UNITED STATES ENVIRONMENTAL PROTECTION AGENCY

Docket No. EPA-HQ-OAR-2009-0171

Endangerment and Cause or Contribute Findings for Greenhouse Gases under Section 202(a) of the Clean Air Act

74 Fed. Reg. 66496 (Dec. 15, 2009)

PETITION FOR RECONSIDERATION BY PEABODY ENERGY COMPANY

February 11, 2010

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Peabody Energy Company respectfully requests that the United States Environmental Protection Agency ("EPA" or "Agency") reconsider its *Endangerment and Cause or Contribute Findings for Greenhouse Gases under Section 202(a) of the Clean Air Act* published at 74 Fed. Reg. 66,496 (Dec. 15, 2009).[1] Peabody's petition is based primarily on the release of email and other information from the University of East Anglia ("UEA") Climatic Research Unit ("CRU") in November of last year.[2] The CRU information undermines a number of the central pillars on which the Endangerment Finding rests, particularly the work of the Intergovernmental Panel on Climate Change ("IPCC").

Given the seriousness of the flaws that the CRU material reveals in the development of the IPCC reports, and given EPA's extensive reliance on those reports, the Agency has no legal option but to reexamine the Endangerment Finding in light of this new information. Indeed, the analytical process in which EPA engaged in reaching its Endangerment Finding is so tainted by the flaws now revealed in the IPCC reports that the Agency must take the unusual step of convening full evidentiary hearings in order to provide an open and fair reconsideration process.

[1] For convenience, we will refer to these findings as the Endangerment Finding.

[2] We are providing these emails and all of the information that was released from the CRU website as it was originally released. This includes the "Harry_Read_Me" files that we refer to later in this Petition. Because of the volume of the information, we are providing it to EPA on a disk. The emails are identified in this Petition using both the dates and numerical identification provided in the emails. We quote these emails exactly as they were written without correcting or indicating improper spelling. By now, there is little doubt that the emails are authentic, as none of those who wrote the emails have denied their authenticity.

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GLOSSARY OF ACRONYMS

ACUS	Administrative Conference of the United States
AGU	American Geophysical Union
ANPR	Advanced Notice of Proposed Rulemaking
AR4	Fourth Assessment Report from the IPCC
CAA	The Clean Air Act
CCSP	United States Climate Change Science Program, now USGCRP
CEI	Competitive Enterprise Institute
CO_2	Carbon dioxide
CRU	University of East Anglia's Climatic Research Unit
DOE	United States Department of Energy
ENSO	El Niño Southern Oscillation
EPA	United States Environmental Protection Agency
FAR	First Assessment Report published by the IPCC
FOIA/FOI	Freedom of Information Act, in the United States and the United Kingdom
GEF	Global Environment Facility
GISS	Goddard Institute for Space Studies
GHCN	Global Historical Climatology Network
GHG	Greenhouse gases
GRL	Geophysical Research Letters
IISD	International Institute for Sustainable Development
IJC	International Journal of Climatology
IPCC	Intergovernmental Panel on Climate Change
IQA	Information Quality Act
JGR	Journal of Geophysical Research
LIA	Little Ice Age
MBH98	Mann et al. paper *Global Scale Temperature Patterns and Climate Forcing Over the Past Six Centuries,* published in 1998.
MBH99	Mann et al. paper *Northern Hemisphere Temperatures During the Past Millennium" Inferences, Uncertainties, and Limitations,* published in 1999.
MWP	Medieval Warm Period
NASA	National Aeronautics and Space Administration
NOAA	National Oceanic and Atmospheric Administration
NRC	National Research Counsel
PAGES	Past Global Changes
RAOBCORE	Radiosonde Observation Correction Using Reanalyses
SAPs	Synthesis and Assessment reports produced by the CCSP
TAR	Third Assessment Report from the IPCC
TSD	Technical Support Document
UEA	University of East Anglia
UCAR	University Corporation for Atmospheric Research
UNEP	United Nations Environment Programme
USGCRP	United States Global Change Research Program
WGI	IPCC Working Group I

WGII	IPCC Working Group II
WMO	World Meteorological Organization
WWF	World Wildlife Fund

LIST OF PRINCIPAL SCIENTISTS

Dr. Caspar Ammann - Scientist II in the Climate and Global Dynamics Division of National Center for Atmospheric Research. Contributing author of Chapter 9 of Working Group I Report, 2007 IPCC Fourth Assessment Report.

