COMMITTEE ON SCIENCE

U. S. HOUSE OF REPRESENTATIVES

Hearing on New Directions for Climate Research and Technology Initiatives
April 17, 2002

Testimony of Radford Byerly, Jr.

Mr. Chairman, Members of the Committee;

Thank you for inviting me to testify on the US Global Change Research Program. For more than twenty years I have observed and participated in the program and predecessor efforts. For example, I was the principal House staffer drafting and handling the National Climate Program Act, which this committee reported out and which eventually became public law in 1978. Similarly, I was the lead staffer in this committee's work on the "ozone -Freon" issue and drafted the language on "Stratospheric Protection" which the committee put into the Clean Air Act Amendments of 1977. I participated in the regional meeting at Colorado State University for the National Climate Impact Assessment in 1997 and the national meeting the next year in Washington. I was a reviewer of the recent National Research Council "Pathways" report. [NRC 1998] Finally, in 1989 I wrote an article on "The Policy Dynamics of Global Change" which still seems relevant. I would like to submit it for consideration for inclusion in the record. [Byerly 1989] Were I to write it today I would give more emphasis to the involvement of users.

The reason I have worked in this program is that I believe global warming is a serious threat. That we are significantly increasing atmospheric CO2 is incontrovertible, and it would be surprising if this had no effect, or only benign effects. And of course, like many if not most citizens, I have personally experienced the greenhouse effect: In Colorado where I live the air is very dry; it contains little water vapor (and of course water vapor is a greenhouse gas). Because of this when the sun goes behind the mountains to the west of Boulder, the temperature quickly begins to drop. In the summer it can be in the high nineties, and it takes a while for the buildings, the ground, and pavements to cool, but by the next morning the temperature is 50 degrees. In Houston where I grew up, the air is full of water vapor, so when the sun goes down the night remains warm. Of course there is some cooling, but by morning the temperature may be still in the seventies. This is a greenhouse effect. The difference between water vapor and CO2 is that water vapor is regularly removed from the atmosphere as rain or snow, while CO2 is not, it remains and accumulates. So greenhouse warming seems likely.

Further the physical record of the past, including ice cores, and pollen and fossil records, makes clear that climate changes all the time, sometimes fairly rapidly. Even before humans were a factor there were large swings in climate. The human historical record tells of climate changes, e.g., it records the coming and going of glaciers. So even those who are skeptical of greenhouse warming should be interested in gaining information that will help us deal with a changing climate. We are going to have to deal with climate change with some mix of mitigation and

adaptation.

Before answering the specific questions you asked about the USGCRP, I should make a few general, background comments about the program.

The program has its virtues. Scientifically it is excellent and continues to contribute to our understanding of earth systems and why climate changes. Perhaps increasing our understanding will convince some skeptics that greenhouse warming is a real threat (but see below). Concerning policy, it has made global warming an issue. The hot summers of the late 80s generated the attention and urgency necessary for the issue to get on the public agenda, but climate research gave a scientific basis for believing that the heat was more than a random variation or an act of God. It might have been a harbinger of something we might be causing but might be able prevent. Thus today's hearing.

All of the above is a point of departure from which I will address the questions posed in your letter inviting me to testify.

You first asked if the program addresses the right questions? My answer is No, for two related reasons.

The first reason is that the program operates under an assumption, explicit in program documents and in the authorizing legislation, that if we do good research and accurately predict future climate, then making policy for climate problems will be easier. This is a flaw because it misleads in several ways:

It is misleading because we may never have definitive, credible climate predictions. In such a complex system the more we learn the more complexity we recognize, and our uncertainty may increase until we observe definitive warming. Note that global average temperature predictions haven't changed fundamentally in 10-15 years, but the range of uncertainty is slightly larger. The global climate system is much more complex than the global economy -- indeed it encompasses the economy -- and we do not fully understand and cannot predict the economy. Why do we think we can predict the climate? The more we know about systems like the Earth and everything that influences it -- from the sun to U.S. energy policy -- the more complicated things become, and the more uncertain predictions become. Even if we had a good prediction of what the climate will be in say 10 years, we have no way to know if it is accurate, except to wait ten years. So, what difference would that good prediction make? Would anyone believe it enough to make different decisions? On what assumptions is it based? Are the assumptions believable?

The discussion in the preceding paragraph shows why increasing our understanding of the climate system is not certain to convince skeptics. There will just be more things to argue about. There is always a chance that, as in politics, the mainstream scientists will divide into quarrelling schools, paralleling politics, as Pielke implies is a possibility. [Pielke 2002]

Second, and this is most important, a prediction will not tell us what to do in terms of mitigation and adaptation. To understand this, consider a familiar example: Social security. We can predict the problem very well. It is a matter of well-understood demographics. More and more people will be taking money out of the trust fund and fewer and fewer will be putting money in. We can accurately predict when the fund will go broke. But that tells us nothing about what to do today.

