between military and civilian Federal R&D funds, since we've traditionally had more in the civilian side, as my numbers indicate.

If Schmitt truly wishes to save the important research programs he is concerned about, he should cite the more meaningful numbers.

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SCHMITT REPLIES: My purpose in the article was to find the common ground between the purposes of those who support research and those who do it. There are many other issues—including broader philosophical ones—that also surround research in the US today. The position espoused by Todd Duncan is simply beyond the scope of the issues I dealt with.

As for the comments of Dwight Duston, the data I used are those cited by the Clinton Administration in setting a goal of 50:50 by 1998. If he does not like this characterization of the issue, he should argue with them, not me!

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Global Warming: Which Sky's the Limit?

John Kepros (October 1992, page 142, and January 1994, page 68) suggests detecting increased global greenhouse warming by satellite measurement of atmospheric expansion due to air warming. Although his idea is thought provoking, it is flawed because of a misinterpretation of the nature of the predicted atmospheric warming.

Atmospheric general circulation models, which provide us with estimates of the increased greenhouse warming, consider in their simulations the troposphere (the atmospheric layer from the surface to about 10–15 km) and a portion or all of the stratosphere (the atmospheric layer above the troposphere, which reaches to about 50 km). Since weather processes are confined almost exclusively to the troposphere, it is reasonable to assume that these models consider a sufficient atmospheric depth to resolve the greenhouse climate. Typically the general circulation models have predicted[1] warming of the troposphere and *cooling* of the stratosphere by an even greater amount than the tropospheric warming. This behavior is in contrast to Kepros's assumption of an increased greenhouse warming throughout the depth of the *entire* atmosphere. There is currently debate about the accuracy of these models' predictions. On physical grounds, however, some tropospheric greenhouse warming and stratospheric cooling (as a result of increased long-wave irradiance emitted by the upper atmosphere to space) should be anticipated. Therefore an amplification of the greenhouse effect would lead to volume expansion of the lower atmosphere and, conversely, volume contraction of the stratosphere.

Even if satellites have detected atmospheric expansion, Kepros's suggestion is inapplicable to the real greenhouse situation. The layer involved is between the Earth's surface and the satellite altitude, which is typically several hundreds of kilometers. Any detected expansion of the atmosphere would, in the greenhouse warming scenario, be the net result of warming of at least one layer and cooling of at least one layer. It is likely that any such expansion is solely (or mostly) the result of warming above the stratosphere.[2] Such warming, however, would have essentially no influence on the greenhouse climate. Generally speaking, we can't infer the details of changes in the atmospheric thermal structure, particularly that of the troposphere, from the ideal-gas law and a single measurement (such as the height of the atmosphere's "top"). Stating it mathematically, applying the ideal-gas equation to more than one layer results in fewer equations than unknowns.

Finally, it is worth noting that it takes a considerable period of time to collect enough measurements to detect any climatological trend.

References

1. J. F. B. Mitchell, Rev. Geophys. **27**, 115 (1989). J. T. Houghton, G. J. Jenkins, J. J. Ephramus, eds., *Climate Change*, Cambridge U. P., Cambridge, England (1990).
2. D. King-Hele, *Observing Earth Satellites*, Macmillan, London (1983).

MOTI SEGAL
RODNEY KUBESH
Iowa State University
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As an indication of globally averaged temperature change, John Kepros has suggested determination of atmospheric expansion from satellite-based measurements, and he has estimated the size of this effect. Such an estimate, however, needs to consider the vertical structure of the atmosphere. A possible temperature rise due to a change in carbon dioxide abundance would primarily affect only the region with significant absorption of thermal radiation, the troposphere, where most of the mass and almost all of the water is located. Thus only a region on the order of 10 kilometers in height would be affected, rather than the 480 km used by Kepros, and the corresponding expansion would be much smaller than the 1.488 km he estimates for a mean temperature change from 300 to 301 K. Changes to the much larger upper atmosphere, which is effectively infrared transparent and responds to changes in absorbed incoming radiation as mentioned in the letter of Greg Davidson (May 1993, page 91), would then dominate the proposed measurement. An increased amount of molecules that absorb, and therefore emit, thermal radiation could also have a cooling influence at some heights.

Measurements of tropospheric temperature *are* being made by satellite detection of thermal microwave radiation originating from atmospheric oxygen. A recent report[1] presents results for the last 15 years that seem dominated by short-term and cyclic effects.

Reference

1. J. R. Christy, R. T. McNider, Nature **367**, 325 (1994).

MICHAEL K. KELLY
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KEPROS REPLIES: I am pleased to see these two responses to my previous letters. The authors seem to know many details about atmospheric modeling. Moti Segal and Rodney Kubesh are even aware that the models are controversial: "There is currently debate about the accuracy of these models' predictions." Their arguments as to the superiority of the models they discuss to my ideal-gas model would have carried more weight if they had made an estimate from those models of the effect on the atmosphere of a 1 °C increase in the mean atmospheric temperature. The ideal-gas model, although simple, at least makes a potentially measurable prediction.