Dr. Raymond Bradley - Director of the Climate System Research Center, University of Massachusetts. Co-author of MBH98 and MBH99.

Dr. Keith Briffa - Deputy Director, Climatic Research Unit, Univeristy of East Anglia. Lead author of Chapter 6 of Working Group I Report, 2007 IPCC Fourth Assessment Report. Contributing author of Chapter 2 of Working Group I Report, 2001 IPCC Third Assessment Report.

Dr. John Christy – Director ofEarth System Science Center at the University of Alabama in Huntsville. Lead author of Chapter 3 of Working Group I Report, 2007 IPCC Fourth Assessment Report. Lead author of Chapter 2 of Working Group I Report, 2001 IPCC Third Assessment Report.

Dr. Edward Cook - Doherty Senior Scholar and Director, Tree-Ring Laboratory, Lamont-Doherty Earth Observatory. Contributing author of Chapter 6 of Working Group I Report, 2007 IPCC Fourth Assessment Report. Contributing author of Chapter 2 of Working Group I Report, 2001 IPCC Third Assessment Report.

Dr. Thomas Crowley – Professor, School of GeoSciences at The University of Edinburgh.

Dr. Rosanne D'Arrigo – Senior Research Scientist at Tree-Ring Laboratory, Lamont-Doherty Earth Observatory.

Dr. Valérie Masson-Delmotte -Laboratoire des Sciences du Climat et de l'Environnement. Lead author of Chapter 6; Contributing author of Chapter 9 of Working Group I Report, 2007 IPCC Fourth Assessment Report.

Dr. David Douglass - Professor, University of Rochester.

Dr. Jan Esper – Head of Dendro Sciences Division, Swiss Federal Research Institute WSL.

Dr. Melissa Free - Air Resources Laboratory (ARL), NOAA. Lead author of Chapter 3 of Working Group I Report, 2007 IPCC Fourth Assessment Report.

Dr. Chris de Freitas - Associate Professor, Deputy Director of School, Associate Director (Postgraduate Affairs), School of Environment, University of Auckland.

Dr. Vincent Grey - a founder of the New Zealand Climate Science Coalition.

Dr. James Hack - Senior Scientist, CMS Section Head, CGD Deputy Director. National Center for Atmospheric Research.

Dr. Malcolm Hughes - Regents' Professor, Laboratory of Tree-Ring Research at The University of Arizona. Co-author of MBH98 and MBH99.

Dr. Eystein Jansen - Research Director of the Bjerknes Center for Climate Research, University of Bergen, Norway. Coordinating lead author of Chapter 6 of Working Group I Report, 2007 IPCC Fourth Assessment Report.

Dr. Phil Jones - Professor, Climatic Research Unit, Univeristy of East Anglia. Coordinating lead author of Chapter 3 of Working Group I Report, 2007 IPCC Fourth Assessment Report.

Dr. Thomas Karl - Director, National Climatic Data Center, NOAA. Review editor of Chapter 3 of Working Group I Report, 2007 IPCC Fourth Assessment Report. Coordinating lead author of Chapter 2 of Working Group I Report, 2001 IPCC Third Assessment Report.

Dr. Otto Kinne - President, Inter-Research Science Center.

Dr. A.T.J de Laat - The Royal Netherlands Meteorological Institute.

Dr. Murari Lal - Chairman, Climate, Energy and Sustainable Development Analysis Centre. Co-ordinating Lead Author for Asia - Chapter 10 of Intergovernmental Panel on Climate Change's Fourth Assessment Report of Working Group II (2004-2007). Contributing author of Chapter 2 of Working Group I Report, 2001 IPCC Third Assessment Report.

