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THE CARBON DIOXIDE/CLIMATE CONTROVERSY: SOME PERSONAL COMMENTS ON TWO RECENT PUBLICATIONS

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Abstract. The pervasive opinion on the relationship between the state of the climate and the increasing concentration of CO_2 is that a general global warming will occur with social, economical and environmental corollaries that may be adverse. However, there exist a number of dissenting arguments that call for a much smaller increase in global temperature or even an induced global cooling. Furthermore, the positive biological effects of a greater atmospheric CO_2 loading are emphasized.

The difference of opinion is highlighted in two recent publications: *CO_2, Friend or Foe* by Sherwood Idso and *Carbon Dioxide: A Second Assessment* by the National Academy of Science CO_2/Climate Review Committee. Using the two publications as focal points, some personal remarks are made regarding the controversy and the relative merits of the scientific arguments.

1. Introduction

Accompanying our increasing ability to measure, archive, model and (hopefully) understand the many facets of the Earth climate system has been an explosion in the number of questions regarding the overall sensitivity of the system itself; with answers that often lead to a forecast of climatic disaster of the most serious kind. Of course, some were easily dismissed and these cases such as those which suggested an impending glacial epoch resulted from (at best) an intrepid extrapolation of an incomplete or unrepresentative data set. Discarding that hypothesis was accomplished by merely noting the reversal of the trend in the same sketchy data set.

Other scenarios have been much harder to dismiss and it is difficult to know whether our inability to discard the climatic forecasts out of hand results from an incomplete knowledge of the climate system or because a basic truth lies within such an hypothesis. Two instances come to mind and both deal with the contamination of the atmosphere by substances produced by anthropogenic causes. Specifically, I refer to possible changes of the ozone layer of the upper atmosphere and to the effect on the climate as a whole by substantial changes (e.g., factors of two) in the CO_2 concentration. In a sense both problems are quite similar and, of course, related. The ozone problem involves an appreciation of the role an anomalous contaminant will play within the myriad of chemical and photochemical interactions of the ozone cycle. The CO_2/climate question is at least equally complex. The complexity of the CO_2 problem arises because we must ascertain two effects: a primary effect and a host of secondary effects that may enhance or mitigate the primary response of the system.

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The two climate questions raised above are also similar in another aspect beyond their shared complexity. Both highlight areas of gross uncertainty in the understanding of the subjects and both have become the subjects of intense, and occasionally emotional, debate. In both cases, following appeals from prestigious committees, extensive national and international studies have ensued in an attempt to understand the complex processes. Another common feature is the surge of publication that has attented the debates and controversy. For the most part, the manner in which we approach science has been quite successful in the dismissal of the "crank" ideas on one hand, and the exposure of misconceptions, misunderstandings and plain errors in our preconceived knowledge of the climate system on the other. I emphasize 'for the most part', for throughout the ebb and flow of the many controversial issues, the one which has prevailed as an unresolved problem is that which seeks to uncover the effect of an increasing atmospheric concentration of carbon dioxide on the earth's climate system.

Two recent publications contribute to the CO_2/climate question. They require attention because they report startlingly different conclusions and interpretations and have emerged from completely different sectors of the scientific community. The first publication is a pamphlet which is important because it emerges from the National Academy of Science's CO_2/Climate Review Panel (*Carbon Dioxide: A Second Assessment*); a panel which exists under the auspices of the Climate Research Committee of the National Research Council. The NAS report was written by a prestigious committee chaired by Professor J. Smagorinsky and a panel of eight atmospheric modelers, oceanographers and empiricists. The assessment by the Climate Research Board was undertaken some years before by a panel chaired by the late Professor J. G. Charney [hereafter referred to as the Charney Report].[1] The second publication (*Carbon Dioxide: Friend or Foe?*) by Dr S. B. Idso, is also of considerable importance, but from a completely different perspective. It is written by an energetic and distinguished scientist with a wide range of scientific interests who holds vastly contrary views to the conclusions recorderd in the NAS report. In fact, the conclusions are diametrically opposite in almost every aspect; the only agreement is that that actual CO_2 concentration of the Earth's atmosphere is increasing exponentially and the reason for the rise is of an anthropogenic nature.[2]

The NAS report follows the course set by the Charney Report and states

"... the present study has not found any new results that necessitate substantial revision of the conclusion of the Charney report ...," (p. 1, NAS).

The earlier NAS report, of course, suggested that the Earth faced substantial climatic, economic, societal and biological upheaval as a result of the increasing CO_2 concentration. On the other hand, the main thrust of Idso's book is:

"... that elevated levels of atmospheric CO_2 should have only a minor positive effect on surface air temperature, no effect at all, or even a slight negative effect ..." (Idso, p. 51),

and further that:

"... CO_2 effects on both the managed and unmanaged biosphere will be overwhelmingly positive ..." (and that) "... the rewards" (to the increasing CO_2) "... are mind boggling ..." (Idso, p. 80).