"What to do" involves politics and policy. Politics is not a dirty word. In a democracy it is how we resolve conflicts of values, which certainly describes the issues comprising the global warming policy situation.

Third, the assumption that prediction will make decisions and solutions easy extends into areas, that is, politics and policy, in which climate scientists have little expertise. This assumption may be unconscious and therefore unexamined.

Finally, to illustrate the unhelpfulness of prediction, let us assume we have a perfect prediction of future climate. Assume next year or five years from now the USGCRP concludes "If CO2 doubles, our regional average temperature will increase exactly 3.14 degrees centigrade. That's it, we're done. Goodbye." We also make the assumption that the prediction is somehow known to be accurate, i.e., that it will be believed. This is a very big assumption.

Now, what does such an accurate prediction mean in terms of mitigation and adaptation? As with Social Security, a prediction indicates a problem, but does not point to solutions. A climate prediction provides little usable information for decision makers who face difficult choices today on issues in water resources, agriculture, public health, energy, transportation, disaster planning, foreign policy, and national security, to name a few issue areas.

Any definitive statement about future climate change would be insufficient and perhaps irrelevant information for decision makers facing climate problems. It would be only a starting point for looking at these kinds of societal and policy issues. The important point is that we presently can provide information that is relevant to such issues, without achieving perfect prediction of the future.

There is historical precedent for believing that science can solve large policy problems. After World War II the scientists who had built the atomic bomb worried that they had created a monster and launched an international movement to control atomic power, including bombs. They believed intelligence and goodwill would resolve conflict. "Underlying the scientists' movement was a belief in the power of fact to compel assent in the political realm no less than in the laboratory." Boyer [1994] assesses the scientists' movement as politically naïve, as seeing atomic energy control as an event rather than a process. There are parallels here.

My second reason for saying that the program is not addressing the right questions is that the USGCRP does not focus on the many needs for information to support mitigation and adaptation

that could be identified and met now. Rather, it is a long-term scientific program to understand, model, and predict the behavior of the Earth System. Its research is planned to answer scientific questions, and is evaluated in terms of progress on those questions.

This leads to the second question you asked: How could a climate initiative yield information of greater relevance to end-users, people who make decisions related to climate?

To assure that a research program generates information of great relevance to end-uses, the users must be involved in planning and evaluating the research. That is, they must have a say in what research is done and in what counts as a success. Users must be able to ensure that research addresses their problems, and delivers usable results.

In the present program climate scientists typically develop information they want to develop, i.e., answers to scientific questions, and then try to get bewildered users to use it (the users may never have heard of the scientific question). Research results become a solution looking for a problem.

Sound research programs dedicated to problem solving typically have three phases: A beginning -- planning, a middle -- the research, and an end -- application and evaluation. The present program is almost all in the middle phase, that is, it is scientific research on scientific questions.

A better program, i.e., a program that would do more toward solving identified problems, would be conducted as follows: Research would be preceded by a planning phase in which users and scientists would identify and define specific problems to be attacked, as well as specific questions and information needs, and would look ahead to the application of the results. At this planning stage the primary sources of information about the problems are future users, the owners of the problems, not climate scientists. This planning process can be thought of as the researchers taking joint ownership of the problem with the users. The researchers do not relieve the users of responsibility, but together they take responsibility for solving the problem. Then in the middle the research is done, and new information is obtained and published. This second phase is often erroneously considered the entire project. Finally, in the third phase the results are applied in the field by the users on their problem and the research is evaluated in terms of how it helps solve the problems.

We hope that users will eagerly, fruitfully use the information, since they participated in planning the research. But such planning is hard and unfamiliar. Users may not express their needs clearly, or researchers may not hear them, and not every project will succeed. This is why the projects must be evaluated based on success in the field. Research projects unsuccessful in addressing the problem are terminated and successful ones are continued or replicated in a new context, as appropriate. That is, you correct and iterate. Of course provision is made for projects that are making good progress in a demonstrably practical direction. In this way a program of projects solving real problems is grown. Along the way good science of a different kind is done.

Of course, this sort of research does not replace basic research that might produce unexpected

breakthroughs. Some amount of basic research is needed, but not all in climate science. For example, basic research might be needed to provide a better understanding of how ordinary people think about formless, distant threats such as greenhouse warming.

We have spoken of users as if they are a well-defined class. Who are these users? On the one hand they are each of us who may be asked to change behavior and who will want to know why. We do not know what information will convincingly explain why.

But who are the specific users, owners of the problems to be served by the program? Unfortunately, after two decades and billions of dollars we don't know fully who they are. The list of potential users of the program is very long, as suggested above, yet we remain ignorant about how the program might best help them. If we don't know the users we can't know their problems well enough to know who needs what information to make what decision -- policy decisions, water decisions, investment decisions . . ., the same endless list of questions given above. This ignorance cries out for research, but not business-as-usual.

