Letters

Carbon Dioxide and Climate

We are dismayed by S. B. Idso's claim (Reports, 28 Mar., p. 1462) that he has uncovered "a major discrepancy between current theory and experiment relative to the effects of carbon dioxide on climate," on the basis of which "a serious reconsideration of the whole CO_2-climate problem seems imperative."

Our chief reasons for concern about Idso's report are threefold: (i) the author claims to have made a calculation of the change in mean global surface temperature that would occur with a doubling of CO_2, but his calculation appears inapplicable to the *global* question; (ii) in light of the incomplete and simplified nature of his analysis, we do not find that his answer "casts doubts upon the common result of the many theoretical numerical models" that have been published in the past decade or so; and (iii) we believe it is out of place for a report to appear in a refereed journal of the high caliber of *Science* with a main conclusion based on a pivotal reference still "in preparation." We will explain the first two points; the last needs no further comment.

In his model, Idso considers the increase in downward flux of infrared radiation that would be produced by a doubling of CO_2, taking into account the effect of the added infrared emission from the CO_2 and making rough corrections for the effects of clouds, and then multiplying the increased downward flux by a "surface air temperature response function" to obtain the change in surface air temperature. His results for the increased downward flux directly caused by CO_2 doubling differ from published estimates, for example (*1*), by a factor of about 2 to 3. He claims that his result for a doubling of CO_2 is virtually identical to the value of ≤ 0.25 K obtained by Newell and Dopplick (*1*) and notes that it is about an order of magnitude less than that generally accepted on the basis of a number of much more complete model calculations, which were recently critically reviewed by two well-qualified working groups (*2*).

One should understand what Newell and Dopplick did, why it is not an independent confirmation of Idso, and why neither includes a complete calculation (that is, one that conserves energy) of the climatic response to a CO_2 doubling. A crucial physical process included in most global climatic models (which have inferred a 2 to 3 K increase in mean surface temperature with a doubling of CO_2) is the mutual interaction between surface temperature, surface wetness and evaporation, and air temperature and humidity directly above the surface. Specifically, if an increase in CO_2 were to cause an incremental increase in downward infrared radiative flux arriving at the earth's surface, much of this surface heat input would be returned to the atmosphere as fluxes of latent, sensible, and infrared heat—thereby restraining the immediate *local* surface temperature increase. To this point, the "consensus models" [for example, those discussed in (*2*)] and the studies of Newell and Dopplick are similar.

In the energy conserving global models, however, the increased fluxes of latent, sensible, and infrared heat both warm the atmosphere above the ground and increase the water vapor content. These effects cause the infrared emission of the lower atmosphere to increase, the downward-directed part of which amplifies the initial surface warming. The Newell and Dopplick model (and perhaps Idso's as well) ignores this positive feedback effect *inherent* in energy-conserving models and unrealistically assumes that the heat and water vapor fluxes from the surface are, in effect, lost to space. The simplistic approach also ignores other important interactions, such as the effect of air temperature change on vertical heat transfer. Together, these feedback processes enhance the global response of surface temperature by roughly an order of magnitude.

The calculation by Newell and Dopplick (*1*) was based on the assumption of *no feedback* between surface and free air properties and thus predicted the surface temperature response to an increase in CO_2 with both air temperature and humidity held constant. As Newell and Dopplick stated in their abstract (*1*), their model implies that the latent heat flux from oceans is lost to space instead of warming the atmosphere above the ground, which is unrealistic. Thus, their result cannot be considered as appropriate for a complete CO_2-climate change calculation nor as independent confirmation of Idso's approach.

The temperature response obtained by Idso is not reproducible because crucial details are absent from his report. He claims to have obtained his "response function" from (unspecified) independent observational data. However, his report does not give us assurance that he has attempted to conserve energy on a global basis. We encourage him to provide the full details of his method as soon as possible, so that they can be subjected to the normal scrutiny of the peer review process that should precede the wide dissemination of new scientific results—especially in an area of this kind where there is so much public concern.

Idso argues that "although a consensus in the range of 2 to 4 K is beginning to emerge from [estimates of] a doubling of CO_2 concentration . . . the studies that predict effects in this range are all too similar to be regarded as independent routes to a common solution." We agree that several uncertainties still mark the CO_2-climate problem and have said so in various of our own publications (*3*). However, we believe that the principal uncertainties arise from our still incomplete knowledge of certain long-recognized "climatic feedback processes" (*4*) and from questions of the transient response of the climatic system to the actual time-dependent perturbation represented by CO_2 increases (*2*; *5*, section 5).

