winners were principal investigators on individual research grants from the National Science Foundation or Department of Defense agencies or were supported by companies that provided largely unrestricted support for research of the scientist's choosing. During the later period the prizes were typically given to European physicists who had support of the kind that earlier had been available to Americans but was increasingly disappearing from the American scene. To what extent can our failure in this measure of effectiveness be attributed to a change in science policy by the NSF, which under Erich Bloch shifted increasingly to the support of research centers and programs in prespecified areas, at the expense of individual programs?

The NSF might argue that some half of the support it offers is still through individual research grants. However, this level of support has fallen so far behind the growth of the physics community that the nature of the support has automatically, and completely, changed. In my own area of condensed matter physics, which comprises about half of the active physicists in the US, NSF proposals are typically reviewed by five peer referees. A return on a recent proposal of mine of one "excellent," three "very good's" and one "good" placed it far out of the running for funding. In fact, another proposal, with a rating of two "excellent's" and three "very good's," was also not granted funding.

Drawing the line for support so high is of major concern. It would appear, for example, to rule out just the kind of work that led to the recent Nobel Prizes to European physicists. Three sets of European prizewinners in condensed matter physics were Klaus von Klitzing, for the quantum Hall effect; Gerd Binnig and Heinrich Rohrer, for the scanning tunneling microscope; and J. Georg Bednorz and K. Alex Müller, for high-T_c superconductivity. One could certainly expect that each of these programs would have drawn at least one disqualifying "good" from a set of five reviewers prior to its ultimate success. The difficulty is not only that such leading programs might well not be funded in the present climate, but that all physicists are strongly pressured to generate proposals that they hope might avoid *any* critical reviews. The American physicist is driven away from the kinds of projects that earlier brought Nobel Prizes to Americans and now bring them to Europeans.

The research centers, thrusts and topical programs that have replaced these individual projects are programmed mediocrity. The funders can feel that they no longer squander their money on small projects—which were in fact the lifeblood of American physics—but concentrate it in large projects of their own choosing. Can they argue that this approach is providing backup to American industry? Has it made us competitive with industry abroad? The thought would be amusing if it were not so sad.

At the same time that the NSF is draining the ingenuity that earlier characterized American physics, Bell Labs, IBM and other industrial firms, perceiving physical science research as not so relevant to their futures, are reducing their support for it. And with peace in the world, there is increasing pressure to cut the traditional research support by the Department of Defense. Perhaps the problem is soluble. What is required is the reversal of the trend toward large projects and collective centers, and a return to the principal-investigator system. The current and projected funds for the NSF may be enough to restore the vitality of our physics. The peer review system is intact; only the policy at the top level needs revision.

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Germans at Farm Hall Knew Little of A-Bombs

In early 1992, the British government released the top secret transcripts[1] of the surreptitiously recorded conversations of Werner Heisenberg, Carl-Friedrich von Weizsäcker and the other leading German nuclear scientists confined at Farm Hall in England in the period around 6 August 1945—the day on which, at approximately 6 pm, they were apprised by their "host," Major T. H. Rittner, that an atomic bomb had been dropped. Thus it is now possible to compare the contents of the transcripts with the conclusions published by physicist Samuel A. Goudsmit[2] in 1947 and with the contrary opinions put forward by historian Mark Walker in 1989 and thereafter.[3,4] (See Walker's article in PHYSICS TODAY, January 1990, page 52, and the letters to the editor on that article, which include a reply from Walker, in May 1991, page 13.)

Brief reviews skimming the transcripts have appeared already.[4,5] Some contain short quotations from the conversations, ranging over a variety of subjects; others are edited interpretations of the conversations without direct quotations.

I would like to focus on just two central issues:

▷ What was the value of the critical size of the core of the bomb calculated by Heisenberg?

▷ What did the Germans know about plutonium-239?

For each topic I have assembled essentially every relevant comment in the Farm Hall document (which is in English) and transcribed each verbatim. Every one of the statements listed was made after Rittner's announcement.

