenberg's *Gesammelte Werke*, prove that Heisenberg knew that both uranium-235 and plutonium could be used as nuclear explosives and that these nuclear explosives (and thereby nuclear weapons) used fast-neutron chain reactions. As far as the critical mass is concerned, page 13 of the comprehensive German Army Ordance report of February 1942, "Energiegewinnung aus Uran" (from Erich Bagge's private papers, Kiel), written by Army physicists in consultation with Heisenberg and the other project scientists, proves that the German researchers were working with an estimate of 10–100 kilograms, comparable to the Allied estimates at this time. Heisenberg may have sounded confused at Farm Hall, but that proves little about what he knew or did during the war. Heisenberg and his colleagues knew how to build a bomb in principle by February 1942 at the latest, and it is unlikely that they would have forgotten so soon.

I respectfully suggest that Rose consult my book, especially since I believe he is writing his own book on this subject. If he remains unconvinced I am willing to send him copies of the relevant documents.

MARK WALKER
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10/91 *Schenectady, New York*

Legislating Science Funding Levels

It is sad but increasingly apparent: A scientist has to be politically savvy to be scientifically successful. Leon Lederman's Reference Frame column "The Privilege—and Obligation—of Being a Physicist" (April 1991, page 9) will have received thousands of sympathetic ears, I am sure, but haven't we heard this kind of plea more than once before? I am quite suspicious of the notion that a cohesive political strategy representing the interests of all subfields of physics, to say nothing of the interests of individual institutions, groups and investigators, can be formulated and executed effectively.

I hereby propose a *legal* alternative to Lederman's admirable *political* course. We must keep our message and tactics simple. Start with legislation to mandate that funds are committed to basic science and technology research at a *per capita* rate comparable to that of our major economic competitors (notably Japan). Such a legal course has several advantages. First, it's simple and forceful. Many a politician who might sniff privately at the idea of giving more funds to science may find it politically unwise to object publicly. Second, it is a uniting path of action, around which all disciplines of