significantly elevated prior to an increase in reticulocytes (Fig. 2). The correlation coefficients for GSH/PCV and reticulocyte count for each group on each sample day were not significant when compared with published critical values (13). Elevated GSH/PCV may indicate a unique biochemical response by avian red cells experiencing oxidant stress or a toxic mechanism involving reduced activity of a GSH-dependent enzyme such as glutathione peroxidase. More detailed biochemical studies are needed before these GSH data can be interpreted. Anemia was reported in earlier experimental studies of oil ingestion by birds, but it was not characterized and generally was not considered significant (5, 14).

The amount of oil ingested by wild birds that become oiled is not known, and thus we cannot precisely evaluate the environmental implications of our experimental results. Subtle but significant changes in red cell life-span or function may occur at doses that do not produce overt anemia. Disturbances of red blood cell function potentially can affect many other body tissues. Pathological changes described in studies of oil ingestion by birds (4–6) may be, in some cases, secondary or tertiary responses to a primary dysfunction of the erythron.

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Climatic Effects of Atmospheric Carbon Dioxide

Hansen et al. (1) have used numerical models to provide some insight into why and how the climate will respond to increasing CO2 concentrations. In addition, however, they argue that the consistency of results from one-dimensional climate models and from observations of global surface air temperature over the last 100 years indicates that the climate is warming due to increasing CO2 concentrations as global models predict. I agree that the climatic record is not inconsistent with the projected warming to be expected if there is to be an increase of 2 to 3 K for a doubling of CO2 concentrations and strongly agree that first detection of such changes should be sought by analyses such as done by Hansen et al. However, there are a number of limitations in their analysis that must be resolved if we are to say with as much confidence as their article conveys that the initial climatic response to increasing CO2 has been detected. Among the issues to be resolved are the following.

Although observations [such as figure 3 in (1)] show a global cooling from the late 1930’s to the early 1960’s, the results of Hansen et al. with their best model show a much smaller decrease. Thus, while their curve looks good, it shows less than some of their predictions. It is not that because of natural fluctuations or because of a serious omission in the model? We do not yet know. Even the very small decrease in temperature from 1930 to 1960 shown by Hansen’s model is strongly dependent on the physically untenanted postulation of Hoyt (2) concerning umbra/penumbral ratio. Hansen et al. comment that Hoyt’s hypothesis was the only one of three viable contenders concerning solar activity that worked. This aspect of their work is extremely uncertain. In addition, their analysis does not consistently apply in each of their three areas. For example, the 1935 to 1960 cooling takes place almost exclusively north of 23.6°N. They do not explain why umbra/penumbral only works in that region (in an exaggerated way) and not over the other 70 percent of the globe.

Hansen et al. have not analyzed the volcanic results on a hemispheric or regional basis. Why, for example, does the near-equatorial Mount Agung eruption in 1963 have a larger effect in the Northern than the Southern Hemisphere when observations show much more aerosol in the Southern Hemisphere? The answer usually is given that the ocean’s thermal inertia is larger in the Southern Hemisphere, but in some other cases—for example, around 1900 to 1910—the response was much larger in the Southern Hemisphere, yet many of the volcanic eruptions at that time were in the Northern Hemisphere.

The authors indicate that their results tend to confirm model results for global climate change. Virtually all of these
models also indicate an amplification of the temperature change in polar regions, yet recent data of Angell and Korshover (3) (and, I suspect, Hansen’s data) do not show this. Hansen et al. should also say that the polar amplification—the largest regional effect predicted by models—is not confirmed. It is essential that a reasonable set of climatic parameters that are expected to respond to increasing CO$_2$ be determined and a coordinated search be initiated to find correlated changes among them all.

There are a number of uncertainties related to the size of the CO$_2$-induced temperature change used by Hansen et al. (i) The history of CO$_2$ concentrations is known only back to 1957. Before that they rely on carbon cycle considerations that virtually ignore the suspected contribution of CO$_2$ to the atmosphere by the biosphere. If the biosphere played a role, their fit will change. (ii) Their climate model balances some simplifications against others in arriving at the expected temperature change. The 2.8 K temperature increase used for a doubling of CO$_2$ could be wrong by a factor of 2, which would affect the correlation. (iii) Their continent/ocean ratioing to increase the size of the climatic effect seems to assume that the continents cannot themselves cause heat to be transported to the upper atmosphere (for instance, by convection) and then radiated to space; instead, this heat must be transported upward by additional latent heat release after warming the ocean. The assumption needs to be tested. (iv) Their analysis begins in the 1880’s, when the climate was apparently quite cool due to major volcanic injections during that decade. Stratospheric aerosol concentrations 100 years ago were not well measured. Extending the temperature data set back to 1850 may help reduce any bias introduced by the choice of time interval.

