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Discussion Paper

by

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on

William W. Kellogg's

**CARBON DIOXIDE AND CLIMATE CHANGE:
IMPLICATIONS FOR MANKIND'S FUTURE**

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1. INTRODUCTION

Following a script made standard by CIAP (Climate Impact Assessment Program for the SST), freon aerosol spray cans, increasing anthropogenic emission of atmospheric particles and a host of similar forgotten or as yet unrealized hazards, Will Kellogg has presented the prevailing "Consensus View" of the CO_2 (carbon dioxide) climate problem and then outlined the cascade of deleterious effects this may produce throughout all aspects of human activity and the global ecology. Will did depart a bit from the script. He devoted at least 16 lines (out of 36 pages) to possible beneficial consequences; several times mentioned caveats and uncertainties and he even admitted that "there are still some who even refuse to agree with the consensus that the change will indeed occur".

Other than that I am one of those who have refused to agree with the consensus, what more can I say? There are at least three points I would like to make:

(1) I would like to have you step back and take a critical look at the process by which the now fashionable "Interdisciplinary Science through Workshops of a Committee of Experts" operates; how it transforms preliminary guestimates into a consensus supported by a constituency with a vested interest in confirming and perpetuating the problem rather than solving it.

(2) Secondly I would like to have you visualize the hazard from the receiving end of the hazards cascade and contrast this with the description starting from the origin given by Will.

(3) And finally, I would like to present a few of the reasons why I have refused to accept the "Consensus View".

2. DEVELOPMENT OF THE CONSENSUS VIEW

In preliminary planning sessions the basic hazard or problem and its cascade of possible ramifications down through the global environment to the human economy and social practices are sketched out. Experts in the various disciplines involved are recruited and then, one or a series of workshops are scheduled. In order to facilitate and speed the impact assessment process, specialists in the primary impact area - in our case, climate modeling - are asked to draw up preliminary estimates of the basic impacts in terms of quantitative changes in temperature, precipitation, wind, solar radiation, sea level, etc. (independent of the uncertain time of occurrence). These are passed down the line to the ecologists, biologists, agronomists, economists and social scientists as imput for assessing impacts in their areas of interest.

In the process a number or set of numbers of little concern to anyone has suddenly acquired, among the recruited specialists, their employees and employing institutions, a large constituency. The basic scientific problem lies in a discipline foreign to most of those recruited to work on the impact assessment and is generally outside their capabilities and of little intrinsic interest. Thus a large constituency has been established more concerned with confirming the hazard and prolonging the impact assessment process than they are in solving or increasing our understanding of the specific problem which gave rise to the impact assessment effort.

As the work proceeds, each group of researchers writes their report and these are successively meshed and condensed into a final one-paragraph statement of findings - and almost invariably, an eventual single headline. No matter how carefully the original reports are written, at each step in the meshing and condensation, the caveats get dropped, shortened or weakened while the hazards or impacts of dramatic interest acquire successively more dominant roles - i.e., they are made to appear both more severe and more certain. Remaining uncertainties which cannot be resolved or ignored are covered by parameterizations and error bars.

I witnessed this process first hand during CIAP. I early recognized the adoption of "The One-Way Filter" approach (Ellsaesser, 1974) in which only those pathways of the effects cascade leading to deleterious effects are pursued and described while any possible benefits are studiously ignored.

CIAP's evaluation of the SST was comparable to a debate on whether we should continue to have children in which the discussants are allowed to consider only the problem of dealing with bodily excreta. Harmful ultraviolet became a single unseparable word - no statement or report was complete without a reference to its causing an increase in the incidence of skin cancer. Rickets, which may be alleviated by increased exposure to ultraviolet, might as well have been an unknown phenomenon. It may be of interest to note that the original concern that aircraft exhaust would cause destruction of ozone in the stratosphere is now shifting to a concern for ozone generation in the troposphere - since this now appears to offer the only saleable argument for continuing research on this subject. However, no one, so far as I am aware, has yet suggested that increase of ozone in the troposphere might cause a decrease in the incidence of skin cancer.

Since my experience with CIAP I have preferred not to get too involved in "big science" and to dare to try to be right rather than merely to reflect the prevailing consensus. It has been my experience that the greater the concern, as indicated by media attention to a research program, the more difficult it appears for the participating scientists to maintain and exercise objectivity.

If there is a parameter in another disciplinary, such as oceanic thermal lag impacting the answer, I don't have to accept the opinion of the committee oceanographic expert, I can investigate for myself and determine whether it is any more firmly established than other parameters impacting on the problem.

