from
Physics Today
February, 1995

How Cold Would We Get Under CO_2-less Skies?

Global warming is frequently portrayed as physically self-evident, and nonspecialist scientists commonly assent to proclamations of the potential dangers. Nevertheless the exchange (December 1993, page 66) between Henry Charnock, Keith P. Shine and Robert S. Kandel, on the one hand, and astronomer Jocelyn Tomkin, on the other, illustrates the confusion surrounding even the most elementary aspects of the problem: in this case, the radiative role of CO_2. To be sure, Tomkin's original remark (December 1992, page 13) that removing all CO_2 from the atmosphere "would lead to a 1 °C decrease in global warming" was overly casual. However, the condescending responses of Charnock and Shine and of Kandel were no less casual and, given the purported expertise of the authors, far more misleading.

As Tomkin correctly notes in his second letter, the issue of the intrinsic importance of CO_2 to the greenhouse effect depends on the degree to which the water vapor continuum (and presumably the infrared absorption by clouds) competes with the absorption bands of CO_2 as well as on the saturation of the CO_2 spectrum. Charnock and Shine skip over the cloud element of the problem by restricting themselves to clear-sky calculations. Moreover, one cannot properly consider the role of water vapor with the simplistic one-dimensional model they invoke. Water vapor in the atmosphere is highly nonlinear in latitude. Focusing on a single middle latitude underestimates the role of the large amount of water vapor in tropical latitudes. Doing so, my graduate student Daniel Kirk-Davidoff and I find, exaggerates the importance of CO_2 by about 30%.

A convenient way to give proper weighting to tropical water vapor is to perform a two-dimensional radiative-dynamic calculation. The simplest way to perform such a calculation is to take the longitudinally averaged distribution $T_0(\theta,z)$ of temperature with latitude θ and altitude z. For radiatively perturbed states let $T = T_0(T,z) + \delta T$. One calculates δT by requiring net incoming solar radiation to be balanced by net outgoing infrared radiation. Kirk-Davidoff and I have performed such calculations using the well-verified radiative code of Ming Da Chou and colleagues.[1] We see that even for clear-sky conditions, the effect of doubling CO_2 from 300 parts per million to 600 ppm (keeping specific humidity constant) is only 0.5 °C, and when 40% cloud cover is included, this warming is reduced to 0.22 °C. Halving CO_2 to 150 ppm produces an effect of the same magnitude but opposite sign. Reducing CO_2 to zero cools the atmosphere by 5.3 °C, assuming no cloud cover, and by 3.53 °C when 40% cloud cover is assumed. These latter numbers are closer to Tomkin's 1 °C guess than to the "authoritative" estimates of Charnock and Shine (12 °C) and Kandel (11 °C).

Our warming for doubled CO_2 is far less than what is commonly given for the no-feedback case in numerical models. The reason for this difference is that in most calculations the bulk of CO_2 warming at the surface arises from the reduction of temperature in the stratosphere that accompanies increased CO_2 (and conversely the increase in stratospheric temperature that accompanies a reduction of CO_2). In the case of increased CO_2 the stratospheric cooling reduces the emission temperature for CO_2, which, in turn, *could* enhance its greenhouse effect at the surface. Our present calculation omits this feedback. Stratospheric cooling *has* been observed, but the expected accompanying surface warming has not been. There are at least two possible explanations. It is possible that the surface response has been delayed by the ocean's heat capacity. It is equally possible that the downward flux from the stratosphere warmed primarily the upper troposphere, leaving the surface relatively unaffected. The last possibility has been ignored in both the present calculations and those of Kandel and of Charnock and Shine.

This is not a trivial point. The present calculations fix the total vertical and meridional profile of temperature, thus eliminating any "feedbacks" resulting from changing structure. The calculations of Charnock and Shine and of Kandel allow temperature structures to change above the tropopause while constraining such changes below the tropopause. In reality, the structure of the temperature profile below the tropopause is also observed to change—and in ways that can offset changes above the tropopause.

