RESPONSES OF DR. MICHAEL MANN TO QUESTIONS PROPOUNDED BY THE COMMITTEE ON ENERGY AND COMMERCE SUBCOMMITTEE ON OVERSIGHT AND INVESTIGATIONS

Question No. 1. I understand that although your current practice is to make your computer code available publicly, many researchers in your field do not do so. Although computer code may not have commercial value, why would a researcher not want to release his code?

Answer:

This is a question that my colleagues and I have wrestled with over the years. As the question acknowledges, for the past five years or more, my colleagues and I have made public our computer codes, just as we made public our code for the 1998 study last year. Our decision to make our code public comes at a time when there is increased standardization in codes, and the need to tailor codes to accommodate the various and often idiosyncratic computer systems that were used in the 1990s has diminished. But even today, many, perhaps most, climate scientists do not share their codes. In my view, there are legitimate reasons for reaching that decision, even though it is not the decision my colleagues and I have made.

For one thing, most code is written to enable scientists to perform specific functions, and thus code is generally written in a form of short-hand that is not easily understood by others. To make code usable by other researchers, the code writer has to undertake significant additional work, in the form of documentation, testing for potential platform dependence, tidying, and so forth, that places a significant burden on the code writer. Many scientists do not think that undertaking that additional burden is worth it.

Second, access to computer codes is not necessary to replicate a study. I realize that some of my critics have argued otherwise, but it is just not the case that scientists need access to computer codes to replicate studies. As I tried to make clear in my testimony before the Committee, a study may be replicated if the scientists conducting the initial study make available both the underlying research data and an algorithm that gives a step-by-step account of how that data was analyzed. As my testimony pointed out, the 1998 and 1999 work by my colleagues and me was recently replicated by a team of scientists (Wahl and Ammann) who did not have access to our codes, but who were able to replicate our work without difficulty. So replication does not depend on access to computer codes.

Moreover, scientists, like entrepreneurs, corporations, and others engaged in the production of intellectual capital, are competitive, and rightly so. Competition in the marketplace of ideas is what science is all about. We would all like to make our greatest possible contributions to advancing the forefront of our scientific disciplines. Indeed, we are rewarded (in terms of grants, promotions, academic recognition, and do forth) in proportion to the contributions we make in the advancement of science. Asking scientists to release their codes before they have had an opportunity to apply them to a number of potential interesting problems is asking them to sacrifice their competitive advantage. This would be no different than asking Microsoft to release the code for its latest operating system as soon as it reaches the market. Microsoft is not about to do that, and most people would consider a requirement that Microsoft freely dispense its intellectual property --- its codes --- as antithetical to the principles of a free market. The argument is no different in the case of scientists and their computer codes or other tools of their trade.

Question No. 2. Dr. Wegman states that paleoclimatologists do not interact with statisticians. Do you have any response to that statement? What steps, if any, is the paleoclimatology field taking to ensure that it is using appropriate statistical methodologies?

Answer:

Unfortunately, Dr. Wegman made this claim without engaging in any effort to ascertain the extent to which climate scientists interact with statisticians. To the contrary, Dr. Wegman simply assumed --- without data, indeed, without any basis at all --- that climate scientists, and paleoclimatologists in particular, do not interact with statisticians.

Dr. Wegman's accusation could not be further from the truth. The participation of statisticians in climate science has become so routine that there is an entire field of climate research known as "statistical climatology," which involves the collaboration of large numbers of statisticians and climate scientists. There are even textbooks dedicated to the study of statistical climatology. In his testimony before the Committee, Dr. Hans Von Storch found it necessary to inform Dr. Wegman of this fact. And Dr. Von Storch should know; he and Dr. Francis Zwiers (a Ph.D. statistician specializing in climate applications) have written one widely used textbook on statistics and its applications to climate studies. Another statistician, Professor Dan Wilks of Cornell University, has written an additional textbook on statistics and its applications to the atmospheric sciences.

The extensive collaboration between climate scientists and statisticians is also reflected in the academic literature. Hundreds of papers have been published in the climate and paleoclimate literature involving the collaboration of statisticians and climate scientists. These are all publicly available and could have been identified by Dr. Wegman in a few hours of research. Two members of the NRC committee that reviewed paleoclimate reconstructions in its recent report (Dr. Douglas Nychka and Dr. Peter Bloomfield) are statisticians (both of their doctorates are in statistics) who have published in the climate literature and who have actively collaborated with climate scientists.