3. THE IMPACT OF CLIMATIC CHANGE AS VIEWED BY THE IMPACTEE

Will Kellogg said that the primary impact of doubled CO_2 would be a rise in planetary mean temperature of 3±1.5°C and that this can be expected to occur in the time range mid to late 21st century. I ask you to visualize yourself as a farmer, a natural gas distributor or a construction engineer. You are engaged in producing and delivering a product for sale in a competitive market. There is a premium for delivering when no one else can and a penalty for not being able to deliver on demand. On a daily and annual basis you contend with the seasonal cycle and with its day to day and interannual variability. The annual range of surface temperature can reach 120°F (67°C); day to day variability here in Chicago is such that the daily temperature forecast was on at least one occasion missed by over 100°F (56°C). I ask you, to what extent are you going to modify your business practices when Will tells you that the mean temperature, not <u>will</u>, but <u>may</u> rise 3°C in the next 50 to 100 years?

4. THE CLIMATIC EFFECT OF CARBON DIOXIDE

The consensus that doubled-CO_2 will lead to a global temperature increase of 3°C is actually quite narrowly based - it exists primarily among climate modelers and their associates. While climate models come in a wide assortment, the two considered most reliable are the radiative-convective models and the GCM's (general circulation models); both of which are quite complex and detailed. The optimum attributes for developing such models are burning ambition and an uncluttered mind - which helps explain why most such models have been developed by graduate students and post-Doc's. I don't believe that most of us would agree that such people are in general the ones who best understand how the atmosphere as a whole works.

Among non-climate modelers - which includes a good portion of the meteorological profession - there are many who doubt the "Consensus View" on climatic effects of CO_2 but their opinions rarely appear in print. The ones who did get their doubts in print (Newell and Dopplick, 1979; Idso, 1980) have been strongly attacked by the establishment (Crane, 1981; Kandel, 1981; NAS, 1982; Schneider et al., 1980; Watts, 1980, 1982). My own reason for doubting the "Consensus View" are as follows.

Climate modelers have so far concerned themselves mainly with two climatic feedback processes both of which are claimed to amplify any CO_2 warming: (1) the so-called ice-albedo feedback, and (2) the water vapor feedback. There are at least four reasons for believing that the ice-albedo feedback is currently overestimated -- if not actually of the wrong sign:

(1) Very little sunlight is received to be reflected in those latitudes and seasons in which seasonal snow and ice cover occur.

(2) Planetary albedo is also strongly influenced by solar zenith angle. Once this is allowed for there is relatively little difference in high latitude planetary albedo between ice-in and ice-out (Lian and Cess, 1977). The poor satellite data available in these areas suggest a change of no more than 0.1 to 0.25 albedo units at 50-70°N and 60-80°S (Campbell and Vonder Haar, 1980; Stephens et al., 1981).

(3) Ice and snow, on the other hand, do have a very strong ice-insulation negative feedback. An ice cover reduces the wintertime loss of latent and sensible heat to the atmosphere and outer space from open water bodies by orders of magnitude. Also, snow covered land and sea ice can reduce their radiational loss of energy by cooling to radiating temperatures well below those reached without an insulating snow blanket. Reduced winter loss of heat to space represents a warming for the planet.

(4) Polar ice and snow behave quite differently in a seasonal than in an annual mean model in which the sun shines all the time.

That the negative ice-insulation feedback is the one that predominates is suggested by the observations that successive summer and winter Antarctic ice cover anomalies tend to have the opposite sign (Zwally et al., 1983) and that Arctic ice cover shows a negative auto-correlations at 12 months lag (Weisenstein, 1978). In fact, negative auto-correlations of sea ice coverage for lags of 12 months or less appear to effectively deny the existence of any significant positive feedback relation between temperature and ice cover.

The important point to remember about the water vapor feedback is that it does not become effective until after the surface temperature has been increased by some other mechanism. Over the tropical oceans doubled CO_2 will give a minimal increase in IR (infrared) flux to the surface due to the fact that the lower atmosphere there is already opaque to IR radiation due to the continuum absorption by water vapor. In addition, as temperatures rise, an increasing fraction of any enhanced flux to the surface will be used for evaporation as opposed to temperature rise. Thus, the effect of doubled CO_2 on tropical ocean temperatures should be minimal. From a study of maximum temperatures attained by plant foliage and the surface waters of tropical oceans Priestley (1966) deduced that there should be a "rather sharply defined upper limit to which air temperature will rise above a well watered surface." Using monthly averages of daily maximum temperatures he identified the limiting temperature for land stations which had not exhausted their soil moisture as about 92°F (33.3°C). Priestley and Taylor (1972) found that the Bowan ratio (ratio of sensible to latent heat flux from the surface to the atmosphere) decreased monotonically with increasing temperature suggesting that sensible heat flux became negative (flowed from the atmosphere to the surface) at temperatures above about 32°C. A reversal of the heat transfer between plant leaves and air in the vicinity of 33°C was reported earlier by Linacre (1964). A review of Bowan ratio data by Brutsaert (1982) placed the apparent sign reversal near 30°C. Newell and Dopplick (1979) sought to illustrate the physics of this situation, but by holding the surface atmospheric conditions fixed at 27°C and 65% relative humidity computed an indefensible requirement of 30 W/m^2°C for warming the surface water of the tropical oceans. However, their lack of realism was not unusual for the introduction of a new idea and in any case, in no way negates the requirement for substantial increments in radiative flux to warm tropical ocean waters in contact with the atmosphere to temperatures above their current level of 26 to 30°C.