I was stimulated to make my calculation for a gaseous volumetric shell surrounding a sphere by a comment (correct or not—see Greg Davidson's letter [May 1993, page 91] and my subsequent exchange with him [January 1994, page 68]) by CBS Radio news that the Hubble Space Telescope's "orbital lifetime" would be shortened due to atmospheric expansion. My model does not concern itself with local temperatures but as-

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sumes an average temperature and then shows the resulting volumetric change in the event of an increase of 1 °C to that average temperature. I chose the height of the shell, 300 miles, or 480 km, because that is typical for many satellites. I have *never* mentioned "satellite-based measurements." I have, however, looked at descriptions of the atmosphere issued by NASA ("US Standard Atmosphere Supplements," 1966 and 1977) and asked if present or future measurements, made under appropriate conditions, could be compared with them. I do hope that the accompanying letters are not implicitly suggesting that the NASA documents are valueless and such a comparison would be of no use. That would be asserting that a very large committee made a terrible mistake and compounded it 11 years later.

What these two letters say is that for a more nearly accurate calculation three shells with three mean temperatures should be considered. As these kind researchers are very familiar with the models they cite, it would be most considerate of them to make what they consider to be correct calculations. As they have more confidence in their models than in the one I created, they might view the task as a straightforward step. They could state the temperatures they assume in the respective zones and use either the ideal-gas law or "corrected" laws as they see appropriate to arrive at results that can be compared with the 1.488-km expansion that my first-approximation model predicts. Perhaps their intermediate-zone calculations would give useful predictions that could be compared with measured data. Thus they could make an immediate and positive contribution to our understanding of a serious problem that potentially threatens our coastal cities. I respectfully submit this as a serious request.

JOHN G. KEPROS
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Was Nazi Know-how Enough for an A-Bomb?

I found Irving Klotz's analysis (October 1993, page 11) of the state of knowledge of the German atomic energy project, as revealed by the Farm Hall documents, to be fascinating. Lothar Nordheim and I, in 1945, examined the German documents that had been captured by the Alsos mission. We concluded that the German understanding of chain reactions moderated by D_2O was equal to ours as far as the lattice spacing and critical size were concerned. However, we missed the fact that the Germans were unaware of the delayed neutrons, which of course determine the kinetics of a slightly supercritical reactor.

I was particularly interested in the Farm Hall discussion of a protactinium bomb, since, around 1943, I had discussed with Fermi the possibility of using protactinium in a bomb. Fermi regarded the idea as far-fetched but did concede that if we were short of plutonium, a small amount of protactinium might be used to bring the total fissile mass up to what was needed for the first bomb.

Finally, some of what Klotz quotes appeared in General Leslie Groves's *Now It Can Be Told* (Harper, 1962; Da Capo paperback, 1983). Groves also includes the following excerpt from the Farm Hall discussions:

> *[Carl-Friedrich von] Weizsäcker*: We didn't do it . . . because all the physicists didn't want to do it on principle. If we had all wanted Germany to win the war we could have succeeded.
> *[Otto] Hahn*: I don't believe that, but I am thankful we didn't succeed.

Heisenberg's statement "Quite honestly I have never worked it out [the critical mass of a bomb]" proves that the German scientists did not take nuclear bombs seriously.

ALVIN M. WEINBERG
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Based on Irving Klotz's list of transcribed Farm Hall statements many readers might accept at face value his conclusion that the Germans knew little of the A-bomb. However, knowledge of certain technical details, at least with regard to the actinide element protactinium, which was mentioned in many of the technical statements, can lead to diametrically opposite conclusions.

Far from being indicative of the Germans' ignorance of A-bomb matters, their repeated references to protactinium in fact display a surprising degree of awareness of the practical aspects of procuring approximately correct quantities of the only naturally occurring element that has a fast fission cross section in the 1.5-barn range,[1] which is about the same as that of plutonium-239.

Protactinium's very low mineralogical abundance—about the same as radium's—is undoubtedly what made the Germans reluctant to believe American claims of an A-bomb. Their obvious ignorance of the nuclear properties of plutonium-239 and uranium-235, on the other hand, was probably due to the former's total absence from the mineral world and the latter's unavailability in an isotopically pure form (in constrast to protactinium, which is 100% isotope 231).

In those early days of the nuclear age protactinium represented a relatively low-tech, albeit long-shot, route to nuclear weapons materials acquisition. Today the vastly increased scale of the worldwide uranium mining industry makes milling plant tailings a practical source of protactinium—either for direct bomb use or as a starter for a fast breeder reactor for production of plutonium-239 or uranium-233. Someone like Saddam Hussein might discover one of these days that the protactinium route is the one that requires the least amount of restricted technology imports. If we misunderstand the meaning of the Farm Hall statements, we may yet pay a dear price for it.

Reference

1. D. I. Garber, R. R. Kinsey, *Neutron Cross Sections, Vol. II: Curves*, 3rd ed., BNL-325, Brookhaven Natl. Lab., Upton, N. Y. (1976). M. Srinivasan *et al.*, Nucl. Sci. Eng., July 1989, p. 295.

JAROSLAV FRANTA
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I would like to contribute to the discussion of the "secrets" of the atomic bomb.

In 1942 or '43, I was in Warsaw, Poland. I remember some of the physics lectures of Mieczyslaw Wolfke, a professor at the technical university I was attending. During one of them he explained the excess mass of low-atomic-number and high-atomic-number elements, and the minimum in between. He quickly mentioned that division of the high-atomic-number elements would result in conversion of mass to large amounts of energy and that studies of this effect were in progress before the war. He also said that if there had been more progress we would not have had the difficulties we were then experiencing (meaning that we would not have lost the September 1939 campaign and would not be occupied by Germany).

After the August 1944 insurrection in Warsaw, I ended up in a POW camp (Stalag 4B Muhlberg/Elbe) in Germany. Among the inmates in the barrack I was in were Leonard Sosnowski, who had been a professor at the University of Warsaw, and a couple of his clandestine students. For