It may seem strange that two books that speak on the same subject and are different in so many ways, would be discussed in tandem. In agreeing to review the publications, I had decided to review them separately. After all, the sources were different and their conclusions divergent to say the least. On the other hand, there was a certain entwining which existed throughout the two publications. The NAS spent considerable time discussing Idso's earlier publications from which most of his controversial claims emanate Furthermore, Idso has had the opportunity not only to rebut the NAS disclaimers of his work, but also to present it as a cohesive whole. With this in mind, the simultaneous review of the papers appears reasonable.

There is also another reason. Idso's book questions not only the scientific merit of the NAS report, but also the integrity of atmospheric science itself. In fact Idso's claims that scientists, at least those who are involved with CO_2 and climate, aim at "... conducting science by decree ..." (Idso, p. 85), and that:

"... science by decree seems to be the primary characteristic of the National Academy in attempting to discredit what I have done ..." (Idso, p. 86).

Has Idso been given a proper hearing? Are, as his book claims, his ideas and observations sound, and the criticisms of his works base and devious and a concerted attack by the "... scientific establishment ..." and the climate modelers who have:

"... marshalled their forces to orchestrate the production of a report ... described as the official rebuttal of my work ..." (Idso, p. xi)

or are these words merely a veil for poorly conceived science

"... based upon incomplete methods and observations ..." (NAS2, p. 19).

As it turns out, we are in a fairly unique position to tackle the problem of such an assessment. Regarding the Academy dismissal of research portraying contrary opinion, Idso states that:

"... In all fairness, however, I must say that the Academy and the climate modelling community did not have available to them the full spectrum of my work ..." (Idso, p. 87).

Unlike the National Academy Committee we do have before us Idso's book *"Carbon Dioxide: Friend or Foe"*.

There are two aims in this review. In the first place, I will attempt to outline the arguments as they appear in the two documents. I will also attempt to determine the scientific differences occurring between the two reports. At best, we can hope to achieve a feel for the assessment of the validity of the Academy's report's view of Idso's work, or in turn, the validity of the scientific claims made by Idso's "... scientific method as distinct from" ... (the 'establishments') ... "science by decree ..."

2. The Idso Pamphlet: Carbon Dioxide: Friend or Foe?

The Idso pamphlet cannot be read without conjuring up a full spectrum of emotions.

It will be sweet food for the already converted and probably bitter swill for the critics of Idso's conception of the CO_2/climate relationship. However, for those interested in the pamphlet for purely scientific purposes and who have been lured by Idso's promise to "... set the record even straighter ...", it may be a significant disappointment. Moreover, a common disappointment, if not distaste by most readers, may be to the manner by which Idso has slurried the motivations and intent of people holding contrary views to his own. One may question the ethics involved in such actions.[3]

A more serious question is to wonder to whom the book is directed. Given its overall antagonistic tenor, it is surely not aimed at just convincing with scientific arguments of rigor those who have written the NAS2 report. As the reactions of friend and foe are thus fairly predictable, we must ask of its utility to the uninitiated in climate, those who are keen on education, probably the policy maker seeking guidance or just plainly interested. Is this a book rich in scientific argument or unsubstantiated and self-serving rhetoric? It is a central question that possesses many answers.

The book is foreworded by Sylvan H. Wittwer.[4] Here the general case for the biospheric implication of CO_2 concentration is first posed. Wittwer's primary concern appears to be that the NAS2 document has given little or no consideration to the possibility of *positive* biospheric-climate feedback; a basic tenet that is echoed throughout the book. However, it is written as an unfathomable or, at best, an incomplete philosophy. On the one hand, Wittwer sees that CO_2 enrichment of the atmosphere will promote plant yield and reduce water dependency and is bewildered that meteorologists and climatologists only inspect the negative nature of climate change. But the problem is that Wittwer appears to view the climate problem in an 'either-or' perspective and does not address the possibility that the two effects (i.e., enhanced plant growth and adverse climate effect) are mutually exclusive. If there are adverse climatic effects, as cryospheric destruction and climate zone shifts occur, then the two effects are independent and remain so until it can be shown that the biosphere can induce a negative feedback which will ameliorate any warming and adverse climate effects in general.

Wittwer's comments appear to be influenced strongly by the results of Idso (1980a, b) and Newell and Dopplick (1979) who suggested that the proposed global warming may be considerably smaller than has been suggested in NAS2. If the two effects of biospheric activity and adverse climate perturbation are independent, then the validity of the Idso/Newell-Dopplick arguments become the real nut of the controversy.

Eight chapters follow the foreword by Wittwer and a brief preface by Idso. The first chapter (*Birth of a Controversy*) presents the author's view of the chronology and the emergence of a divergence of opinion of the climate-CO_2 controversy. So contrary are the scientific opinions regarding the climate effects of CO_2 enrichment of the atmosphere that Idso commences with the statement:

"... it behooves us to look dispassionately at the whole problem in critical review ..." (Idso, p. 2).

Idso's dispassion, as it turns out, lasts but a few pages.