This point is supported by the palaeoclimatologists. Matthews and Poore (1980) proposed the working hypothesis that the surface temperatures of the tropical oceans are tied to the solar constant and have maintained their present values back even through the Cretaceous -- the period of warmest terrestrial climate yet documented (Barron et al., 1981). In their reconstruction of the climate of 18,000 YBP (years before present), CLIMAP (1976, 1981) found very little change in tropical ocean surface temperatures other than that attributed to stronger Chile and Benguela ocean currents and stronger equatorial upwelling. Additional support is provided by the pattern of seasonal change in ocean surface temperatures. The map of Newell (1979) shows vanishingly small changes near the equator, bands of maximum changes at 35-45°N and S, and decreasing changes poleward of the midlatitude maxima. While they had no data to show it, the seasonal water temperature change must vanish at the most poleward summer boundaries of the sea ice. Seasonal temperature changes near the equator remain small despite both the considerable changes in solar flux due to the seasonal change in zenith angle and the 7% change in solar flux due to ellipticity of the Earth's orbit.

Broecker (1982) presents maps of the difference between current ocean surface temperatures and those for 18,000 YBP as reconstructed by CLIMAP (1981). These indicate that the pattern of ocean surface temperature change for a change in terrestrial climate (glacial to interglacial) is analogous to that for the seasonal changes, i.e., vanishingly small at the equator, latitudinal bands of maximum change in mid-latitudes and decreasing changes toward the poles. While we do not know the cause of the climate change which occurred over the past 18,000 years, the change in mean global temperature was at least as large as is currently anticipated for a doubling of CO_2.

Doubling alone will increase the IR flux to the surface by 1-1.5 W/m^2 averaged over the globe. If continuum water vapor absorption is considered, doubling CO_2 will produce hardly any increase in IR flux to the surface in tropical latitudes where the moist surface layer is already essentially opaque to IR radiation (Kiehl and Ramanathan, 1982). Yet most doubled-CO_2 model experiments appear to compute a tropical ocean warming of about 2°C. I have yet to hear a plausible explanation of this apparent gross discrepancy.

Analyses of polar glaciers (Neftel et al., 1982; Oeschger et al., 1982) and oceanographic data (Chen, 1980) have recently been added to inventories of the preindustrial biosphere as reasons for lowering the range of estimates for the preindustrial level of atmospheric CO_2 -- this range now extends from 250 to 290 ppm. With today's level of 340 ppm and a model computed doubled-CO_2 warming of 3.0°C; we should, assuming equilibrium, have already had a warming of 0.68 to 1.33°C. Increases in other anthropogenic greenhouse gases such as methane, nitrous oxide and chlorofluorocarbons have presumably contributed an additional warming of approximately one-half this magnitude (Lacis et al., 1981; Craig and Chou, 1982). If, to warm the ocean surface, it is necessary to warm the total depth of the ocean, there is of course no problem. However, if we examine the oceanic temperature profiles where there are significant seasonal variations in surface temperature, we find a negative correlation between the amount of surface warming and the depth to which it penetrates (Newell, 1979). In the regions of large seasonal temperature change and even in the Gulf Stream we find that as the surface layer warms, the water at depths of 100 to several hundreds of meters actually cools (Newell, 1979; Niiler and Richardson, 1973). These data do not suggest that the whole or even a significant fraction of the oceanic layer has to be warmed before the mixed layer temperature can rise. They rather suggest that any general warming of the ocean surface will be reflected in an upward extension of the thermocline to a warmer but shallower mixed layer. The more the horizontal pattern of ocean surface warming conforms to the pattern of seasonal temperature change described above the more plausible this vertical pattern of temperature change becomes. And if this vertical pattern of warming occurs, the ocean thermal lag will be less than a decade. These three conditions; the lowering of the estimates of the preindustrial level of CO_2, the shortening of the possible oceanic thermal lag, and the inclusion of other greenhouse gases; are all making it increasingly difficult to explain why the model predicted CO_2 warming has not yet been unequivocably detected. The situation is made even more untenable by the significant biospheric source of CO_2 which must have been released prior to 1900 and thus should have produced its warming effect by now for any credible oceanic thermal lag.

