lations, WCP-93, World Meteorological Organization, Geneva (1984); Bull. Am. Meteorol. Soc. **69**, 40 (1988).
4. S. Manabe, R. T. Wetherald, J. Atmos. Sci. **24,** 241 (1967).
5. R. Lindzen, Bull. Am. Meteorol. Soc. **71,** 288, 1465 (1990). A. K. Betts, Bull. Am. Meteorol. Soc. **71,** 1464 (1990).
6. J. T. Houghton, G. J. Jenkins, eds., *Climate Change: The IPCC Scientific Assessment,* Cambridge U. P., New York (1990).
7. R. Kandel, *Our Changing Climate,* McGraw–Hill, New York (1992).
8. R. D. Cess *et al.*, Science **245,** 513 (1989); J. Geophys. Res. **95,** 16 601 (1990).
9. E. Roeckner, U. Schlese, J. Bierkamp, P. Loewe, Nature **329,** 138 (1987). J. F. B. Mitchell, C. A. Senior, W. J. Ingram, Nature **341,** 132 (1989). V. Ramanathan, W. Collins, Nature **351,** 27 (1991).

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TOMKIN REPLIES: So I must defend my no doubt foolhardy back-of-the-envelope estimate of how much global temperatures would fall if the atmosphere had no CO_2!

My figure of 1 °C may well be an underestimate; on the other hand both Henry Charnock and Keith P. Shine and Robert S. Kandel have to stretch to justify Alison Campbell's statement that the temperature decrease would put the mean global temperature "substantially below zero." Both of their no-feedback estimates of the temperature decrease leave the mean global temperature above freezing and have to be boosted by positive-feedback-only scenarios to get it below freezing. This is not very convincing. Their no-feedback estimates of the cooling may also be exaggerated.

Charnock and Shine's estimate that a doubling of CO_2 with no other changes produces a 1.5 °C warming is on the high side. Ramanathan,[1] for example, finds that the no-other-changes response of the surface temperature to doubled CO_2 is an increase of only 0.3 °C. This suggests that Charnock and Shine's 12 °C cooling for removal of CO_2 with no other changes may also be an overestimate.

Kandel quite rightly reprimands me for overlooking the saturation of the 15-micron CO_2 band. It is also the case, however, that water vapor is a non-negligible absorber at all wavelengths of the 15-micron CO_2 band,[2] so removal of CO_2 from the atmosphere would leave it far from being completely transparent at these wavelengths. Kandel's estimate of an 11 °C cooling for removal of CO_2 does not appear to allow for this band overlap and therefore must be treated with reserve. While my estimate of a 1 °C cooling is unlikely to be correct, I stand by it in the sense that it is probably closer to the mark than my colleagues' estimates of 11, 12 or even 17 °C.

As regards other assorted criticisms and comments: CO_2 *is* a minor greenhouse gas in the straightforward sense that it isn't the major greenhouse gas! Nor am I the only person in the world to label it as such. A well-known meteorologist writes: "Of these substances, the most important by far are water vapor and layer clouds. There are also *minor* greenhouse gases like carbon dioxide [emphasis added]."[3]

Kandel's emphasis on the possibility of "*some*" negative feedback is puzzling. Why only some? Negative feedback is the norm in long-lived stable systems such as the Earth's climate; otherwise, they would not be stable. Surely a more likely presumption is the presence not just of some negative feedback, but of more negative feedback than positive. Furthermore, because water has the unique characteristic that at the Earth's surface it is always present in at least one condensed phase as well as in the vapor phase, water vapor, not CO_2, is the key ingredient in the Earth's atmospheric heat engine,[4] and so it is very likely that water vapor, not CO_2, is the key ingredient in this (currently unknown) negative feedback.

The significance of my fellow astronomers' mistaken belief that CO_2 is the major greenhouse gas is not really a matter of astronomers *vis-à-vis* meteorologists. Rather, if most astronomers, whose business it is to know about these things, labor under this illusion, then it must be almost universal among the general public. But the public has a right to know the basic factual background of the global warming debate; after all, it will be the public who will have to pay the potentially enormous costs if legislation is enacted to prevent the alleged threat of catastrophic global warming. At the moment they are not getting these facts as far as water vapor is concerned. I think it is up to all of us—astronomers, meteorologists, oceanographers, physicists and other scientists in the physical sciences—to break through the dominant culture's presentation of the question, which in the US is pretty one-sided, and get out the word on water vapor.

References

1. V. Ramanathan, J. Atmos. Sci. **38**, 918 (1981).
2. C. W. Allen, *Astrophysical Quantities,* Athlone P., London (1973).
3. R. S. Lindzen, Environ. Protection Agency J., March–April 1990, p. 46.
4. J. R. Philip, J. Atmos. Sci. **44**, 1666 (1987).

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Pittsburgh: A Model Center for Galaxies

It is unfortunate that in our article "Computer Models of Colliding Galaxies" (March 1993, page 54) we omitted mention of the Pittsburgh Supercomputing Center, where many of the calculations presented were run. In our experience, PSC and other national supercomputing centers have consistently supported innovative research in computational astrophysics.

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Computational Physics Talks: Tell Techniques

I recently spent two days at the American Physical Society meeting in Seattle, where I listened to presentations chosen on the basis of what might be interesting to me. (I am an applied mathematician with some experience in solid mechanics.) I was very surprised by the attitude taken by many speakers toward scientific computing. In particular, a typical talk would say that the findings were based on the results of computations but say nothing about how the computations were done or about the reliability of the results. There was only one exception, and in it the speaker explained why his numerical algorithm was faster and more reliable than competing methods.

I happen also to have heard two experimental talks, and they provided a marked contrast in the appreciation of the importance of explaining how the results were obtained. These talks started with diagrams and photographs of the equipment, and the results contained error bars. I ask that similar standards be applied in computational physics. Otherwise we have no way to decide disputes that may arise when different computations produce conflicting results.

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