member but also with ours. The censure mentioned in the letter was not for his public statements but rather for his taking complaints outside the department *before* attempting to resolve them within the department. The letter fails to mention that before that censure de Llano had been repeatedly censured by the department and reprimanded by the dean for yet more serious misconduct. That de Llano has also criticized the NDSU administration in public makes it easy to portray the matter as an issue of academic freedom or freedom of speech. It is not.

Some idea of the careful selection of information that appears to have been made available to the letter's authors can be had by examining their statement of de Llano's accomplishments at NDSU. Supposedly he "reformed and revitalized the department during his five-year term" as chair. De Llano was in fact hired as chair in 1985 for a three-year, not five-year, term, at the end of which an election for chair was to be held. That election, however, was canceled with the dean's consent and against the tenured faculty's recommendations. De Llano then continued as chair until the spring of 1990, when a strong and unanimous request initiated by the department faculty resulted in his removal from that position. This removal revitalized the department more than any action of de Llano's had. Indeed, research activity and grantsmanship have increased markedly since that time. De Llano's behavior, however, has grown progressively more disruptive, to the point where we felt compelled to request his dismissal.

As faculty members, we are keenly aware of the importance of tenure for the protection of academic freedom. Since dismissing a tenured professor is an extraordinary measure that must be taken only in extraordinary circumstances, any such action that appears to threaten academic freedom must be examined carefully. However, such examination requires access to *all* the facts, not just those selected by one party.

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Do Pensions Siphon Funds from Research?

Having recently left my position as a research specialist at Ohio State University, I have had the opportunity to learn about the pension plan. There is both an employee and an employer contribution; in my case the employer contribution was paid by Federal grants (NSF, DOE) in the amount of 13.5% of salary, which amounted to tens of thousands of dollars. Despite my having served long enough to qualify for pension vesting, the so-called benefits are so meager that it is financially favorable for me to request the return of the employee contribution (that is, my money), which has been held without interest for up to six years. I won't see a penny of the 13.5% ever.

In fact, it is practically impossible for a postdoc or similar employee of Ohio State University to gain anything from the pension plan. Yet this hefty charge limits available funds and hence salaries.

An additional "benefit" charged to the Federal government was 1.1% of my salary (thousands of dollars after several years), which went to pay for "early retirement." To the best of my knowledge, this money is charged to both faculty and staff members and paid entirely to faculty members. University overhead of 46% is charged on top of these "direct" costs.

Perhaps the general lack of research funds and the low salaries for scientists employed on a short- or not-so-short-term basis are due partly to systematic redirection of those funds to other purposes. I am interested in collecting similar stories (preferably with documentation) to help determine if a formal study is warranted.

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Support for Science: Rationales and Ratios

I very much agree with the main points of Roland W. Schmitt's article "Public Support of Science: Searching for Harmony" (January 1994, page 29). However, Schmitt overlooks one important benefit of science that seems to be missing from most discussions of the rationale for the support of science in the post-cold-war era. Science is important to society not just as a neutral tool to be applied toward meeting societal demands that originate entirely outside of science. It is also important in helping to shape what those demands will be. Clearly a society whose members believe that the Earth is the center of a universe designed specifically as a stage for humankind to prove its worth will set very different goals for itself than a society that believes we occupy a tiny speck in a universe that evolved for billions of years before producing us.

Schmitt's discussion of societal concerns focuses on the need to recognize "what new things we must learn about nature or what pioneering concepts need to be invented to address these concerns." What is omitted is that the "new things we learn about nature" tell us something more about what our concerns should be. If the efforts of our society are to have real significance, we need to do the best we can to base our goals on a true understanding of how the universe is. This, it seems to me, is the most important benefit that science has to offer.

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Roland Schmitt helps perpetuate the myth that the Defense Department owns 60% of US Federal research and development expenditures (roughly \$40 billion of the \$75 billion annual total). Anyone as familiar with the DOD budget as Schmitt is must know that this is a red herring. Misleading ratios are not a satisfactory substitute for logical planning in deciding how much to cut defense R&D in the post-cold-war era.

The \$40 billion figure erroneously cited for the DOD is not just R&D but RDT&E (the T&E denotes "test and evaluation"). The true science and technology portion of the RDT&E total (budget lines 6.1, 6.2 and 6.3a, for the fiscally informed) amounts to about \$8 billion. The rest of the RDT&E budget goes to pay for things like F-22 aircraft development, missile defense deployment, operation of test ranges, military system upgrades and field engineering support.

Thus what a physicist would call R&D is really only about \$8 billion per year. A quick calculation shows this to be only about 18% of what the Feds put into *real* R&D. Included in this \$8 billion is over \$1 billion for basic research, distributed to hundreds of universities by the research organizations of the Army, Navy, Air Force, Advanced Research Projects Agency and Ballistic Missile Defense Organization. There's obviously no magic about attaining a 50:50 split 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
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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).

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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-

continued on page 84