It took some time before the Germans overcame their initial incredulity and their subsequent conclusion that the announcement was a propaganda stunt. One must keep in mind, therefore, that all of their statements thereafter were made after they slowly realized that a self-sustaining nuclear reaction and an atomic bomb *had indeed been created.*

Before each excerpt below I give the page number and date of the entry in the Farm Hall transcript. The italics are mine.

Critical size

Page 50–51, 6 August

Heisenberg: It's got nothing to do with atoms. . . . All I can suggest is that some dilettante in America who knows very little about it has bluffed them into saying, "If you drop this it has the equivalent of 20 000 tons of high explosive" and in reality doesn't work at all. . . . I don't believe a word of the whole thing.

Weizsäcker: I don't think it has anything to do with uranium.

Heisenberg: I don't believe it has anything to do with uranium, but that it is a chemical thing where they have enormously increased the speed of the reaction and enormously increased the whole explosion.

Page 52–53, 6 August

Heisenberg: I still don't believe a word about the bomb but I may be wrong. I consider it perfectly possible that they have about *10 tons* of enriched uranium but not that they have *10 tons* of pure *uranium-235.*

Otto Hahn: But if they have *30 kilograms* of pure 235, couldn't they make a bomb with it?

Heisenberg: It still wouldn't go off.

Hahn: You used to tell me that one needed *50 kilograms* of 235 in order to do anything. Now you say one needs *2 tons.*

Heisenberg: I wouldn't like to commit myself for the moment. . . . If it has been done with uranium-235, then we should be able to work it out. It just depends upon whether it is done with *50, 500* or *5000 kilograms* and *we don't know the order of magnitude.*

Page 60, 6 August

Heisenberg: About a year ago I heard from the Foreign Office that the Americans had threatened to drop a uranium bomb on Dresden if we didn't surrender soon. At that time I was asked whether I thought it possible, and with complete conviction, I replied "No."

Page 64, 6 August

Hahn: Do you think they would need as much as that [30 kilos]?

Heisenberg: Quite honestly I have never worked it out.

Page 65, 6 August

Heisenberg: I must have a lump . . . [of] *about a ton.* . . . It is conceivable that they could do it with less.

Page 68, 6 August

Paul Harteck: The weight is *200 kilograms*, then it explodes.

Page 69, 6 August

Walther Gerlach: I would really like to know how they have done it.

Page 72, 7 August

Heisenberg: To produce fission in 10^{25} atoms [*4 kg* uranium]. . . .

Page 98, 8 August

Karl Wirtz: I feel sure that the bomb is not big.

Heisenberg: It might be of the order of *400 kilos.*

Page 99, 9 August

Heisenberg: This [calculation] would then come to about *a ton.*

Page 99, 9 August

Heisenberg: Well how have they actually done it? . . . It is a disgrace if we . . . cannot at least work out how they did it.

Page 102, 9 August

Heisenberg [discussing with Harteck how much protactinium would be needed for a fission bomb]: One or *2 kilograms.*

Page 114, 14 August

Gerlach: Do you think they put the graphite in to prevent melting?

Heisenberg: It could be something like that.

Page 115, 14 August

Heisenberg: If I assume the smallest value [of fission cross section], 0.5, I get a critical radius of 13.7 cm [*180 kg*], and if I assume the greatest, 2.5, I get 6.2 cm [*16 kg*].

Thus, even after becoming convinced that an atomic bomb had indeed been achieved, the Germans' calculated critical size for the core encompassed the following range of values: 2 kg, 4 kg, 10 kg, 50 kg, 180 kg, 200 kg, 400 kg, 500 kg, 5000 kg, a ton, 2 tons, 10 tons. The clear conclusion is that stated by Heisenberg himself: "Quite honestly I . . . never worked it out."

Plutonium and its cousins

Page 50, 6 August

Gerlach: Would it be possible that they have got an engine [nuclear pile] running fairly well, that they have had it long enough to separate "93"?

Hahn: I don't believe it.

Page 54, 6 August

Weizsäcker: Do you think it is impossible that they were able to get "93" or "94" out of one or more running engines [nuclear piles]?

Wirtz: I don't think that is very likely.