Had Dr. Wegman bothered to make even the slightest inquiry, he would have found that there are in fact many statisticians (that is, individuals with doctorates in statistics) who have been and remain active members of the community of researchers in the areas of atmospheric science and climate research. Even a cursory review of the structure of our community reveals this readily. I have been informed that many of my statistical climatologist colleagues are deeply offended by Dr. Wegman's unfounded pronouncements to this Committee, pronouncements which effectively deny their contribution to the advancement of science.

Moreover, the American Meteorological Society --- the leading professional organization of atmospheric scientists --- has a Committee on Probability and Statistics, and members of the committee are drawn from both atmospheric/ocean/climate scientists and statisticians. I was a member of that committee for a 3-year term (2003-2005) that recently ended. The committee's website can be found here: http://www.isse.ucar.edu/ams/ams_ps.html, and the committee members' biographies are available here:

http://www.isse.ucar.edu/ams/ams_ps.html#members. The chair of the committee, Dr. Rick Katz is a statistician (with his doctorate in statistics from Penn State University) and senior scientist at NCAR. Other statisticians on the committee include Dr. Tilmann Gneiting (Department of Statistics, University of Washington), and Dr. William Briggs (Adjunct Assistant Professor of Statistical Science, Cornell University). These statisticians are active members of the climate research community.

Equally important, one of the primary centers for climate research in the U.S., NCAR, has maintained a thriving Geophysical Statistics Project ("GSP"), which was founded more than a decade ago. This program has been funded by the National Science Foundation's Division of Mathematical Sciences, which has recognized for some time the importance of encouraging statisticians to collaborate actively with atmospheric scientists/climate scientists. I participated as a graduate student in GSP's inaugural workshop in 1994. Many leading statisticians (*e.g.*, Dr. Grace Wahba, Dr. Arthur Dempster, and Dr. Noal Cressie) were participants. The GSP has since thrived, providing an important opportunity for collaboration between statisticians and climate researchers. More information can be found at the GSP webpage: http://www.image.ucar.edu/GSP/.

It bears noting that the project has now produced more than two dozen Ph.D. statisticians who have become active researchers in the atmospheric, oceanographic, and climate sciences. Its members and visitors have included dozens of statisticians who have worked collaboratively with atmospheric scientists and climate researchers. The leader of the project, Dr. Douglass Nychka, was one of the members of the aforementioned NRC panel. He was also a consultant in the recent paper by Wahl and

Ammann that refutes the oft-cited criticisms of the Mann et al. work by McIntyre and McKitrick.

Question No. 3. Dr. Wegman has hypothesized that the peer review process failed and allowed publication of your 1998 and 1999 studies without adequate vetting of the study. This was based in part on his social network analysis that showed you have connections with 42 other authors in paleoclimatology. Of the 42 co-authors identified by Dr. Wegman, how many of them were co-authors with you in or before 1999?

Answer:

Dr. Wegman's accusations are so riddled with flaws that it's hard to know where to begin in response. But let me first address the specious accusation by Wegman that the peer-review process somehow "failed" with respect to our '98 and '99 studies. It is bewildering that Dr. Wegman (who has no expertise in the area of atmospheric science/climate, and indeed was wholly unable to correctly answer some of the most basic questions about climate science during the hearings) would characterize the publication of our work as a "failure." One would assume that an academic would avoid rendering judgments in fields in which he is demonstrably unknowledgeable. Certainly the scientific community has reached the precisely the opposite conclusion. Our 1998 and 1999 studies are widely cited, and the conclusions stated in them have been repeatedly reaffirmed. Just one example of the scientific support for these works should suffice: The National Research Council panel in their recent Report characterized our study as "groundbreaking", and the panel concluded that its key conclusions have held up over nearly a decade of exhaustive and independent follow-up research. That is a pretty good track record by any standard. Thus, judged by experts who understand climate studies, Wegman's efforts to disparage our work as "failed" are nothing short of silly.

Let me next address Wegman's equally specious and unsupported claim that scientists who work in a given field cannot objectively review the work of their colleagues and competitors in that field. By way of illustration, I have attached (as Attachment 1 to these Responses) the famous 1927 photograph of attendees of the Solvay Physics meeting in Brussels. It shows a group of 29 physicists engaged in a collegial, small conference. Virtually every attendee was a driving figure behind our understanding of modern physics. Appearing in the photograph are Einstein, Heisenberg, Bohr, Fermi, Dirac, de Broglie, Born, Pauli, Langmuir, Planck, Curie, Compton, Ehrenfest, Lengevin, and others of equal prominence. The members of this group all knew each other, worked with each other, collaborated on research with one another, visited each other, went mountain-climbing together, and so forth. Familiarity did not compromise their contributions to science. While I do not claim that the group I collaborate with is likely to duplicate the feats of the scientists who gathered in Brussels 80 years ago, the point remains --- scientific collaboration does not turn scientists into timid lapdogs unwilling to criticize the work of their colleagues.

