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Current State of Knowledge in Global Change

Representative Walker , Members of the Committee,

I am grateful to the committee for the opportunity to express my views on the science of Global Change, but I must immediately qualify my remarks by admitting that the subject is so broad that I cannot pretend to any degree of comprehensiveness. My remarks represent my personal observations, and are colored by my own research interests which focus on the theoretical foundations of our understanding of climate. I hope I may be forgiven if I claim that this topic is at the foundation of the broader topic.

My remarks are both positive and negative. On the positive side, I am happy to report that there is substantial progress being made in dealing with fundamental questions such as

- ▶ What is the sensitivity of climate to changing CO₂?
- ▶ What are the mechanisms by which climate change can occur?

I will discuss these matters shortly. However, I will begin with the more negative issue: namely, it is not obvious how either the USGCRP or MTPE have explicitly contributed to this progress. In order to explain this, one must briefly consider the historical, political, and institutional problems that have beset Global Change research. I will begin by considering EOS. This was a massive program which began under some remarkable circumstances. One might naively suppose that such an ambitious program was being undertaken because the science had matured to the point where resources were needed to make progress in settling well focussed basic questions of established importance. On the contrary, NASA appears to have begun with platform decisions and an emphasis on existing technology employed in highly expanded form. NASA's approach would have been eminently suitable to explicit engineering tasks like the Apollo and Manhattan Projects, where the underlying science existed to the extent that focussed aims could be addressed. However, EOS was not specifically an engineering program, and the underlying science was in a primitive state. The stated aim appeared to be to examine all aspects of the Earth System in the hope that both questions and answers would emerge from the massive flow of data. Such an approach can, under some circumstances, be justified. Indeed, much of what we currently know about the climate system has depended on serendipitous measurements taken for other reasons. Thus the Marshall Islands Nuclear Tests provided the meteorological data that led to much of our current understanding of tropical meteorology. This understanding permitted us to undertake the subsequent Atlantic Tropical Experiment of the Global Atmospheric Research Program in the 1970's. However, the Marshall Islands Tests had a compelling rationale independent of the resulting atmospheric science. We cannot say the same

for EOS. From the beginning, EOS science teams were chosen to support instruments rather than to do science which would be assisted by the instruments. EOS provided no support for developing the basic science that might provide a foundation for the program. While it might be argued that such science was the appropriate object of NSF efforts, there was no provision for any coordinated utilization of basic science. Indeed, given the massiveness of the proposed program, it might be argued that it would be embarrassing to admit the relative absence of foundations, and, as a practical matter, EOS was too clumsy to be responsive in any case. The result has been an inability to rationally prioritize, and a vulnerability to radical restructuring and downsizing which seems to have had little impact on the science.

The situation became significantly more complicated as concern developed over global warming. The success of the environmental movement in popularizing this issue provided an after the fact rationale for what became known as MTPE, and an expansion of the effort to other agencies under the aegis of the USGCRP. This led in several ways to a defensive rather than a scientific approach. A major source of support was seen as depending on the perpetuation of an issue rather than on a focussed attempt to solve basic questions in a prioritized manner. Politicization contributed to this by establishing that agreement as to the possibility of crisis constituted public virtue, while scientific questioning was frowned upon (to put it mildly). The situation has been compounded by the desire of a variety of disciplines ranging from economics to trace gas chemistry to medicine to partake in the program. Such participation presupposes a well established problem, and leads to little interest in actually assessing this - especially among scientists whose interests are dependent on the existence of the basic problem. There is also the massive increase in university bureaucracy (nominally associated with the needs mandated by federal guidelines) whose needs are dependent on federal support, but whose immediate concern for science per se is limited.

The widespread insecurity within the scientific community following the end of the cold war also acts to distort normative scientific approaches. There is a conviction that funding is based on fear and would not survive the actual solution of the basic problems or the finding that some problems may not be as serious as supposed. The situation has persisted for so long that we now have a generation of scientists for which this situation seems normal. The political system seems to have had difficulty recognizing the importance of security and stability to the proper functioning of science. This contrasts strongly with the twenty year period following WW II, which, in many ways, constituted the golden age of American science. The issue is not simply one of scale. The level of support is far higher today than it was during the earlier period.