Hahn: Well, I think we'll bet on Heisenberg's suggestion that it is bluff.

Page 93, 8 August

Kurt Diebner: I don't know why it is easier to produce fission in element 94. I don't know all that.

Page 97, 8 August

Hahn: The fission of ionium was experimentally proved in the Radium Institute at Vienna. . . . Thorium undergoes fission. . . . Ionium is also fissile.

Gerlach: Can one make a bomb with it? Can you make a bomb out of 2 kg ionium?

Hahn: I don't know.

Page 98, 8 August

Bagge: This they call "Pluto." This might be 93.

Heisenberg: I still do not understand what they have done. If they have this element 94, then it could be that this 94 has quite a short mean free path. . . . *We did not have this element. . . . How they have obtained this element is still a mystery.*

Page 99, 9 August

Harteck: I believe it would be technically possible to produce 2 kg of protactinium. . . . For ten such bombs this would mean 50 kilos of radium in three years, which is unbelievable.

Page 100, 9 August

Harteck: This [93] could be the decay product of 23-minute-halflife uranium from which they have made 93.

Heisenberg: If they have made it with a machine [that is, a nuclear pile], then there is the fantastically difficult problem that they have had to carry out chemical processes with this terrifically radioactive material. . . . *I do not believe that the Americans could have done it.*

Page 101–102, 9 August

Heisenberg: I believe it almost more likely that they have done something quite original such as getting out protactinium in quantity from colossal quantities of material. . . . *Perhaps the facts are that they thereby discovered "Pluto." Pluto is a code name. Protactinium also starts with a "P." . . . Perhaps the others have used protactinium; this is almost easier to imagine than all other methods. . . . If one has pure protactinium in considerable quantity, then the whole thing would blow up. . . .*

Page 104, 9 August

Heisenberg: You can of course have luck if you make element 94. . . . perhaps just as many [neutrons] come out in the case of protactinium. . . . Let us assume that they have done it with protactinium, which to me at the moment appears to be the most likely. . . . They would have had to work with 140 000 tons of material. "Pluto" may be a code name. . . . American work [1940–41] appeared to establish that protactinium was fissionable below about 50 000 volts. . . . They might also have thought that they had discovered that it was spontaneously fissionable. . . . There are now three quite clear ways in which they have done it and only three: isotope separation, protactinium and a machine with D_2O and element 94.

Page 107, 13 August

Hahn: Element 93 decays in 2–3 days into 94. They have of course 94. This is obviously plutonium.

It is clear from these excerpts that on 6–9 August 1945, the Germans' knowledge of the properties of the fissile heavy elements was comparable to that of American graduate students in nuclear chemistry or nuclear physics in 1940–41.

For his foreword in *Alsos,*[2] Goudsmit, having listened to the Farm Hall recorded conversations, and having earlier directly examined seized German documents and laboratories and taken part in personal interrogations of German scientists, wrote in 1946: "The plain fact of the matter is that the Germans were nowhere near getting the secret of the atom bomb. . . . They did not yet know how to produce a chain reaction in a uranium pile. They did not know how to produce plutonium." A reading of the Farm Hall transcripts abundantly confirms Goudsmit's assessment rather than Walker's.

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continued on page 135

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4. M. Walker, Nature **359**, 473 (1992).
5. J. Bernstein, New York Review of Books, 13 August 1992, p. 47.

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Tips for Sympathetic Symposium Speakers

I always enjoy Professor Mozart's offbeat observations and tendentious manifestos, as well as the more cautious suggestions of his medium David Mermin, who is lucky to have such an interesting visitor. (We never see any "Mozarts" out here in the Midwest, though I think Elvis occasionally visits Urbana discount stores.) Regarding the Reference Frame discussion on the physics seminar (November 1992, page 9): I have seen enough worthy talks in my few years to know the situation is far from hopeless, yet I couldn't help but recall some of the worst talks I have ever seen.