In this first chapter, Idso identifies research tools used to study CO_2/climate change connections. Two are identified: climate models and empirical studies. Climate models will receive scathing treatment in later chapters, principally because of the caveats that

attend the results that, according to Idso, render them inconclusive. The pervasive criticism persists despite the fact that climate modelers freely promolgate these inadequacies and expect their results to be couched in the same. Empirical study, on the other hand:

"... identifies precipitous environmental circumstances which provide natural experiments ... illustrative of how the *real* atmosphere responds to *real* perturbations ..." (Idso, p. 5).

The real problem, of course, resides within the definition of a "natural experiment ..." The use of 'natural' data does not mean in any sense that modelling has not taken place and that there may be an equally long list of caveats that have to be applied to the results of a data study. As soon as an assumption is made such that a *total derivative* must be approximated by a *partial derivative*, then a model of a natural system has been created. Two later chapters (IV: *Nature's Testimony*, and VI: *Making Our Way Back to Eden*) refer to "... natural experiments ..." that, no less than the experiments with numerical models, are thwart with interpretative and diagnostic assumptions.

The remainder of the book may be divided into three parts. The first consists of a critique of climate models (i.e., numerical models) in general, and is spread over two chapters (II: *A Journey Through Caveat Land* and III: *The Art of Swallowing Camels*). The second section (Chapters IV, *Nature's Testimony* and V, *Will the Real World Please Stand Up?*) deals with the author's own research and his interpretations of long term trends in global and regional temperatures. The final section is a mix of biospheric research, principally the effect of CO_2 on plant growth and transpiration rates. It also includes a call for an *increase* in fossil fuel burning (see Chapter VI, *Making Our Way Back to Eden*) as a means of augmenting the beneficial aspects of CO_2 enrichment. Two short chapters (IX: *Science by Decree* and X: *Final Comment*) close the book with a further questioning of 'establishment' motivations and a call for a further assessment of the problem by an independent committee.

The first section of Idso (Chapters II and III) contains a strange mixture of logic and emotion. In a *Journey Through Caveat-Land* (Chapter II) we are treated to a contemporary version of Plato's "Dialogues" in which an:

"... imaginary interview (is constructed) where I pose pertinent questions designed to introduce a variety of model shortcomings and omissions ..." (Idso, p. 16).

And the result may have been an entertaining, informative and clever vehicle to isolate model deficiencies except for the choices Idso uses as respondents:

"... the answers to my questions will be verbatim quotations from recent CO_2-climate papers ... assembled to portray the field as a unified whole ..." (Idso, p. 16).

Despite the intent of the interview, what is particularly galling are the respondents used in the 'interview' and the isolation in which the quotations are exhibited. For example, G. L. Stephens and I[5] are referred to as 'establishment' CO_2/climate modelers, whereas our study warned of the problems of excessive conclusions being drawn from models containing inadequate cloud-radiation parameterizations.[6] Of course we must ask again to whom is the report directed? Very few outside the atmospheric sciences would know of the main thrust of the work quoted by Idso or whether or not Madden,

Webster, Stephens, and Reck were or were not general circulation climate modelers. That they are not seems irrelevant. That their papers contain a suitable phrase which may be plucked out to make some point seems more relevant.

But I must restate that Idso's points are not new even if this manner of elucidation is, and the many model deficiencies and weaknesses are realized and often confessed in the literature anyway. Given that Idso's real point is that if there are so many problems with the models, why is NAS2 so adamant about their conclusions? And two vastly different assessments of numerical model capabilities emerge from the two books. On one hand, the NAS2 concludes that:

"... mathematical-physical models, whether in a highly simplified form or as an elaborate formulation of the behaviour and interactions of the global atmosphere, ocean, cryosphere and biosphere are generally considered to be the most powerful tools yet devised for the study of climate ..." (NAS2, p. 5),

whereas from Idso we have:

"... Unless there is good reason to believe that the models accurately incorporate all significant real-world processes that relate to climate determination, it would be unwise, to say the very least, to even consider acting on their implications, for we live in a *real* climate, and not some computer generated substitute of unknown or dubious questions ..." (Idso, p. 15–16).

The "*Art of Sawllowing Camels*" (Chapter III) extends Idso's criticisms of climate modelling and probably here he is on less shaky ground when discussing specific work rather than attacking a field in general. In particular, Idso concerns himself with a NASA/GISS study[7] where a 100 yr global temperature trend was modelled with three adjustable external parameters; CO_2 concentration, solar radiation variability and aerosol variability. Idso is correct in pointing out (as admitted by the authors) that of the three parameters, only the first is relatively well known. However, his strongest point comes from his noting that one has to be extremely careful with the temperature record one chooses to compare with model predictions. Idso points out that the recent work of Paltridge and Woodruff (1981)[8] shows distinctly different trends between the northern and southern hemisphere.

Chapters IV and V (*Nature's Testimony* and *Will the Real World Stand Up?*) discuss the author's own research. The chapter commences with a fiery attack on numerical climate modelling:

"... But how could such a brilliant group of scientists as I am sure climate modelers are, delude themselves – and others – so badly for so long ... a question asked by more and more people as new pieces of evidence continue to break through the clutter of theoretical predictions flowing as an ocean from the output printers of the computer modelers ... The answer ... is to be found in a journey through caveat-land; the models are all so similar and so far removed from the total reality they need to simulate that almost anything is possible..."