Dr. Stephen Mackwell - Director of the Lunar and Planetary Institute. Editor-in-Chief, Geophysical Research Letters, 2002–2004.

Dr. Michael Mann - Director, Earth System Science Center, Pennsylvania State University. Lead author of Chapter 2; Contributing author of Chapter 7; Contributing author of Chapter 8 of Working Group I Report, 2001 IPCC Third Assessment Report.

Dr. Glenn McGregor - Director of School of Environment, University of Auckland; Editor, International Journal of Climatology.

Stephen McIntyre - editor of Climate Audit.

Dr. Ross McKitrick - Professor of Economics at the University of Guelph.

Dr. Patrick Michaels - Senior Fellow at the Cato Institute and a former Professor of Environmental Sciences from the University of Virginia.

Dr. Jonathan Overpeck - Co-director of the Institute of the Environment (IoE) at The University of Arizona. Coordinating lead author of Chapter 6 of Working Group I Report, 2007 IPCC

Fourth Assessment Report. Contributing author of Chapter 2 of Working Group I Report, 2001 IPCC Third Assessment Report.

Dr. Tim Osborn - Academic Fellow at the Climatic Research Unit, within the School of Environmental Sciences at UEA. Contributing author of Chapter 6; Contributing author of Chapter 8 of Working Group I Report, 2007 IPCC Fourth Assessment Report.

Dr. Roger Pielke, Jr. - Professor in the Environmental Studies Program, University of Colorado and a Fellow of the Cooperative Institute for Research in Environmental Sciences.

Dr. Benjamin Santer - Research Scientist, Program for Climate Model Diagnosis and Intercomparison at the Lawrence Livermore National Laboratory. Lead author of Chapter 8 of the 1995 IPCC report.

Dr. Gavin A. Schmidt –Climatologist and Climate Modeler at NASA Goddard Institute for Space Studies.

Dr. Stephen Schneider - Editor of the journal Climatic Change; Stanford University.

Dr. Olga Solomina - Corresponding Member, the Russian Academy of Sciences; Senior Research Scientist, Institute of Geography, Russian Academy of Sciences. Contributing author of Chapter 4, Lead author of Chapter 6 of Working Group I Report, 2007 IPCC Fourth Assessment Report.

Dr. Susan Solomon - Senior Scientist, Chemical Sciences Division (CSD)Earth System Research Laboratory (ESRL), NOAA. Co-Chair, IPCC Working Group I Report, 2007 IPCC Fourth Assessment. Contributing author of Chapter 4; Lead author of Chapter 6 of Working Group I Report, 2001 IPCC Third Assessment Report.

Dr. Kevin Trenberth - Senior Scientist, Head of the Climate Analysis Section at the National Center for Atmospheric Research. Contributing author of Chapter 1; Coordinating lead author of Chapter 3; Contributing author of Chapter 7 of Working Group I Report, 2007 IPCC Fourth Assessment Report. Contributing author of Chapter 8 of Working Group I Report, 2001 IPCC Third Assessment Report.

Dr. Eugene Wahl - Physical Scientist, Paleoclimatologist, Paleoecology, Climate Reconstruction, NOAA.

Dr. Edward Wegman – Professor, Department of Statistics, George Mason University.

Dr. Thomas Wigley - senior scientist in the Climate and Global Dynamics Division, University Corporation for Atmospheric Research. Contributing author of Chapter 10 of Working Group I Report, 2007 IPCC Fourth Assessment Report. Contributing author of Chapter 4; Contributing author of Chapter 9; Contributing author of Chapter 12 of Working Group I Report, 2001 IPCC Third Assessment Report.

I.

EXECUTIVE SUMMARY

"I tried hard to balance the needs of the science and the IPCC, which were not always the same."

> Dr. Keith Briffa, lead author of Chapter 6 of Working Group I Report, *The Physical Scientific Basis*, 2007 IPCC Fourth Assessment Report

"It related to several countries in this region and their water sources. We thought that if we can highlight it, it will impact policy-makers and politicians and encourage them to take some concrete action. It had importance for the region, so we thought we should put it in."