▷ A specialist in certain highly technical applications of advanced mathematics to solids began his talk portentously: "I'm sure you've all seen hundreds of talks on this topic, so I won't insult your intelligence with a lengthy introduction." The talk was completely incomprehensible to nonspecialists. Members of the audience demonstrated their intelligence by fleeing in droves.

▷ During one seminar, several faculty members in the audience—mind you, we're talking about real professors here, not just us dumb grad students—found one of the speaker's central assertions dubious. He deigned to spend a moment explaining it but then abruptly cut off the discussion, here reconstructed with modest poetic license.

Speaker, responding to question: This elementary point you raise may or may not be valid, but I don't care; I have made great strides, and they alone justify the rest of my presentation.

Listener: It is infinitely more satisfying to understand 0.01% of a seminar than 0.00%.

Speaker: Let us not tarry; I have prepared a large number of transparencies and it is vitally important for me to display every one of them.

▷ A visiting theorist says, "The experimentally relevant case is for $t \approx U$, but I still think the case $t \ll U$ is interesting." He does not explain why. Are we supposed to know? Or is it only interesting to other people studying the same limit?

On the basis of these and other observations I offer my own conclusion: The proliferation of poor-to-mediocre physics talks is the ineluctable consequence of our funding priorities. We reward firstly research, which is often extremely technical and which in any event demands the generation of original results, though "original results" sometimes fail every measure of value other than never having been seen before; secondly, we reward teaching, which is important, though at its worst it merely trains students to churn out "original results"; and following in a distant third place—because we scarcely reward it—is scholarship.

I hesitate to define "scholarship," but it most certainly includes the passing down of knowledge in a manner more critical and skeptical than we associate with the word "teaching," and a more serious and less self-serving discussion of the merits of particular avenues of research than practitioners are capable of providing. These characteristics have nasty implications. A "scholar" might tell you that your application of recondite mathematical methods is diverting but does nothing for our understanding of physical law. He might tell you that your experiment does not add to knowledge simply because it gives new data points. "Scholars" probably get punched in the nose more often than the rest of us—but wouldn't that brighten up a 4:00 pm snoozer?

While we utopians await the complete intellectual overhaul of physics, I heartily endorse one of the "Mozart" ideas: A speaker does not have to explain in paralyzing detail his or her own research accomplishments. When preparing a talk, imagine yourself in the audience. Think of things in your field that they're unlikely to know but would find useful and understandable. My first year at Illinois I delivered two utterly dreadful talks to classmates, after which I developed this rule of thumb: If you're afraid of insulting their intelligence, then the only intelligence you're overestimating is your own.

(I'm trying to assert my own marketability here, so maybe Reference Frame-*doyen* Mermin would be kind enough to name this maxim after me.)

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David Mermin's timely comments and his friend's advice on the present state of colloquia can be compared to Planck's advice to Schrödinger.

In June 1926 Schrödinger was invited to visit Berlin to give a lecture, and he wrote to Planck for advice regarding the level of presentation. Planck's response[1] is still useful as a guide:

> You also ask about the level at which your lecture should best be given, or rather at which it should begin. I would like to propose, in agreement with my colleagues, that you imagine your audience to be students in the upper classes who, therefore, have already had mechanics and geometrical optics, but who have not yet advanced into the higher realms; to whom, therefore, the Hamilton–Jacobi differential equation, *if* they are acquainted with it at all, signifies a difficult result of profound research, deserving of reverence, and not by any means something to be taken for granted. Under no circumstances, however, should you be afraid that any one of us will consider one sentence of yours to be superfluous. For even if the sentence should not be necessary for an understanding of your train of thought, it would always offer the particular interest of seeing what special paths your thought takes and which particular forms your perception favors. For all of us the main point of your lecture will be what you yourself in your letter designated as a general survey of the fundamentals for the purpose of orientation without much calculation and without many individual problems.

Planck then goes on to suggest that Schrödinger give a second lecture, at which time he can go into greater detail.

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Many congratulations to N. David Mermin for his Reference Frame column in the November 1992 issue. As an astronomy graduate student, I can relate very well to many of the points he brought up about the disastrous state of today's physical science colloquia.

Grad students here are "expected" to attend most or all colloquia offered