Somehow, from the ashes of the climate models, it would be hoped that Idso's own work and techniques will provide a clearer and more viable research alternative, perhaps providing some fresh insight and interpretation. Unfortunately, we find only disappointment. The techniques are merely alternatives with no superlative attached, and there is no insight or magic involved. Caveats are equally necessary as with the numerical climate models and the only difference between the two types of work is that Idso's caveats and approximations are unwritten!

The most disappointing aspect of the book lies within Chapter IV where Idso has the opportunity and the space to provide a very careful description of his own work. Idso presents two equations summarizing his "natural experiment" in which an air temperature response function is defined and an "experiment" described that relates daily minimum air temperature at one location to concurrent fluctuations in atmospheric humidity. But as air temperature is a multi-variate function (i.e., it depends upon other factors besides atmospheric humidity), Idso's "natural experiment" reduces merely to a model of the atmophere simply because he has replaced the total derivative with a partial derivative. In his second experiment (a "... synthetic experiment ...") Idso considers the comparison of the Earth without an atmosphere to the real world equilibrium situation. The question to be asked thus becomes clearer. What are the differences between Idso's model (as that is what it is!) and the models criticized by Idso earlier? Except in detail, very little! It is not a question discussed by Idso.

To some extent, the problems of Chapter IV are outlined in the only figure which pertains to Idso's own research. Supposedly it shows relationships among air temperature, vapor pressure and infra-red emissivity of the atmosphere plotted in sets of (surface?) vapor pressure ranges for Phoenix, Arizona. The diagram is supposed to provide "... aids in visualizing this phenomena ..."; the phenomena being the possibility of some form of a negative climate feedback. The relevance of the data to the argument of a negative feedback existing is not explained or discussed in the text, nor is the relevance of spot observations made at Phoenix to the global climate. But even more frustrating is the technical quality of the diagram itself. The reduction in size of the diagram is so great that the various clusters of points are not differentiable even with a magnifying glass so that even if there were a cross referencing between the text and the diagram, it would not matter. However, as far as I can detect, for Idso's purpose the diagram is supposed to show that under the auspices of the Clausius-Clapeyron equation, a number of climate feedbacks are possible. After considerable study, I am still not sure if the diagram states anything else except that saturated vapor pressure is solely temperature dependent and that, to a large extent, the downward component of infrared radiation at the surface is a function of the moisture content of the boundary layer and its temperature.

In summary, Idso's earlier promise "... to set the record even straighter ..." and the closing statement of Chapter IV:

"... That is the testimony of nature and it is more impressive than all the lofty pronouncements of all the world's science academics combined ..." (Idso, p. 51)

both ring hollow and hang lossely when the full content of Chapter IV is considered. It is sad when the very chapter of the book that should be the best and most carefully prepared turns out instead to be the worst.

Idso's fifth chapter (V: *Will the Real World Please Stand Up?*) extends his arguments to the global domain. Given an exponential increase of CO_2 concentration over the past century, Idso makes an arbitrary demarcation between a 'gradual' CO_2 increase and a 'rapid' CO_2 increase noting from the data study of Paltridge and Woodruff (1981) that in all regions except the deep southern hemisphere, the atmosphere appears to have undergone a slight cooling despite the more rapid CO_2 increase of the last few decades. Of

course, one must agree that such a departure of trends may point towards a physical inconsistency in the arguments of proponents of a positive relationship between CO_2 and global temperature. Besides the problems of data representativeness itself, there are the considerations of other factors which may introduce temperature variability into the system. That the southern hemisphere and the northern hemisphere are somehow phase lagged in temperative variation, as pointed out by Idso, probably would be suggestive of the importance of other factors such as an oceanic role in the state of the climate. A prudent investigation may stop at this stage with some well founded skepticism of seeking many causal relationships from extrapolation of data. Unfortunately Idso chooses a contrary route and suggests that the data is showing that the CO_2 concentration increase in the atmosphere may be *decreasing* the temperature of the earth![9]

In the final section of the book, Idso discusses a series of biological experiments in which the effect of CO_2 enrichment is studied in detail. Here the many biological benefits of a CO_2 increase are discussed. But what is not proven is the link between beneficial biological effects and a beneficial climatic effect. It is one thing to discuss the effects of CO_2 enrichment in a biosphere *confined* within controlled greenhouse conditions. It is another to extrapolate these benefits to a biosphere that is subject to a changing climate.

Final chapters rehash the issues of the virtues of "natural experiments" versus "models" and "true science" versus science by decree". The book closes with a call for a new and concerted effort in that

"... We must forget whatever petty personal reasons we may have for blindly defending our views over the past years and objectively tackle the problem anew ..."

Such words appear as an incongruous ending and somehow do not emulate the tenor of the sentiments reflected throughout the Idso document.