> Dr. Murari Lal, coordinating lead author of Chapter 10 of Working Group II Report, *Impacts, Adaptation and Vulnerability*, 2007 IPCC Fourth Assessment Report, referring to how misinformation about the pace of Himalayan glacier melt was included in the report, as reported in the Daily Mail, January 24, 2010

"Failure to make research data and related information accessible not only impedes science, it also breeds conflicts."

> Ralph J. Cicerone President of the National Academy of Sciences, Science, February 5, 2010, commenting on the CRU material

EPA must reconsider its Endangerment Finding based on new material that was not available during the comment period and which is central to the outcome that EPA reached in promulgating its Endangerment Finding. EPA failed to properly exercise its judgment as required by the Clean Air Act ("CAA") and acted in an arbitrary and capricious fashion by relying almost exclusively on flawed reports of the IPCC in attributing climate change to anthropogenic greenhouse gas ("GHG") emissions. As evidenced by material that became available last fall from CRU, as well as additional information that has become available since the Endangerment Finding was issued, the IPCC reports were not the product of a rigorous, transparent and neutral scientific process.

Indeed, contrary to the CAA and the Information Quality Act ("IQA"),[3] EPA largely ceded its obligation to make a "judgment" as to whether GHGs may endanger public health and welfare to the IPCC, an international body that is not subject to U.S. data quality and transparency standards and whose reports were prepared in direct disregard of those standards. As a result, EPA is set to begin regulating GHG emissions based on a scientific process that was conducted without the basic procedural safeguards *set forth in U.S. law* to ensure the reliability and accuracy of the scientific conclusions underlying the Agency's Endangerment Finding. As an agency of the United States, however, whose regulatory actions will have far-reaching consequences for U.S. citizens, EPA must abide by U.S. standards and not the standards of international bodies whose actions are governed by different norms.

Accordingly, the EPA should reconsider its Endangerment Finding in light of the recently discovered defects in the IPCC's procedures and convene full evidentiary hearings to provide an open and fair reconsideration process.

Background

On December 5, 2009, EPA released its landmark *Endangerment and Cause or Contribute Findings for Greenhouse Gases under Section 202(a) of the Clean Air Act*.[4] EPA found that "[t]he scientific evidence is compelling that elevated concentrations of heat-trapping gases are the root cause of recently observed climate change."[5] According to EPA, "[m]ost of the observed increase in global average temperatures since the mid-20th century is very likely due to the observed increase in anthropogenic GHG concentrations," with "very likely" defined

[3] The IQA was enacted as the Treasury and General Government Appropriations Act for Fiscal Year 2001 § 515, 44 U.S.C. 3504(d)(1) and 3516 (2000).

[4] The finding was subsequently published at 74 Fed. Reg. 66,496 (Dec. 15, 2009).

[5] Endangerment Finding, 74 Fed. Reg. at 66,518.

as a 90-99% probability.[6] EPA further found that this change in climate is already causing a variety of detrimental impacts to U.S. public health and safety and that these impacts are likely to worsen in the future.[7]

Just weeks before EPA issued its Endangerment Finding, a considerable body of email and other information from CRU became available on the Internet. The CRU information undermines a number of the central pillars on which the Endangerment Finding rests, particularly the work of the IPCC.[8] The CRU information reveals that many of the principal scientists who authored key chapters of the IPCC scientific assessments were driven by a policy agenda that caused them to cross the line from neutral science to advocacy. Indeed, they went far beyond even what is acceptable as advocacy, as they actively suppressed information that was contrary to the "nice, tidy story" that they wished to present, they refused to disclose underlying data concerning the studies in which they were involved to third parties who might use the information to critique those studies, they engaged in a wide variety of improper and indeed unethical tactics to manipulate the type of scientific information that appeared both in the IPCC reports and in the peer-reviewed scientific journals upon which the IPCC largely relied, and they relied on inaccurate and unverified information from secondary source material that was produced by advocacy groups, information that the authors apparently knew was unverified but

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[6] ENVTL. PROT. AGENCY, TECHNICAL SUPPORT DOCUMENT FOR ENDANGERMENT AND CAUSE OR CONTRIBUTE FINDINGS FOR GREENHOUSE GASES UNDER SECTION 202(A) OF THE CLEAN AIR ACT. ("TSD") (2009) at 48, 7.