Rather, the research is to find real decision makers, potential users of climate information, see what information they want, provide it, see if they use it and if it makes a difference in their decisions. Think about what you learn. Use what you have learned about the users and how they use information to repeat the cycle to learn more. It must be emphasized that these users are not found by a survey questionnaire, but through research observing them use climate information to make decisions. It is hard, slow work at the start. As successes accumulate and are promoted, users may "come out of the woodwork." As you repeat this cycle for several different kinds of users you will begin to gain a general understanding of users of climate information and can feed this into all research planning. We don't know enough now to launch a large program of such research, because it would waste money. Rather, we should build on the few successful user-focused research projects and grow by iterating the process.

A Federal agency familiar to the committee uses such a research management approach. The National Institute of Standards and Technology involves users in its research planning processes. It has a system of panels of outside experts who oversee and evaluate each of its major divisions. The panels are composed of roughly half industry (that is, NIST's users) and half university researchers to provide scientific ballast and keep the industry folks from asserting special interests. [NRC 2000] It also does formal studies of the economic impact of its work, although admittedly not for every project. [see e.g., Link 97] For six years I served on the NRC Board of Assessment of NIST Programs, an umbrella group to which the panels reported. The Board integrates the panel reports and makes a comprehensive report to the NIST director. I saw their system work. Besides doing work its users want and need, NIST also has won two Nobel prizes in the last several years, for work directly supportive of its mission.

The USGCRP says it is moving in the direction I have recommended, i.e., more user-focused research. The RISA program which Professor Miles will describe is a step in the right direction, but to me it appears still too focused on prediction and scientific questions.

Finally, you asked about legislation. I have three suggestions:

One, authorize a program of research like that described above to find and serve the information needs of users, and also a program of small research centers, each focused on a user-defined, societal problem. The centers should be evaluated based on their progress in solving their problem. A recent NRC report (in which I participated) on DOE research to support its cleanup of radiation-contaminated sites recommends exactly such small, problem-focused centers. [NRC 2001] In operation, the research to find-and-serve users could be the focus of one or more of the first problem-focused research centers: The mission of these early-established centers would be to identify users who need and use climate information in their decisions, and to feed this information back into the program's planning effort. If they uncovered a user-defined problem needing research it could become the focus of one of the next generation of the small research centers. The important thing is to involve decision makers who use climate information in practical situations. And to learn by doing and correct what you are doing based on the experience gained.

Two, establish an external program evaluation board with a majority of its members being users not scientists. This board would be somewhat analogous to the NIST Evaluation board described above. It would especially oversee the selection of user/problem-focused research and the evaluation of practical results. It would complement the Federal interagency committee that guides the program's management. It should report to the Congress and to the head of the USGCRP -- perhaps the chair of the interagency committee.

Three, add status to user-focused research: Establish a set of substantial cash prizes for research that has shown clear societal benefit in important climate-related problem areas. As the prize would be based on practical results, a several-year waiting period would be necessary after completion of the research to allow those results to emerge. Currently scientific status comes primarily from outstanding work on scientific questions. We need not tamper with this, but should provide a different channel for recognition of excellence in solving climate mitigation or adaptation problems.

In conclusion,

1. The program incorrectly implies that we need more scientific understanding and predictive capability before we can support policy decisions. Actually, we already know enough for some decisions and in other areas need to know what users want.
2. The present program does not address relevant societal problems, but could by committing some of its resources to user-defined research.

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Phi Beta Kappa at Williams College, PhD in Physics at Rice University in 1967 in experimental atomic and molecular physics. After a postdoctoral fellowship at JILA, Byerly moved to science management and policy at the National Institute of Standards and Technology. There he worked mainly on environmental measurement and fire research. He joined the staff of the U.S. House of Representatives Committee on Science and Technology in 1975 with responsibility for environmental research programs (e.g., climate research, stratospheric ozone). In 1980 he took on space science and applications programs, and became staff director of the Space subcommittee in 1985.

In 1987 Byerly moved to the University of Colorado and built a space policy research program as director of its Center for Space and Geosciences Policy, an interdisciplinary research center supported in part by grants from the Sloan Foundation and NASA.

Rep. George Brown, (D-CA), new chair of the House Science and Technology Committee, appointed him Committee chief of staff in 1991. He was responsible for all operations of the Committee from staff and budget to policy development and the development and passage of legislation. Byerly retired in 1993, and now writes on science policy and serves on various committees (e.g., Bd of Directors, Associated Univ's for Research in Astronomy, NRC Bd of Assessment of NIST Programs, NRC Comm on DOE EQ Res., AAAS Comm. on Science, Engineering, and Public Policy, NSF review panels). In 2001 AAAS elected him a Fellow.

Selected Publications:

Book editor (w. Sarewitz and Pielke) and chapter author, *Prediction; Science, Decision Making, and the Future of Nature*, Island Press, April 2000.

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