3. The National Academy of Sciences Second Assessment

As noted in the Introduction, NAS2 provides no new surprises beyond the earlier conclusions of NAS1 (the Charney Report). In discussing their conclusions, the NAS committee summarized the principal issues in modeling studies. Given the basic problems involved in empirical studies they once again conclude that the modeling strategy is probably the best technique available for assessment of the climate-CO_2 relationship. A whole hierarchy of models is discussed ranging from the one-dimensional radiative convective models to the large and complex general circulation numerical juggernauts.

NAS2 also provides its own tour of 'Caveat Land' highlighting the weaknesses of the large scale models and providing estimates of their importance together with an assessment of the degree to which their omission or poor representation could effect the CO_2/climate relationships. Such features, of course, include cloudiness and ocean processes: the latter emphasized as a very important omission in climate models.

It it stated in NAS2 that two issues promt the need for a "... second assessment ..." In a preface the Chairman of the CO_2/Climate Review Committee states that as a first reason that:

"... an update stems from the accelerated research activity on the many facets of the problem stimulated by vigorous national and international public attention ... (and that in addition to the subject matter of the Charney Report) ... new considerations have been included, reflecting a broadened base of understanding the problem ..." (p. XVII, NAS2)

and as a second reason that new research has suggested very different conclusions to those of the Charney Report which require discussion. Specifically:

"... The report discusses at length two recent studies that have concluded that the effect of increased CO_2 on surface temperatures will be much less than estimated by the majority of the scientific community ..." (p. XVIII, NAS2).

In summarizing and appraising these efforts:

"... the panel believes these studies are flawed and incomplete, and the report attempts to identify their deficiencies ..." (p. XVIII, NAS2).

It is this latter issue that is the common point of the two publications and that we will continue to emphasize in this review. In addition, we will discuss the substantive additions to the NAS1 which are the "Prediction and Scenarios of Climate Change" (Section 3, NAS2) and the "Development of Monitoring and Early Detection Strategies" (Section 4, NAS2). Furthermore, we will consider the committee's suggestions regarding "empirical studies" as these are, of course, in essence the "natural experiments" of Idso.

It is in Section 2 ("Principal Scientific Issues in Modeling Studies") that the committee addresses the "... Dissenting Inferences from Energy-Balance Models and Empirical Studies ..." referring specifically to Newell and Dopplick (1979) and Idso (1980a, b) and (1981), all three of which are summarized in Idso. I believe that the committee did not have Idso (1983) and, obviously, Idso's *Carbon Dioxide: Friend or Foe*? (which offered "... the full spectrum of my work ...") and, consequently, Idso's suggestion of a CO_2 induced earth cooling scenario. In all, the report spends some four or five pages in a detailed rebuttal of the dissenting viewpoint stating that

"... These papers are based upon incomplete methods or observations and their conclusions appear to be of limited utility in assessing the climate effects of increased CO_2 ..." (NAS2, p. 19).

This conclusion arises from an eight point list of interconnected processes that the Committee states are necessary for a study, model or empirical, to address in order to determine the effects of increased CO_2. The dissenting papers are noted as a failing to address these points in some way or another.

The eight-point list of interconnected processes includes radiative effects at the surface and in the atmosphere including the resultant changes in surface sensible and latent heat fluxes, similar changes due to changes of moisture in the atmosphere and the thermal inertia of the land surface, the atmosphere ("... and especially ...") the ocean. Furthermore:

"... It is within this context (i.e., that all of these processes must be taken into account in calculating new equilibrium conditions) that we consider the cited studies ..."

For one holding a dissenting view, one could easily call foul at the conditions of assessment as, surely, no technique, be it empirical study or climate model, has measured up to these criteria irrespective of the conclusion that has been reached.

In any event, Idso's response functions are discussed relative to the above criteria. Basically, the arbitrariness of Idso's observational strategy (e.g., place and time of observation) and the mismatch between time scales of 'anticipated' changes due to CO_2 increase and the length of the various experiments are emphasized as problem areas. Furthermore, the use of data from one locale (e.g., Phoenix) to infer a global scale conclusion is criticized.

NAS2 argues againts Newell and Dopplick (1979, 1981) research in much the same manner. In the first place, NAS2 reiterates that the CO_2 problem is of a global scale so that a local change (in Newell and Dopplick case, the tropics) does not necessarily refute the conclusion of a global assessment. They further argue that there are inconsistencies in the model assumptions in that relative humidity and atmospheric temperature are held fixed while calculating the increase in surface temperature needed to increase heat fluxes from the surface enough to counterbalance the additional energy input from the atmosphere due to CO_2 enhancement.

In a sense the criticism of the Newell-Dopplick arguments must be put in perspective. For example, if we consider the one dimensional models (Section 2) discussed by NAS2 in Section 2, probably all would fail relative to the 8-point list. General circulation models or climate models would also have a hard time despite their energetic consistency and complexity and global domain probably because of assumptions relating to the ocean. One still has to rely upon parameterizations to determine the sign of a feedback even if it is felt that the sign and magnitude are fairly well understood. But it must be remembered that until processes are represented in their full physical regalia, these determinations usually rest on phenomenological arguments where some quantities are held constant while others are allowed to vary. In that same sense, they possess the same strengths and weaknesses of the arguments of Newell and Dopplick.