[7] Endangerment Finding, 74 Fed. Reg. at 66,523-26.

[8] The IPCC is a body that was established by the United Nations Environment Programme ("UNEP") and the World Meteorological Organization ("WMO") to "provide the world with a clear scientific view on the current state of climate change and its potential environmental and socio-economic consequences." The IPCC releases Assessment Reports, and those reports contain a section entitled *Summary for Policymakers* that makes suggestions to government based on the conclusions in the Assessment Reports. The two most recent IPCC reports comprehensively assessing climate science are the 2007 Fourth Assessment Report, referred to as "AR4," and the 2001 Third Assessment Report, referred to as the "TAR." Both AR4 and the TAR included three Working Group reports. Except as specifically noted, when we cite to AR4 and the TAR below, we are referring to the Working Group I reports concerning the "scientific basis."

included anyway to advance the authors' advocacy agenda. Moreover, the Information Commissioner's Office of the United Kingdom ("U.K."), the agency that oversees and enforces the U.K.'s freedom of information laws, after investigation, recently concluded that CRU broke those laws in refusing to respond to information requests.[9]

Thus, the IPCC's recent retraction of its "poorly substantiated estimates of the rate of recession and the date for the disappearance of Himalayan glaciers" as set forth in the AR4 Working Group II Report,[10] and the numerous other recent IPCC errors that have come to light, is indicative of a process that was far less neutral and robust than EPA assumed. Moreover, since the IPCC, as an international body, is not subject to any nation's Freedom of Information Act, and since the CRU material obviously reflects only one small source of information concerning the drafting of the IPCC reports, it is not known what other flaws may have occurred in the IPCC process and are yet to be revealed.

EPA believes that it has broad discretion in making its Endangerment Finding.[11] Although the extent of EPA's discretion is debatable, what is not debatable is EPA's obligation to justify the particular choices it made in exercising that discretion.[12] EPA's determination here was not that it *might* be possible that anthropogenic GHG emissions will cause *possibly* dangerous climate change at some point in the future. Instead, EPA decided that such emissions *were almost certainly already* causing dangerous climate effects, with the danger almost

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[9] *See Climate row unit 'broke data law,'* BBC NEWS Jan. 28, 2010 *available at* http://news.bbc.co.uk/2/hi/uk_news/8484385.stm.

[10] *IPCC statement on the melting of Himalayan glaciers,* Jan. 20, 2010 *available at* http://www.ipcc.ch/pdf/presentations /himalaya-statement-20january2010.pdf.

[11] *See* general discussion at Endangerment Finding, 74 Fed. Reg. at 66,506-09.

[12] *See Motor Vehicle Mfrs. Ass'n of the United States, Inc. v. State Farm Mutual Auto Ins. Co.*, 463 U.S. 29, 43 (1983) (agency must articulate a "rational connection between the facts found and the choice made").

certainly likely to worsen in the future. This is an important distinction because obviously the degree of endangerment that EPA finds will guide the nature and extent of regulation that EPA will now promulgate. Based on the CRU material and other information that has come to light, however, there is now reason to question the basis for EPA's endangerment finding and thus the type of regulation that this finding may lead to.

In sum, given the seriousness of the flaws that the CRU material and other information reveal in the development of the IPCC reports, the Agency must reexamine the Endangerment Finding. The Agency can no longer have confidence that those reports present a fair, unbiased and accurate assessment of climate science. Since these reports were relied on extensively in the Endangerment Finding, the Agency has no choice but to conclude that the Endangerment Finding itself is now tainted and must be reconsidered.

Peabody

Peabody is the world's largest private sector coal company. Peabody supports the deployment of next-generation green coal technologies to achieve the goal of zero or near-zero emissions from the use of coal. Peabody is involved in a variety of efforts worldwide to make this goal a reality and has made significant investments in these technologies. Last fall, for instance, Peabody became a full equity participant with Chinese companies in the 650 MW GreenGen power project, a commercial scale near-zero emissions power project that is under construction near Tianjin, China. In a joint statement issued by President Barack Obama and President Hu Jintao in Beijing at the time that Peabody's investment was announced, the two world leaders recognized the importance of GreenGen and other projects.