The details of the response of δT to the decrease of CO_2 are not without interest. When cloud cover is included, reducing CO_2 from 300 ppm to 19 ppm leads to a cooling of only 1.23 °C; the remainder of the 3.53 °C cooling is associated with this last 19 ppm. Since radiative band models like those of Chou and colleagues or Shine are tuned for present levels of CO_2, it is questionable whether they properly include H_2O absorption at the low levels of absorption associated with 19 ppm of CO_2, and the resulting calculations of the cooling associated with the last 19 ppm of CO_2 are suspect. However, we have not checked this matter in detail.

Kandel's remarks concerning the role of diminished CO_2 during the last glacial, as determined from the Vostok ice core, properly note the problem of causality. (The drop in CO_2 in the ice core appears to be preceded by the drop in temperature.) However, his remarks fail to note how little cooling is produced by a drop in CO_2 to 200 ppm (which is the size of the drop shown by the Vostok ice core for the glacial period).[2] Our calculations indicate that the expected cooling (barring truly astounding positive feedbacks) is only about 0.2 °C.

It is worth noting at this stage that changing gross radiative properties is not likely to be the only or even the main way of changing climate. Past climate change was characterized by profound changes in the equator-to-pole temperature difference while the equatorial temperatures remained relatively unchanged.[3] This pattern of change most likely involves substantial changes in the dynamic heat flux from the tropics to higher latitudes, high-latitude albedo changes or both, rather than gross global changes in radiative forcing.[3,4] That said, it should be noted that our results would not be significantly altered if we allowed for changes in the equator-to-pole temperature distribution.

Kandel's comment that "relative humidity is roughly constant between winter and summer, as one might indeed expect from the Clausius–Clapeyron relation" is hardly logical. The Clausius–Clapeyron relation would certainly be relevant if the atmosphere were saturated. However, the observations Kandel refers to pertain to the relative humidity in the surface turbulent boundary layer, and characteristic relative humidities above that layer are on the order of 30%.[5] Thus while the Clausius–Clapeyron relation may reasonably be used to bound the humidity in the atmosphere, it tells us nothing more about the actual value. To claim otherwise is to claim that a quart mug holds more beer than a pint mug regardless of how much beer is poured into each. It is now known that it is the water vapor above 2 km that dominates the water vapor feedback, and this water vapor is, for the most part, not even reliably measured.[5] Current model predictions of climate sensitivity to doubling of CO_2 in excess of about a degree depend on positive feedback from water vapor above 2 km despite the fact that the physics appropriate to water vapor at these levels is absent from the models—an unsatisfactory situation to say the least.

Tomkin's suspicion that the role of CO_2 has been exaggerated is certainly not without foundation. The suggestions of Charnock and Shine and of Kandel that scientists outside the atmospheric sciences should know better about the "quite straightforward" basic physics of the climate system, given the unstated "stretching" in their remarks, might reasonably be regarded as disingenuous.

References

1. M. D. Chou, D. P. Krats, W. Ridgway, J. Climate 4, 424 (1991).
2. J. M. Barnola, D. Raynaud, Y. S. Korotkevich, C. Lorius, Nature 329, 408 (1987).
3. R. S. Lindzen, Annu. Rev. Fluid Mech. 26, 353 (1993).
4. P. Ya. Groisman, T. R. Karl, R. W. Knight, Science 263, 198 (1994).
5. D. Z. Sun, R. S. Lindzen, J. Atmos. Sci. 50, 1643 (1993).

RICHARD S. LINDZEN
Massachusetts Institute of Technology
Cambridge, Massachusetts

CHARNOCK AND SHINE REPLY: Richard Lindzen observes that our calculations of the effects of the hypothetical removal of all atmospheric CO_2 were simplified. That was made clear in our original letter.

Lindzen will also be aware that results from one-dimensional (height) global mean radiative-convective models like ours are not inconsistent with the global means of results from three-dimensional (latitude, longitude, height) models. The results from the two-dimensional (latitude, height) model that he and Daniel Kirk-Davidoff have developed are particularly interesting because they differ markedly from both. We look forward to reading a fuller account of their work.