It is quite probable that the case against the dissenting views is valid. But rather than being considered as merely eliminating dissenting opinion, I would rather look at the discussion of the contrary opinions in a broader sense and that is of highlighting the difficulty of the assessment of changes of a complex system that is dominated by interlocking processes.

The problem of determining and constructing meaningful empirical studies requires special attention and this is addressed by the Academy Report. In the last section of the NAS2, a broad plan is laid out for the "Development of Monitoring and Early Detection Strategies." As a basis, it is argued that as the changes in global temperature may result from a number of non-CO_2 causes and as a consequence:

> "Climate variations due to these non-CO_2 influences must be quantified to the extent possible in order to permit the climate changes attributable to CO_2 to be identified." (NAS2, p. 62)

For the observations necessary to achieve this very desirable result, observational programs are proposed that will monitor temporal variations in such quantities as insolation and terrestrial radiation variations, aerosol concentrations as well as changes in surface albedo due to desertification and vegetation changes, etc. Unfortunately, the accuracies required for such observations that would allow a signal to emerge out of the noise is *not* addressed at all. Nor are the difficulties due to lack of understanding of some very basic physical

principals (e.g., the form, type and radiative properties of tropospheric and stratospheric aerosols) that would allow a meaningful climate experiment to be designed. We are left with a series of wishes, but not enough indication to assess if the problem is soluable from a theoretical or observational tack, or neither.

Thus, when we consider the prime focus of the chapter, the development of an *early warning strategy* and are faced with the statement:

> "... it is clear that the early detection of the CO_2-climate signal requires not only a prediction of the CO_2-induced climate change but also a knowledge of the natural climate variabilities. Therefore, it is necessary to determine (from the past climatic records) the variability of relevant climatic variables such as temperatures of the atmosphere and the oceans ..."

questions of the practicality of the endeavor come into focus. Even if a sufficiently long history of surface temperature is known with sufficient accuracy (and the critical adjective is sufficient), what of the reconstruction (again with *sufficient* accuracy) of the causal functions such as insolation, albedo (ice and land) and aerosols? Problems increase, of course, if it were to be found that oceanic responses to a perturbed radiation flux at the surface due to CO_2 and non-CO_2 effects were of importance *below* the surface. What then of meaningful reconstructions or measurements? The NAS2 does not provide a convincing argument nor an empirical study strategy for the deciphering of "natural" climatic variability, let alone an "early warning" system for CO_2 induced climate change.

4. Some Comparisons and Conclusions

Two issues appear to separate the Idso and NAS2 documents. In the first instance is the very major scientific difference pertaining to the sign of the climatic response to the increase of CO_2. The second refers to the definition of a model and an empirical study. Idso's main evidence of a negative CO_2/climate relationship comes from the recent trend in global temperature being negative even though the CO_2 concentration is exponentially increasing. The NAS2 evidence for a positive relationship stems from basic radiation theory and climate model simulations which employ characteristics of the CO_2 and H_20 absorption of long wave radiation.

Personally, I have great problems and reservations in the manner in which Idso gleans a global cooling from the time series of global mean temperature data. If one were to extend the data series further back than the 1880 lower limit chosen by Idso, one would find periods of distinct temperature fall which would at least equal that which has occurred over the last few decades (e.g., the Little Ice Age period). Thus, if one states that a fall in temperature since 1945 occurs because of an increase in CO_2, how do we explain the temperature falls that occurred when there was no corresponding increase of CO_2? Of course these arguments here could be turned around and used relative to research that proposes increases in global temperature with increasing CO_2. They also, it may be suggested, have to account for previous, preindustrial temperature rises. I suppose, though, that the difference between the explanations depends upon whether there exists a physical hypothesis that establishes an *a priori* expectation of a sense of a climate change; a causal relationship that helps sort out forces response from climatologi-

cal noise. The proponents of an increasing temperature relationship with CO_2 have a physical hypothesis that at least suggests the sign of the relationship. However, to establish that the magnitude of the effect will emerge above the climatological noise will require a very long period data set and probably global empirical studies. As we mentioned previously, NAS2 proposed that such a sorting out be addressed via empirical and numerical studies, but does not exactly say how.

