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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8. An analysis of the PBCO used in this study was reported in the study of Peakall *et al.* (*6*).
9. Packed cell volume (PCV) was measured by the microhematocrit method and total hemoglobin by the cyanomethemoglobin method. Total solids in plasma were measured with a refractometer. Reticulocytes were counted as the percentages of 300 red blood cells in smears stained with new methylene blue (NMB). Conservative criteria were used in identifying reticulocytes, and only cells with a complete or nearly complete perinuclear band of blue granular material were counted [A. M. Lucas and C. Jamroz, *Atlas of Avian Hematology* (Agriculture Monograph 25, Department of Agriculture, Washington, D.C., 1961), pp. 22–30]. Heinz bodies were observed in smears stained either with NMB or with brilliant green and neutral red [M. L. L. Schwab and A. E. Lewis, *Tech. Bull. Regist. Med. Technol.* **39**, 93 (1969)] and were recorded as the percentage of 200 red blood cells that contained Heinz bodies. For the study of blood cells in sections, whole blood was fixed in 1 percent glutaraldehyde in buffered isotonic saline, post-fixed in osmium tetroxide and uranyl acetate, and embedded in epoxy resin. Sections (1 μm) were stained with toluidine blue, and ultrathin sections for electron microscopy were stained with uranyl acetate and lead citrate.
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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 CO_2 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 CO_2 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 CO_2 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 CO_2 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 chops off the peaks and valleys over the last 60 years. Is 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 untested postulation of Hoyt (*2*) concerning umbra/penumbra 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/penumbra 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 given is 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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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 1935. 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_2O 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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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-

Cracken and Idso are incorrect in stating that observations do not show a polar enhancement of the temperature trend, which is the principal expected subglobal temperature effect (*1, 2*). The global temperature warmed by 0.4° to 0.5°C in the past century and high latitudes warmed by 1.2° to 1.5°C (Fig. 1), polar enhancement by about a factor 3.

Polar enhancement is expected for time scales long enough for the ocean surface temperature to approach its equilibrium response at all latitudes, that is, a few decades or longer. Figure 1 shows that polar enhancement also occurred for such decadal periods: the 1880–1940 warming and the 1940–1965 cooling and warming thereafter.

MacCracken and Idso make much of regional details and peaks and valleys of the observed temperature trend. We agree that geographic patterns and short-term variations of observed temperature contain valuable information on the climate system. As our article made clear, we strongly support the development of three-dimensional models that will realistically model natural climate variability on a regional scale and that can simulate the effect of greenhouse warming on such factors as standing and transient long waves and ocean currents. In the interim, changes in regional temperature patterns do not provide a basis for confirming or disproving the greenhouse effect. Note also that the global temperature trend has substantial interannual fluctuations (Fig. 1). Thus our procedure of analyzing the mean long-term trend should be more reliable than that of comparing temperatures from valley to valley after large volcanic eruptions, as proposed by MacCracken.

Climate sensitivity. Idso states that our model gives a water vapor feedback factor of 1.8 and Ramanathan's model (*3*) yields 4.4. He uses this to argue that the consensus in the climatological community on the approximate magnitude of the greenhouse effect is misleading and the models are therefore untrustworthy. However, careful reading of the papers would have shown that the difference is one of semantics. We use the conventional definition of water vapor feedback: we run our model with CO_2 doubled but everything else unchanged, and then with CO_2 doubled and water vapor increased according to fixed relative humidity. With the same definition, Ramanathan obtains a feedback factor of 1.6 [table 4 in (*3*)].

MacCracken's numbered comments revolve around the question of whether several uncertainties in our analysis, such as the CO_2 abundance in 1880 and the choice of starting date for our analysis, could throw the results off. (i) A plausible change of 10 to 15 ppm in the 1880 CO_2 abundance would have little effect on our results. In fact, the sense of suggested positive biospheric contributions to atmospheric CO_2 prior to 1940 is to slightly improve the ability of the model to match the relatively rapid warming in 1880 to 1940. (ii) We explicitly recognized the uncertainty in global climate sensitivity, and we even tested the effect of letting the model sensitivity be a free parameter. We found that with an exchange rate between the ocean mixed layer and thermocline based on passive tracers (k-1 to 2 cm^2/sec), a climate sensitivity of 2.5° to 5°C is needed to best fit the observed global temperature trend. The consistency of this empirical sensitivity with the a priori sensitivity estimated from climate models provides some evidence that this sensitivity is of the right order. (iii) Changes in the continent/ocean factor were tested in our 1-D model and found not to be of appreciable importance. (iv) Extension of the analysis back to 1850 is desirable, but it is not possible at this time due to lack of information on global temperature and atmospheric composition for that period. In summary, we agree with McCracken that it is desirable to account for as many uncertainties as possible in modeling climate, but in fact our analysis covers a greater range of possibilities than he has raised.

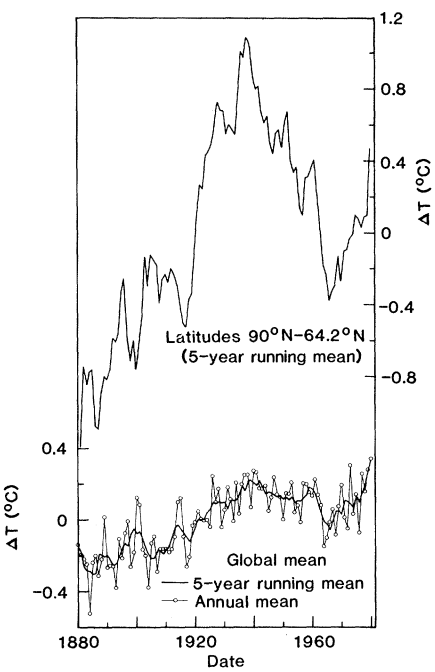

Fig. 1. Observed temperature trends based on data set described in (*1*), updated through 1980.

Confirmation of CO_2 warming. MacCracken misinterprets our position on detection of the warming. We stated "More observations and theoretical work are needed to permit firm identification of the CO_2 warming." Also, we quantitatively compared projected warming to natural climate variability, concluding that the greenhouse warming "should emerge from the noise level of natural climate variability by the end of the century, and there is a high probability of warming in the 1980's."

The eruption of El Chichón in Mexico in 1982 injected a stratospheric aerosol veil comparable to that from Agung in 1963. This may counteract the CO_2 warming for 1 or 2 years, and it provides an excellent opportunity for testing global climate models. But, barring improbable further eruptions of the magnitude of Agung or El Chichón, significant warming is still likely in this decade.

Beneficial effects of CO_2. Idso raises the question of CO_2 benefits for photosynthesis. Our article was limited to the climate implications of increasing CO_2. We did not attempt to weigh the beneficial and detrimental effects, concluding: "Improved global climate models, reconstructions of past climate, and detailed analyses are needed before one can predict whether the net long-term impact will be beneficial or detrimental." We agree that the well-known issue of CO_2 "fertilization" of plants is an important question, but its discussion is extraneous to our article.

In summary, MacCracken and Idso present no new information which significantly modifies our analysis. The evidence that continued emission of CO_2 and trace gases will lead to climate change is sufficiently compelling to call for vigorous investigation. The required efforts in global observations and climate analysis are challenging and require long-range commitment, but the benefits from improved understanding of climate will surely warrant the work invested.

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