However, my strongest reason for doubting current climate model estimates of the CO_2 warming are the gross differences I see between how the atmosphere works and how it is modeled to work. Water vapor feedback in particular is usually visualized as originally modeled by Manabe and Wetherald (1967). Under the assumption of a fixed relative humidity profile and a fixed tropospheric lapse rate, every increase in surface temperature instantaneously increases the temperature and absolute humidity throughout the tropospheric column. Clearly this leads to a significant positive water vapor feedback. However, both seasonal and latitudinal variations reveal a negative correlation between temperature and relative humidity.

Water vapor in the real atmosphere is cycled mainly by convection which is organized into deep cells by orography, thunderstorms, convergence lines including both wave cyclones and tropical cyclones, ITCZ's, monsoons and the Hadley circulation. Each of these on its own space scale sweeps the warm moist air from the boundary layer into narrow ascending columns where it is dried by condensation and precipitation of the water vapor and the latent heat is carried above the climatological moist layer and then spread by horizontal divergence. This diverging air is frequently accompanied by a cirrus layer which both reflects incoming solar radiation and provides a black body emitter above approximately half of the atmospheric mass and above a much larger fraction of our principal greenhouse gas — water vapor. At the same time each convective cell is surrounded by a region in which subsidence, compensating for the air lifted within the cell, dries the surrounding troposphere.

Any objective analysis of tropical convection leads to the conclusion that it is a highly effective nonlinear planetary temperature regulator. Consider the tropical cyclone for a moment. These are very sensitive to both surface temperature and tropospheric lapse rate and can only form over the low friction surface provided by the ocean. They rarely if ever form at ocean temperatures below 27°C. Each one individually removes a tremendous amount of heat and moisture from the vicinity of the planetary surface in the forms of precipitation, latent heat carried to and spread horizontally in the upper troposphere, solar radiation reflected back to space by the cloud shield and even sensible heat removed from the oceanic mixed layer by mixing it down into the thermocline by wind induced turbulence. In addition, most if not all of the air carried aloft is compensated by subsidence which dries the surrounding areas. It is inconceivable that a tropical cyclone does not temporarily reduce the precipitable water vapor and thus its greenhouse effect for hundred of kilometers on either side of its path. Most models appear to predict that increased CO_2 will intensify the Hadley circulation. This could as well lead, through increased subsidence and drying of the tropospheric air column in the subtropics, to a negative rather than a positive water vapor feedback throughout most of the tropics, or nearly half of the global surface.

Lindzen et al. (1982) recently demonstrated that replacing the conventional convective adjustment in a radiative-convective model with a physically based, cumulus-type parameterization of convection reduced the model sensitivity to doubled CO_2 by up to 80% for tropical temperatures. The sensitivity continued to decrease for even higher, unrealistic temperatures in such a way that they concluded that "cumulus convection effectively inhibits any possibility of a run away greenhouse on earth." It is noteworthy that Lindzen et al. (1982) obtained these results without considering the drying of air columns surrounding convection cells due to the subsidence which compensates the convective updrafts. Idso (1982) concluded from a completely different approach that a run away greenhouse was not possible under terrestrial conditions. Some of the more sophisticated models used to perform the doubled-CO_2 experiment presumably contain the physics to handle the Newell and Dopplick (1979) problem correctly. However, the fact that they continue to predict equatorial surface warmings of about 2°C represents an enigma which merits priority investigation.

Current climate models overestimate the effect of ice-albedo feedback in high latitudes and overestimate ocean warming and water vapor feedback in tropical regions and as a consequence are in general overestimating the climatic effect of doubled CO_2 by at least 2- to 3-fold. This upper bound of 1 to 1.5°C degrees for the effect of doubled CO_2 is derived from the magnification effect usually attributed to the two feedbacks cited and from the fraction of the currently predicted warming allowed by available temperature records. While I find reasons to expect the effect of doubled CO_2 to be even smaller than this upper bound, they cannot be quantified without extensive additional model tests.

Robinson (1983) has just reminded us that both cloud coverage and the radiative effect of clouds differ significantly between day and night and pointed out that a model which does not explicitly attack this problem "does not provide safe ground for an assessment of the carbon-dioxide climate problem".

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