

CORRESPONDENCE

Reply to A. J. Crane's "Comments on Recent Doubts About the CO₂ Greenhouse Effect"¹

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Crane (1981) has done an excellent job of describing the differences between current numerical modeling and empirical approaches to the problem of CO₂-induced climatic change. Whereas prior to this time it has appeared that the major discrepancy between these two techniques was their employment of radically different surface air temperature response functions, it is now clear from Crane's contribution and the recent paper of Ramanathan (1981) that the discrepancy comes from the way in which feedback processes are handled. Indeed, both Crane and Ramanathan have noted that my experimentally derived value of the surface air temperature response function over land is essentially identical to that obtained from the model of Ramanathan *et al.* (1979).

The central question of the current controversy thus reduces to this: Should only direct effects of CO₂ comprise the radiative perturbation that is multiplied by the surface air temperature response function, or should feedback effects—particularly those due to water vapor—also be included in the forcing term? Since the three experimental means I originally used to derive the surface air temperature response function were set within time frames ranging from only hours to months, Crane has appropriately questioned whether this response function can be validly used to assess the ultimate equilibrium response of the surface air temperature to a doubling of the atmospheric CO₂ concentration without adding the feedback effects of enhanced atmospheric water vapor to the forcing term; and this is also the position of Ramanathan (1981).

Without additional information, it would be dif-

ficult to answer this question—perhaps even impossible. However, I have recently devised a fourth means of evaluating the surface air temperature response function which should apply to long time scales and should also have included within it the effects of all feedback processes that operate within the earth-ocean-atmospheric system (Idso, 1981, 1982). Interestingly, this evaluation yields the same result for the globe as a whole as that which I obtained from my three original analyses. Since this value of the surface air temperature response function thus has long time scale feedback processes included within it, I feel that only the *direct* effects of an increase in atmospheric CO₂ concentration should be multiplied by it. Consequently, I feel confident that the ultimate equilibrium response of the earth's surface air temperature will be as I originally predicted it to be, i.e., about an order of magnitude less than the current consensus estimate of the theoretical numerical models (Idso, 1980).

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