It must be emphasized that the role of observations is both to uncover phenomena, which must then be analyzed by means of theory, and to test the resulting theory with an explicit aim of falsifying rather than confirming the theory. In this crucial regard, scientific inquiry differs profoundly from legal advocacy - a frequently misunderstood point. With respect to MTPE, the aim must not simply be "better, faster, cheaper," but must be to place legitimate scientific inquiry at the center of the program. Fortunately, there need be no contradiction among these aims.

Having begun with a brief litany of structural problems, I must note that the ultimate spirit of

science is still alive and well. Scientists still want to know how things work. They continue to ask questions, and huge amounts of data are available – even without additional programs. Indeed, it is a shortcoming of most new programs that they fail to give adequate attention to existing data. With respect to global change, the most obvious question is what is the gross sensitivity of the climate to increasing CO₂? It is curious how little attention has been paid to this basic quantity. After all, should it prove much smaller than what current models produce, then many of the remaining concerns over consequences of warming would prove less urgent.

In principle, this is an easy quantity to define. It is generally argued (though not without caveats) that doubling CO₂ would lead to an increased radiative forcing at the top of the atmosphere of 4 Watts per square meter. The question of sensitivity boils down to asking how much must the globally averaged surface temperature increase in order to get rid of this additional 4 Watts per square meter. This is what is being referred to when one says that one expects a doubling of CO₂ to lead to an equilibrium warming of from 1-5°C. Unfortunately, the expectation depends on weak aspects of current models where uncertainties are many times larger than 4 Watts per square meter. Currently, most assessments of models consist in model intercomparisons which is methodologically questionable. It would obviously be better to have actual measurements against which to test models. In principle this can be done. The global mean temperature fluctuates from year to year naturally. One can measure the top of the atmosphere radiation budget in order to see how it changes in response to the observed changes in global mean temperature, thus yielding an observed value for the sensitivity. A preliminary attempt along these lines was made over a year ago by M.-D. Chou of NASA/Goddard, and he found a sensitivity that was about a tenth of what models suggested. However, the situation is not that simple. Chou used ERBE data and considered averages over only the tropical Pacific.

As I noted in a paper to appear in the Proceedings of the National Academy actual measurements of sensitivity would have to consider global means, and even then, the sensitivity will depend not only on the change in global mean temperature but also on its pattern. Nevertheless, I suggest that one should, at least, be able to obtain an upper bound on sensitivity. Moreover, one will inevitably get fundamental diagnostic quantities for testing models. With respect to the last item, C. Covey of DOE's Lawrence Livermore Laboratory has performed a preliminary comparison of model outputs with Chou's observations, and has confirmed the model tendency to overestimate sensitivity. At the moment, the main problem in going ahead with a better analysis is the absence of suitable data on surface emissivity over land. There is some possibility that data exists from which this may be extracted, but it is uncertain as to whether accuracy will be sufficient. In any event, there will be a small meeting in April at MIT where a few scientists from NASA, NOAA, DOE, JPL and various universities will gather to see what data is currently available to make such a study and to critically assess the proposed methodology. I am hopeful that the effort will succeed, but if it does not, we will know more exactly where we must improve our observations.

I should add that according to the work by Chou and by myself, the crucial factor in climate sensitivity seems to be the behavior of atmospheric water vapor (far and away the most important greenhouse gas, and completely natural) in dry cloud free regions where a 4% change

in relative humidity leads to a change in the radiative flux of 4 Watts per square meter. Until very recently, our knowledge of the behavior of water vapor was severely restricted by the absence of reliable data; uncertainties and errors exceeded 20%. However, in the last two years a number of things have changed this situation significantly. The instruments used on meteorological balloons have improved as has their calibration algorithm. NASA is obtaining upper tropospheric humidities from SAGE II limb sounders, and the Department of Defense's 183 GHz microwave sounder on SSM/T-2 is allowing Roy Spencer and Dan Braswell at NASA/Marshall to prepare beautiful daily maps of water vapor over the whole earth. What we see is differing in important ways from what models are producing. A crucial aspect of the difference was identified by Sun and Held at NOAA's Geophysical Fluid Dynamics Laboratory where they found that the variations in water vapor at upper levels and at the surface were far more tightly coupled in models than they are in nature. This almost certainly points to numerical problems. Nevertheless, the same huge global programs which argue for the inclusion of ecologists, economists and physicians rarely point to the far more fundamental need for applied mathematicians to put the models on a sounder basis. Fortunately, this does not mean that mathematicians will not involve themselves; the problem is challenging enough to attract attention even without programs.