On the other hand, the NAS2 possesses a dichotomy in that it forcably states that the sign and magnitude of CO_2 induced effects are fairly well established, yet calls for continuation of basic research and an extension into more elaborate and sophisticated empirical studies. And the call for continued *basic* theoretical research is probably very valid. Recently, Kiehl and Ramanathan (1982)[10] considered the effects on the radiative heating due to increased CO_2 by the H_2O continuum absorption in the 12–18 μm region. In this band, the CO_2 absorption bands are overlapped by the H_2O pure rotational band and the H_2O continuum band. Most climate impact studies have only considered the former H_2O effect. Kiehl and Ramanathan show that the downward long wave surface radiative heating due to CO_2 doubling is reduced from about 1.5 $W m^{-2}$ to 0.1 $W m^{-2}$ over greater than half the area of the globe when H_2O continuum overlap is considered in conjunction with the H_2O rotational band in overlap with the CO_2 absorption bands. In terms of changes to the net heating of the troposphere, the effect accounts for only about 4.5% reduction. But the Kiehl–Ramanathan paper indicates a distinct redistribution of radiative heating including considerable surface effects. What effects the changes in surface heating have on the long term oceanic interaction or the more straightforward effect on the surface energy fluxes requires considerable attention. Probably considerable experimentation is necessary and certainly discussion by the CO_2/Climate Review Panel. It is a pity that the Kiehl–Ramanathan paper was not available for discussion. Even so, it underlines the need for continued research in the very fundamental cornerstones of the CO_2/climate positive feedback process. Even if the sign of the change is established, there still may be some calibration required regarding the magnitude of the change.

With regard to the second major difference between the two reports, the important factor, of course, is the soundness of the assumptions that attend the models ('natural' or empirical) and the ability of the experimenter to establish a control case. In that sense, there is *no* difference between numerical and empirical studies. With numerical models, the usual problem deals with the inadequacy of the various approximations or parameterizations. Controlled experiments are their greatest virtue, if, of course, you have faith in the system they represent. With pure data or diagnostic studies, you still have to make the assumption that the data stream is representative, in a statistical manner, of some longer term reality and not a manifestation of random meteorological variability. Having established the statistical representativeness of the data, one is still faced with the major problem of any diagnostic study; the establishment of a control for the " . . . natural experiment . . . " I am aware of only one empirical climate study (Ohring, *et al.*, 1981[11]) that can claim to have established a real control. The criticism raised by Idso towards numerical modelling can thus be turned around. Is the lack of controls involved in the 'natural experiments' any less damming in the establishment of a causal relationship (or lack of it) than the weaknesses of the numerical model

simulations of climate? A common feeling is that both techniques possess weaknesses, but each shows a certain utility. However, there are few who would claim that either technique would possess the magical properties suggested by Idso for empirical studies.

In the preceding paragraphs, it has been difficult not to refer to the vastly different styles of the two reports. The NAS2 follows the very professional tone of Academy reports and, despite the few shortcomings discussed earlier, provides a rather up-to-date and unemotional discussion of generally accepted concepts of the CO_2/climate relationship together with a rebuttal of contrary opinions. Idso's document, on the other hand, is vigorous, very personal and emotional. Its content also varies in quality. Its highlights are a reminder of uncertainties in our treatment of the historical data and its weaknesses and greatest disappointment is its failure to clearly elucidate Idso's own ideas. Perhaps one can excuse Idso's emotion in that it probably emanates from the general rejection of his own ideas or, even worse, the ignoring of them. In a recent publication by the Australian Academy of Science, Tucker (1981)[12] completely ignores the papers of Idso and Newell and Dopplick cited earlier. Surprisingly, for a scientific paper, there is a distinct lack of critical discussion of contrary hypothesies. With such omissions in mind Idso appears much more reasonable as the questions relating climate and CO_2 concentration are not sufficiently resolved that we may ignore accepted procedures of critical discussion. On a more positive note, Tucker did discuss at length the possibility of biomass enhancement and the possibility of a feedback to the climate state, the cause championed by Idso.

It is very doubtful that the controversy will end with these two reports and probably it should not. It is doubtful if full answers will emerge until we improve our understanding of oceanic processes from both modelling and empirical methodology. Clearly neither technique is capable of providing answers at this time and probably we will have to wait for the global observations of the World Climate Research Program, especially the World Ocean Climate Experiment to provide data for empirical studies and for model validation.

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Notes

1 Climate Research Board (1979), Carbon Dioxide and Climate: A Scientific Assessment, National Academy of Sciences, Washington, D.C. 22pp.

2 It will become obvious in the paragraphs which follow that the majority of the arguments that are contained within Idso's book are his own. However, it would be misleading to indicate that Idso alone holds contrary views. Beyond Idso's work, Idso* (1980a, b, 1981, 1983) stands Newell and Dopplick* (1979, 1981), Choudhury and Kukla* (1979) and Ellsaesser* (1983). Whereas this bibliography is far from complete, these will give the reader some appreciation for the breadth of the dissenting opinions.

*Idso, S. B.: 1980a, 'The Climatological Significance of a Doubling of Earth's Atmospheric Carbon Dioxide Concentration', *Science* **207**, 1462.

Idso, S. B.: 1980b, 'Carbon Dioxide and Climate (Reply to Schneider, *et al.*, 1980) and Leovy (1980)', *Science* **210**, 7.

Idso, S. G.: 1981, 'Carbon Dioxide, an Alternative View', *New Science* **92**, 444.

Idso, S. B.: 1983, 'Do increases in Atmospheric CO_2 Have a *Cooling* Effect on Surface Air Temperature?', *Climatological Bulletin* **17**(2), October 1983.