Let me turn now to the specifics of the question. It is baffling how Dr. Wegman arrived at the number (42) he used to describe my co-authors. One would think that a statistician could do simple arithmetic. My curricular vitae (CV) is available on the internet, and it is clear that Wegman consulted it (but not me) in the preparation of his paper. Nonetheless, none of the numbers he uses add up. Part of the problem may stem from Wegman's ill-advised effort to distinguish between authors engaged in "paleoclimatology" and "climatology," since most climate researchers have worked, in some manner, on some aspect of paleoclimate. So the distinction he attempts to draw between "paleoclimatologists" and "climatologists" is illusory at best. This too underscores the hazards of an amateur seeking to draw conclusions in a field in which he has no expertise.

But to answer the question Wegman poses, let us consider the correct numbers (see Attachment 2 to these Reponses) which are based on all of my peer-reviewed journal publications as listed on my CV (and not including "gray literature" such as book chapters, encyclopedia pieces, reports, conference proceedings, letters to editors, opinion pieces). I published with 10 co-authors prior to 1993 based on my undergraduate research in solid state physics. These publications are unrelated to climate research, and are not included.

So let us consider just my climate-related papers (i.e., post 1993), as Wegman purports to do. In climate research, I had 14 co-authors through the year 1999. I had 101 co-authors through the end of 2005. So Wegman's calculations, based on 42 co-authors, are off-base by more than a factor of two. Wegman also appears to have made even more fundamental errors in his review of the science (a point I address below).

But I believe the question goes to how influential I was in the field, in a relative sense, at the time of publication of my '98 and '99 studies. After all, Wegman claims that there is, in essence, an almost sinister conspiracy of like-minded climate scientists who act as a cartel to control the published literature in climate studies. And his "proof" is the fact that I have published with many prominent scientists who, in Wegman's view, would be unwilling to criticize my 1998 and 1999 work even if it were seriously flawed. But this theory does not wash. Apart from the fact that even my closest collaborators are perfectly willing to criticize my work when they think it is flawed, Wegman's math just does not support his theory. As indicated above, the vast majority (86%) of my co-authorships occurred *after* my 1998/1999 studies. So Wegman's effort to suggest that I was influential in the field at the time these studies were published, or in the aftermath of their publication, cannot be squared with the data, and is, in fact, nothing short of absurd.

Question No. 4. Does the scientific community rely exclusively or primarily on the peer review process conducted before an article is published to test the robustness and validity of new scientific discoveries or theories? Or does the development of science depend on an iterative process that involves not only peer review before publication, but also review and competing research and analysis by other scientists after publication?

Answer:

This question raises an important issue that was unfortunately not adequately aired at the hearing. Dr. Wegman and others have expressed the view that the scientific community somehow places exclusive reliance on the peer review process as the determinant of scientific truth. But the peer review process is hardly the only, let alone most important, way that the scientific community tests the accuracy and reliability of scientific papers. Indeed, Wegman's contention reflects a fundamental lack of understanding of the basic principles that govern the scientific discipline. Science progresses through an open, self-correcting process whereby scientists place their ideas in the marketplace, typically by publishing articles in peer review journals. The peer review process ensures only that basic mistakes are not made, that the article acknowledges the existing literature on the subject, and that it contributes in some way to the exploration of important scientific issues. But peer review does not and cannot vouch for the accuracy of the paper. That is the function of the scientific process, by which other scientists test out and question the work of their peers. Some ideas stand the test of time; others do not. Copernicus was proven right over time; Ptolemy's conception that the Earth forms the center of the universe was proven wrong. Much of Einstein's work has stood up to reevaluation, but some of his theories have been proven to be incorrect as well.

It is relevant in this context to again emphasize that the key conclusion that my colleagues and I drew tentatively in our work in the late '90s --- that late 20th century Northern Hemisphere average warmth was *likely* unprecedented in at least the past 1000 years --- has held up for more than a decade, after dozens of independent studies have reexamined that claim. So it has passed this important test of time. The peer-review process is simply a quality control process to make sure that claims, theories, and ideas that are self-evidently flawed from the beginning do not clutter the pages of the legitimate peer-reviewed scientific journals, that is, to ensure that published papers have *potential* merit. Peer review is a simple first step at quality control. It does not, nor should it, be considered evidence that the conclusions of a particular published paper are accurate or not. No single paper should ever be used to establish the validity of a particular hypothesis or conclusion. The accuracy of claims, hypotheses, conclusions, indeed theories, can only be established by examining the collective body of peer-reviewed research to date on any particular topic, and the overall thrust of that body of research.