Turning to the second scientific item I mentioned at the beginning of my testimony, the fundamental problem here is to account for the major changes in climate that are known to have occurred in the past. Were such changes necessarily associated with changes in net radiative forcing, or are there basic mechanisms whereby such changes can occur independent of any net radiative forcing or of sensitivity to such forcing? In 1990, I published a paper in the Bulletin of the American Meteorological Society which pointed out that the atmosphere was very inhomogeneous with respect to its main greenhouse gas, water vapor, and that the earth's surface did not cool primarily via radiation but rather via evaporation and motion. The motions act to carry heat both poleward and upward where diminished water vapor permits heat to escape more readily via radiation. The obvious consequence of this is that if we do not accurately model the dynamic heat transport, we cannot calculate the mean temperature of the earth. No one in the atmospheric sciences would argue with this; it is absolutely basic. Rather, members of the modelling community have argued that the models do well with such transports, and that there is no major problem here. However, extensive model intercomparisons conducted through DOE's AMIP program have shown wide differences among models and between models and observations. These differences also represent uncertainties and errors greatly in excess of the contributions from doubled CO₂.

A consequence of the mean temperature depending on dynamic transport is that there might be climate change in the absence of mean forcing. Motions depend on horizontal variations in heating rather than mean heating, and such variations occur for a variety of reasons ranging from ENSO events (dependent on the interaction of the atmosphere and the oceans) to variations in the earth's orbit. The motions responsible for carrying heat consist in a large scale circulation in the tropics, known as the Hadley Cell, and transient eddies in the extratropics. In a pair of papers in 1988 and 1992, A. Hou (of NASA/Goddard) and I established that changing positions and patterns of heating could greatly alter the intensity of the Hadley Cell; it was also noted that

the Hadley Cell could be a major source for the extratropical eddies. Since then, W. Pan and I have established that orbital variations strongly modulate the Hadley Cell providing a possible link between orbital variations and ice ages. A. Hou and E. Chang at MIT have established that variations in Hadley intensity can alter polar temperatures, and Hou has dramatically confirmed this with NASA/Goddard analyzed data. The alteration of Hadley intensities by ENSO events has been established, and the relation of Hadley intensity to the intensity of extratropical planetary scale eddies has been observed by Hou as well as by Chen and van den Dool at NOAA. J.M. Wallace and his students at the University of Washington are finding that significant parts of observed global warming may, in fact, be associated with ENSO patterns. In a recent paper, I have shown that the mixing by extratropical eddies strongly conditions the response of the atmosphere to stationary forcing (as is provided by major elevations like the Himalayas and land-sea differences) which, in turn, determines storm paths. Strong evidence exists that existing models are failing to replicate this behavior, and efforts are beginning at NASA/Goddard to see how this can be remedied. P. Stone, at MIT, has quantified the failure of models to mix properly, and C. Giannitsis at MIT has developed a possible simple diagnostic by showing that mixing determines the position of a major circulation feature, the subtropical jet. In brief we now can be quite certain that the atmosphere (especially when coupled to the oceans) can undergo significant variations in mean climate even without external forcing. In ascertaining this, important possibilities have emerged for improving models for both climate and weather.

What can we conclude from the above. Although I have only focussed on two basic questions which I am intimately involved with, it is evident from these examples that significant advances in our understanding of Global Change are occurring with substantial, and generally unplanned, cooperation among a variety of scientists. It is equally clear that much of the progress is occurring quietly in areas that are not amenable to easy popularization. Most important, I hope to have demonstrated that the scientific community has the capacity to focus in prioritized manner on the basic science of Global Change regardless of whether the large planning bodies do so. The main contribution that the government can make to this science is to maintain the health and integrity of the scientific community – a task which is distinct, in many ways, from the maintenance of specific programs. A healthy scientific community can make progress even without major programs, though in some cases the resources required will benefit from larger efforts. Large programs without a healthy and creative community are almost certain to prove wasteful, and program planning from above by individuals not closely and personally involved in successful research is likely to miss those seemingly esoteric details that form the foundations of science. It can never be forgotten that instruments and programs do not answer questions; individual scientists do.