Newell, R. and Dopplick, T.: 1981, 'Reply to Robert G. Watts' Discussion of "Questions Concerning the Influence of Anthropogenic CO_2 on Atmospheric Temperature" ', *J. Appl. Meteor.* **20**, 114.
Choudhury, B. and Kukla, G.: 1979, 'Impact of CO_2 on Cooling of Snow and Water Surfaces' *Nature* **280**, 608–671.
Ellsaessar, H. W.: 1983, *The Climate Effect of CO_2: A Different View*, Proceedings, Second Conference on Climate Variations, January 10–14, New Orleans, LA.

[3] There are a number of underhanded and unsubstantiated slights of the scientific community throughout the text of Idso. For example, on p. 9 we find: " . . . immediate and vociferous response from the scientific establishment was that the publication of my report in *Science* coincided with the U.S. Senate hearings on the future of research in atmospheric sciences; and my conclusions were held up as " . . . being contrary to the reasons they were citing as justification for major funding requests . . ." As evidence of this accusation, he quotes a personal communication (footnote 29, p. 13, Idso) and the comments of an anonymous reviewer of a rejected paper. It is unfortunate that Idso needs to rely on what must be called fourth order non-standard references to substantiate such allegations. But at best, such comments are churlish and will receive no further discussion in this review.

[4] Wittwer's credentials are impressive. Besides being the director of the Agricultural Experimental Station and Associate College Dean at Michigan State University, he is also a member of the Climate Board of the NAS; the board which charged the CO_2/Climate Review Panel with their task.

[5] G. L. Stephens and Webster, P. J.: 1981, 'Clouds and Climate: Sensitivity of Simple Systems', *J. Atmos. Sci.* **38**, 235–247.

[6] See also: Webster, P. J. and Stephens, G. L.: 1980, 'Gleaning Carbon Dioxide-Climate Relationships from Model Calculations', in *Carbon Dioxide and Climate: Australian Research*. Australian Academy of Science, Canberra, pp. 185–195.

[7] Hansen, J., Johnson, D., Lacis, A., Lebedeff, S., Lee, P., Rind, D., and Russell, G.: 1981, 'Climate Impact of Increasing Atmospheric Carbon Dioxide', *Science* **213**, 957–966.

[8] Paltridge, G. and Woodruff, S.: 1981, 'Changes in Global Surface Temperature from 1980 to 1977 Derived from Historical Records of Sea Surface Temperature', *Mon. Wea. Rev.* **109**, 2427–2434.

[9] See also, Idso, S.: 1983, 'Do Increases in Atmospheric CO_2 Have a *Cooling* Effect on Surface Air Temperature?', *Climatological Bulletin* 17(2) October.

[10] Kiehl, J. T. and Ramanathan, V.: 1982, 'Radiative Heating Due to Increased CO_2: The Role of H_2O Continuum Absorption in the 12–18 μm region', *J. Atmos. Sci.* **39**, 2923–2926.

[11] Ohring, G., Clapp, P. F., Heddinghaus, T. R., and Krueger, A.: 1981, 'The Quasi-Global Distribution of the Sensitivity of the Earth-Atmosphere Radiation Budget to Clouds', *J. Atmos. Sci.* **38**, 2539–2541.

[12] Tucker, G. B.: 1981, 'The CO_2-climate Connection: A Global Problem from an Australian Perspective, Australian Academy of Science, Canberra, Australia.

CORRESPONDENCE

COMMENTS ON J. T. KIEHL'S REVIEW OF JOHN GRIBBIN'S BOOK 'FUTURE WEATHER AND THE GREENHOUSE EFFECT' (*Climatic Change* **5**, 421–422)

Speaking of Dr Gribbin's description of my work on the greenhouse effect of carbon dioxide, Dr Kiehl claims that it 'truly shows that he has not done his homework', based upon Dr Gribbin's acceptance of 'two fallacious arguments due to Idso'. May I suggest that it is Dr Kiehl who has not done his homework, and that it is he who is creating the fallacious argument.

In describing one of my 'natural experiments', as I like to call them, Dr Kiehl states that I compute a surface air temperature sensitivity based on a change in *outgoing* longwave energy at the *top* of Earth's atmosphere. Nothing could be further from the truth. For the case in point – the 34 K temperature difference between the present Earth and a hypothetical Earth identical in all respects but lacking a longwave radiatively active atmosphere – I have clearly stated in four separate papers (Idso, 1981, 1982a, b; Kimball and Idso, 1983), as well as in my own book on the subject (Idso, 1982c), that the impetus for this temperature change is the current mean rate of *incoming* longwave energy at the *bottom* of the Earth's atmosphere. And I have made explicit reference to this natural experiment in still other publications (Idso, 1982d, 1983). Surely anyone so bold as to describe another's arguments and state that their consequences are 'totally without meaning' should be expected to do *his* homework and at least read one original account of those arguments by their author. On this point Dr Kiehl appears to fall far below the level of the person he criticizes, and in so doing he additionally perpetrates a blatant falsehood.

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