We disagree with Idso that his calculations, and those of Newell and Dopplick which he cites as "independent" confirmation of his results, raise the state of the art of climate modeling and permit more reliable estimates of the actual climatic response to CO_2 increases. We are not persuaded that Idso's results should alter our perception of the uncertainties inherent in present estimates of the CO_2 effects on climate beyond the limits already acknowledged by those involved in state of the art climate modeling. Any "serious reconsideration of the whole CO_2-climate problem" does not seem to us to be justified on the basis of what Idso offers in his *Science* report.

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4. *Inadvertent Climate Modification: Report of the Study of Man's Impact on Climate* (MIT Press, Cambridge, Mass., 1971).
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Unfortunately it is not possible to evaluate Idso's argument that the globally averaged surface air temperature response to doubling of the atmospheric CO_2 content would be $\lesssim 0.26$ K, since the key element, an evaluation of the response function, or sensitivity of surface air temperature to perturbations in net radiation, "by three independent experimental means" is not presented. Instead, one is given a reference to S. B. Idso, "in preparation."

In support of his result, Idso does refer however to an important conclusion by Newell and Dopplick (*1*). They attempted to identify the influence of the eruption of Mount Agung (1963) on atmospheric temperatures. Since volcanic dust injected into the stratosphere and changes in CO_2 concentration both alter the net radiation received at the surface (solar plus infrared), this event provides a possible calibration point for theory in the problem of temperature change induced by CO_2 variations. The argument of Newell and Dopplick hinges on two points. First, the drop in free air temperature of the global tropical troposphere after the eruption of Mount Agung was apparently no more than about 0.4 K. Newell and Dopplick interpreted this small change as an indication of very low sensitivity of near-surface air temperature to changes in solar radiation incident at the surface, based on a rough upper limit estimate of the decrease in solar radiation produced by Agung. The most complete theoretical assessment of the Agung effect has been carried out by Hansen *et al.* (*2*), who took into account the detailed optical properties of the stratospheric aerosol in the visible and infrared. This is important because most of the radiation depleted from the direct solar beam by the stratospheric aerosol produced by Agung was scattered downward, and because the depletion in the net downward solar radiation (direct plus diffuse) was partly compensated by enhanced downward infrared flux. Hansen *et al.* found excellent agreement between calculated and observed temperature changes, but they did not present their results in terms of sensitivity of surface air temperature to changes in solar flux. However essentially the same model was applied to the problem of temperature response due to CO_2 enhancement by Wang *et al.* (*3*), who found that a CO_2 concentration-enhancement by a factor of 1.25 could produce an increase in near-surface temperature of 0.79 K. This result is quite consistent with that of the general circulation model of Manabe and Wetherald (*4*), which allows for a variety of factors, including large-scale atmospheric dynamics, in assessing the CO_2 effect.

The second point made by Newell and Dopplick concerned the regulation of surface temperature changes by evaporation rate over low-latitude oceans. They argued for a very low sensitivity in surface temperature to perturbations in net radiation because of the tendency for balance between net radiation and evaporation, but they assumed that the atmospheric specific humidity would not change as a result of surface temperature variations. It is much more plausible that the near-surface mean relative humidity $\bar{r}$ would remain constant for surface temperature changes, in which case the sensitivity estimated by Newell and Dopplick should be increased by a factor $\sim (1 - \bar{r})^{-1} \sim 5$. Newell and Dopplick also omitted the positive feedback on downward infrared radiation resulting from increased atmospheric water vapor content due to increased surface temperature, a factor which substantially increases the sensitivity, and they clearly stated that omission.

Thus, although it cannot be argued that there are no major uncertainties in the estimates of the influence of increased atmospheric CO_2 content on climate, there is absolutely no new evidence presented in Idso's paper to support his claim for an influence only at the level of climatological noise due to doubling of the CO_2, nor was any new evidence presented of a failure in the reasoning or methodology used to obtain the larger estimated effects, such as that found by Manabe and Wetherald.

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Schneider, Kellogg, and Ramanathan are dismayed, and Leovy finds it unfortunate, that the key element in my recent analysis of the CO_2-climate problem was listed as "in preparation." I share their sentiments. However, vagaries of the publication process are such that one sometimes gets the cart before the horse, so to speak, in disseminating several related reports. Some journals just are not as fast as others. To the credit of *Science*, however, in addition to their expeditious handling of my report, the editors also requested the pertinent background materials for study by the referees before a decision was made to publish.

