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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