Indeed, the importance of broad-based scientific assessments (such as those provided by the Intergovernmental Panel on Climate Change or "IPCC") is to evaluate the entire body of peer-reviewed literature on a particular topic and to determine the consensus, if there is one, that emerges in that body of literature.

Question No. 5. Should all scientific papers be withheld from publication until the results are independently replicated?

Answer:

This question also raises an important issue that was not adequately aired at the hearing. Once again, Dr. Wegman and others suggested at the hearing that scientific papers be shelved for the time it takes for the results to be verified independently. This view is misguided, and, if followed, would seriously undermine the development of scientific knowledge. It takes considerable time to replicate a study. Meanwhile, important findings that ought to be disseminated widely to the scientific community would be unavailable to other scholars. Such a requirement would dangerously slow the progress of science.

As I explained above, in my view development of scientific knowledge can take place only through an open, self-correcting process whereby scientists put out ideas, other scientists test them, and those ideas which stand up to future tests survive while those that do not are ultimately rejected. It is important in this context that ideas with potential merit be placed in the scientific discourse in a timely manner, so that they can be followed up in a timely manner by the entire scientific community and not just a few researchers engaged in replication, and the scientific process can proceed at an appropriate pace. Were the suggested requirement to be followed where all papers required independent replication before publication, this would bog down the scientific process to a near standstill.

In data-poor areas of science such as paleoclimatology, the added benefit of new data is much more valuable than the pure replication of a past study. “Replication” in a pure sense provides very poor value for money. A good example would be the now-famous GRIP and GISP2 ice cores from Greenland. These are two different Greenland ice cores that were drilled at two nearby but distinct locations by two different (one U.S. and one European) teams. Had the total available funding simply been used for both teams to drill cores at the same site, and thereby replicate each other’s work, only the technical accuracy of the coring would have been validated. Instead, the reproduction of a record that was nearby but separate gave both support to the main results, but also allowed the groups to discover a mix-up in dating prior to 100,000 years ago in one of the two cores. So drilling two different ice cores, rather than drilling from the same source twice, proved to be a far more valuable use of the available funding and resources.

The proponents of this idea also ignore the near-impossibility of its implementation. How would scientists be persuaded to replicate the unpublished work of others? What would their incentives be to conduct this work quickly, especially if it meant sacrificing the time researchers would prefer to spend on their own work? Would every study be subject to replication? Or only important studies? And who would decide which studies required replication prior to publication? Who would pay for these replications? Would the government pay for them? Is Congress prepared to double the size of research budgets for all of the major scientific funding agencies (e.g. NSF, NIH, NOAA, etc.)? And these practical problems are only the tip of the iceberg.

My essential plea here is that Congress should not fix that which is not broken. Since Copernicus' time the scientific process has successfully weeded out the wheat from the chaff. It would be dangerous for Congress or any government body to tamper with that process.

There is another element of this question which raises a deeply troubling matter with regard to Dr. Wegman's failure to subject his work to peer review, and Wegman's apparent refusal to let other scientists try to replicate *his* work. Professor David Ritson, Emeritus Professor of Physics, Stanford University, has found error in the way that Dr. Wegman models the "persistence" of climate proxy data. Interestingly, this is the same error Steven McIntyre committed in his work, which was recently refuted in the paper by Wahl and Ammann, which was in turn vetted by Dr. Douglass Nychka, an eminent statistician. Dr. Ritson has determined that that the calculations that underlie the conclusions that Dr. Wegman advanced in his report are likely flawed. Although Dr. Ritson has been unable to reproduce, even qualitatively, the results claimed by Dr. Wegman, he has been able to isolate the likely source of Wegman's errors. What is so troubling is that Dr. Wegman and his co-authors have ignored repeated collegial inquiries by Dr. Ritson and apparently are refusing to provide any basic details about the calculations for the report (see Attachments 3 and 4 to this Response). It would appear that Dr. Wegman has completely failed to live up to the very standards he has publicly demanded of others.

Moreover, the errors that Dr. Ritson has identified in Dr. Wegman's calculations appear so basic that they would almost certainly have been detected in a standard peer review. In other words, had Dr. Wegman's report been properly peer-reviewed in a rigorous process where peer-reviewers were selected anonymously, it likely would not have seen the light of day. Dr. Wegman has thus unwittingly provided us with a prime example of the importance of the peer review process as a basic first step in quality control.