HENRY CHARNOCK
University of Southampton
Southampton, UK
KEITH P. SHINE
University of Reading
Reading, UK

KANDEL REPLIES: Richard Lindzen takes a sledgehammer to the back of an envelope in his attack on my "casual and . . . misleading" criticism of Jocelyn Tomkin's letter (December 1992, page 13). This exchange began (more or less) with Alison Campbell's (February 1992, page 123) unrealistic hypothesis of *complete* CO_2 removal and casual estimate of its impact on temperature. My letter (December 1993, page 66) attacked Tomkin for minimizing the role of CO_2 based on a gross underestimate of its radiative forcing, neglecting curve-of-growth saturation effects that should be an astrophysicist's bread and butter. Lindzen casually goes along with the remark in Tomkin's reply (December 1993, page 68) to my letter that my estimate "does not *appear* [my emphasis] to allow for . . . band overlap" between CO_2 and H_2O. But my estimate, which I qualified as "very rough," made use of ICRCCM case studies[1] based on line-by-line models that *explicitly include* this overlap. My interpretation of ICRCCM may be "stretching" things, but even with Lindzen's figures based on band models, cooling for zero CO_2 is at least 250% higher than Tomkin's result. For the real case of CO_2 reduced to 200 parts per million at glacial maximum, saturation effects were of course present, and the contribution to cooling fairly weak though not nil.

Lindzen notes rightly that the Clausius–Clapeyron relation does not tell us what atmospheric relative humidity to expect. I'll offer him a full liter (not a quart) mug of beer on his next visit to Paris. I should have written that if the relative summer–winter invariance of relative-humidity profiles is a fair guide to what can happen in climate change (and it may *not* be, according to recent work here at the CNRS Laboratoire de Meteorologie Dynamique[2]), the Clausius–Clapeyron relation suggests that positive water-vapor feedback is likely.[3] Results of simple climate models are very sensitive to explicit or implicit assumptions,[4] as are those of general circulation models.[5] But my second criticism of Tomkin was his casual ignorance ("It is unclear . . . ") of the fact that most models *do* consider cloud feedbacks, and his bald assumption that cloud feedback *must* be negative, as if one could carry out infrared astronomy under cloudy skies!

In his reply Tomkin almost makes a point, and Lindzen misses it as I did, regarding climate feedbacks. Of course, overall negative feedback must be dominant in a stable system. The trouble is that we in the climate research community have got into the bad habit of calling some reference model result a "no-feedback case." This is wrong: All of these cases include the *basic negative feedback* of radiation. The critical question is whether or not total negative feedback is brought fairly close to zero by positive feedbacks enhancing climate sensitivity. When the late Fritz Möller[6] inadvertently introduced too strong a positive water-vapor feedback in his calculations, he could only avoid runaway by artificially adding a negative feedback. Lindzen has argued[7] that the absence of significant observed warming over the past few decades means that positive feedbacks have been overestimated. Maybe so, although it may also be that *negative* radiative *forcing* by anthropogenic aerosols has been underestimated. Anyone can entertain suspicions that the role of CO_2 increase—or of solar variations—has been exaggerated. But Lindzen's argument gains no credibility from association with Tomkin's know-nothing rhetoric. Readers of PHYSICS TODAY deserve better.

References

1. F. M. Luther, R. G. Ellingson, Y. Fouquart, S. Fels, N. A. Scott, W. J. Wiscombe, *The Intercomparison of Radiation Codes in Climate Models (ICRCCM)—Longwave Clear-Sky Calculations*, WCP-93, World Meteorological Organization, Geneva (1984).
2. J. P. Duvel, R. S. Kandel, S. Bony, H. Le Treut, in *International Radiation Symposium '92*, S. Keevallik, O. Kärner, eds., Deepak, Hampton, Va. (1993), p. 158. S. Bony, J. P. Duvel, J. Geophys. Res. 99, 12963 (1994).
3. M. E. Schlesinger, J. F. B. Mitchell, Rev. Geophys. 25, 764 (1987).
4. R. S. Kandel, Nature 293, 634 (1981).
5. R. D. Cess *et al.*, J. Geophys. Res. 95, 16601 (1990).
6. F. Möller, J. Geophys. Res. 68, 3877 (1963).
7. R. Lindzen, Bull. Am. Meteorol. Soc. 71, 288 (1990).

ROBERT KANDEL
Ecole Polytechnique
Palaiseau, France