Schneider *et al.* are also concerned that my analysis is "incomplete," "simplified," and that it fails to "raise the state of the art of climate modeling." In reply, I note that I am not a climate modeler, and I make no pretensions to raising the state of that art. Quite the contrary, rather than take the modeler's approach and attempt to calculate how the atmosphere *should* respond to a given forcing function, I have taken the experimentalist's approach and tried to ferret out how the atmosphere actually *does* respond. The response function that I thus obtain is a result of all those feedback processes—known and unknown—that operate in the real atmosphere. That it is also a simple approach should be a point in its favor, and not a reason for disdain. Indeed, Kellogg himself has been quoted as saying that "using the real Earth for a model is at least as good as, and probably better than, the theoretical numerical models" (*1*).

Nevertheless, several questions raised by Schneider *et al.* make it appear that my approach ignores important interactions and feedback processes. Perhaps the best way to alleviate resultant concerns is to briefly describe what I have done.

Basically, I have relied on a set of "natural experiments," that is, propitious environmental circumstances that allow for an evaluation of both atmospheric forcing and response functions in situations where a change in near-surface air temperature occurs. The first of these situations involves the vertical redistribution of dust that occurs, in the mean, between summer and winter at Phoenix, Arizona (*2*). This redistribution creates a small change in the flux of thermal radiation to the surface (one of my "in preparation" papers), and a colleague and I have documented this redistribution to be responsible for a near-surface air temperature change that is small but readily detected (*4*). The second situation deals with the arrival of the summer monsoon in southwestern United States. The advent of this moist-air intrusion from the gulfs of California and Mexico greatly increases the flux of thermal radiation to the earth's surface in a readily predictable manner (another of my background papers, now "in press" in *Water Resources Research*) and significantly raises surface air temperatures. Finally, the third experiment makes use of the monthly variation in mean near-surface air temperature caused by the monthly variation in solar radiation reception at more than 100 stations in the United States (the last of my "in preparation" papers).

Consider the differences among these three situations. Different atmospheric constituents are involved (dust and water vapor), as well as different regions of the electromagnetic spectrum (solar and thermal wavelengths), different time scales (hours to many months), and different magnitudes of forcing functions. Yet all situations yield essentially the same value for the near-surface air temperature response function—except for the last approach, where a dozen stations on the Pacific Coast yielded a result that was only half as great; I took that value to be an upper limit for the world's ocean surfaces. Thus, although the data base I worked with was admittedly not global, the good agreement among the results of such diverse experiments suggests that the atmospheric response function thus elucidated may be globally applicable. Obviously, more experiments of this nature would be helpful in establishing the validity of this supposition.

It also remains for future experiments to establish the validity of applying a relatively short-term response function, such as I have measured, to a long-term problem, such as the CO_2-climate connection. Even now, however, long-term global temperature records can be searched for a response to the already significantly increased atmospheric CO_2 content. Indeed, Ramanathan himself has just published one such study in *Science* (15 Aug., p. 763), wherein he concludes that "the surface warming due to increased carbon dioxide which is predicted by three-dimensional climate models should be detectable now." However, he states in the next sentence that "it is not." Thus, both the experimentally observed response characteristics of the real atmosphere and real climatic history cast doubts upon predictions of general circulation models that yield mean global temperature increases of 2 to 4 K for a doubling of the atmospheric CO_2 content.

A final question of Schneider *et al.* and practically the entire content of Leovy's communication have to do with the work of Newell and Dopplick (*4*). I would hate to rob the latter two investigators of the opportunity to speak for themselves, as indeed they will shortly (*5*) in reply to the criticism of their work by Watts (*6*). It should be obvious, however, that my conclusions do not depend in any way upon theirs, and that each will stand or fall on its own.

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Animals in the Classroom

The first and the last paragraphs in the briefing, "Science teachers to ban testing harmful to animals" (News and Comment, 15 Aug., p. 791) are misleading. The National Association of Biology Teachers adopted revised "Guidelines for the use of live animals at the preuniversity level" (*1*) because of better scientific understanding of animal perception and behavior, not in response to pressure from animal welfare groups. Our original guidelines, published in 1960 (*2*), became outdated because of advances in the fields of animal husbandry and experimentation.

In the last paragraph, two statements I made during a telephone interview with author Marjorie Sun are correctly quoted. However, I was responding to the question "How much are animals *used* in the classroom?" rather than "abused," as reported. I replied that animal use in biology instruction has decreased, due partly to a dearth of animal care courses for teachers, but mostly to school budget cuts. Animal abuse, to my knowledge, has never been a problem among biology teachers.

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