As an alternative analysis, one could estimate the climatic effect of increasing CO$_2$ by comparing the minimum temperatures reached after Krakatoa and after Agung, which were equatorial volcanoes. On a global basis the difference is about 0.25 K (the Agung minimum being warmer), with about 0.3 K north of 23.6°N, 0.15 K south of 23.6°S, and 0.25 K in tropical regions. Most data suggest that Krakatoa was bigger than Agung (25 percent larger Lamb dust veil index), so the maximum CO$_2$ effect from 1883 to 1963 must be less than 0.25 K, probably more like 0.2 K. This is not very different than Hansen’s result. However, one might then ask why Agung, if it was smaller, appears to have caused a larger temperature decrease from prevailing values than did Krakatoa.

In summary, although Hansen et al. have probably carried out a broader scale analysis than any previous investigators, I believe that they have understated many uncertainties that deserve careful consideration.

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References and Notes
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Hansen et al. (1) make much of an apparent increase in mean global air temperature starting in the mid-1960’s. However, this warming comes about as a result of their southern latitude data set, which represents far fewer stations than either their low latitude or northern latitude data sets; and when the latter measurements are studied, just the opposite is seen. For instance, a simple linear regression analysis of their low latitude data shows an almost unchanging temperature for the last 55 years, while for northern latitudes the trend has been strongly negative at more than 0.1°C per decade since 1955. The latter result is especially significant, for general circulation models of the atmosphere all predict that the CO$_2$-induced warming should be most evident at high latitudes.

To give some feel for the magnitude of discrepancy, it can be derived from the calculations of Hansen et al. that the "probable" global warming predicted by the models between 1935 and 1980 is about 0.25°C. Since they then suggest that high latitude warming should be two to five times the global mean warming, the models predict that northern latitude temperatures should have increased by 0.5° to 1.25°C over that period. However, the data of Hansen et al. show a mean temperature decrease for this interval of 0.5°C. This discrepancy of 1.0° to 1.75°C between the model predictions and observations in northern latitudes actually refutes the validity of the numerical climate models.

Many people find it difficult to believe that the models can be wrong, particularly since they all seem to predict about the same degree of warming. But this similarity, too, is misleading. For instance, the model of Hansen et al. predicts a 1.2°C temperature rise as a result of direct CO$_2$ effects and a 1.0°C rise as a result of the "well-established H$_2$O greenhouse effect," for a 2.2°C total warming and a water vapor feedback enhancement factor of 1.8. In Ramanathan’s (2) most recent analysis, however, direct effects of CO$_2$ account for only a 0.5°C temperature rise, with feedback effects of water vapor adding 1.7°C more. Thus, although the total temperature increase that Ramanathan calculates is identical to that of Hansen et al., his water vapor enhancement factor is 4.4. If these two models of the atmosphere differ so dramatically from each other in their assessments of this well-established effect, it is no wonder that they fail to properly represent the truly complex aspects of the earth-ocean-atmosphere system which lead to discrepancies of the type described above for northern latitudes.

With respect to potential benefits of increased atmospheric CO$_2$, Hansen et al. mention only the possibility of an increased growing season. In a review of more than 400 experiments dealing with economic yields of agricultural crops, however, Kimball (3) has demonstrated that a doubling of the atmospheric CO$_2$ content could increase global productivity by 33 percent and that a tripling could boost it by 66 percent—without additional inputs of fertilizers or water. It is thus time to realize that the CO$_2$ question is not a single-issue subject and that there are some positive agricultural benefits to be gained from a CO$_2$-enriched atmosphere.

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References and Notes
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We used a one-dimensional (1-D) climate model to show that global mean temperature increased in the past century at a rate consistent with the greenhouse theory (1). The questions raised by MacCracken and Idso in no way alter that result or undermine the conclusion that the greenhouse effect is real and will lead in the next century to global climate change of almost unprecedented magnitude.

Observed temperature trends. Mac-
Climatic Effects of Atmospheric Carbon Dioxide

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