EPA regulation under the CAA will not be a cost-effective way of reducing GHG emissions; indeed, EPA regulation of GHGs under the CAA will likely do more harm than good.

The Agency itself has stated that congressional action is preferable to CAA regulation. Peabody believes that the best way to reduce worldwide GHG emissions from the use of coal is through congressional action to incentivize these new technologies. Peabody knows that EPA also believes in incentivizing new technologies. Although we have our differences, Peabody hopes to be able to work with EPA and the Administration in the future on an overall legislative approach. We share the same goal: reducing global GHG emissions. The question is the best way to do so.

The Risk of Reaching a Wrong Endangerment Finding

The Endangerment Finding is the foundation on which EPA will build far-reaching GHG regulation of virtually all facets of the United States economy. The regulation that will follow the Endangerment Finding will not just be limited to new motor vehicles and new motor vehicle engines under section 202(a) of the CAA, but will extend to numerous categories of stationary sources, both under the Prevention of Significant Deterioration Program and otherwise under Titles I and V, and to mobile sources under Title II.

Most affected by EPA GHG regulation will be the combustion of fossil fuels, and particularly coal, since combusting fossil fuels inevitably produces carbon dioxide ("CO_2"), the most ubiquitous of the GHGs. Eighty-five percent of all energy used in the United States is derived from fossil fuels, and approximately fifty percent of the electricity used in the United States is produced from coal. Indeed, according to EPA, "[v]irtually every sector of the U.S. economy is either directly or indirectly a source of GHG emissions."[13]

In comments on the Endangerment Finding,[14] Peabody explained that EPA's approach to assessing possible endangerment from combustion of fossil fuels was one-sided. EPA assessed

[13] Control of Emissions From New Highway Vehicles and Engines, 68 Fed. Reg. 52,922, at 52,928 (Sept. 8, 2003).
[14] EPA-HQ-OAR-2009-017-3261.

only what EPA believes are the dangers to health and welfare that are created by the emissions produced by fossil fuel combustion. EPA, however, did not assess the benefits to health and welfare that are created by the energy that results from fossil fuel combustion.

As Peabody explained, GHGs are obviously emitted for a reason; they are the inevitable byproduct of the combustion of fossil fuels for energy or the end result of some other process. For this reason, when EPA assesses whether the emission of GHGs endangers public health and welfare, EPA must assess the dangers and benefits on both sides of the point where the emissions occur: in the atmosphere where the emissions lodge and on the other side of the emitting stack or structure, in the processes that create the emissions. Otherwise, EPA will not be able to accurately assess whether society's emission of GHGs is a benefit or a detriment.

Because GHG emissions, particularly CO_2 emissions, are so closely tied with all facets of modern life, a finding that GHG emissions endanger public health and welfare is akin to saying that modern life endangers public health or welfare. But plainly just the opposite is the case. The combustion of fossil fuels has created a level of health and welfare that would have been unimaginable in pre-industrial society. Indeed, the obvious benefits of combusting fossil fuels present a paradox to EPA in making its Endangerment Finding: as the world has combusted more and more fossil fuel and therefore has emitted more and more GHGs, virtually every measurement of public health and welfare has improved.

EPA responded to Peabody's comments and similar comments of other parties by saying that these comments were legally irrelevant. According to EPA, the CAA provides for a two-step regulatory process. First, EPA decides whether there is endangerment created by the emission of air pollutants and then, at a subsequent point, EPA decides on appropriate regulation.

EPA stated that the concerns raised by Peabody and others pertained to the consequences of regulation and were therefore relevant at step two and not at step one.[15]

Peabody believes that EPA's legal analysis is faulty and will challenge it at the appropriate time and in the appropriate forum. EPA's view of the statute, however, emphasizes all the more why EPA must especially ensure that its Endangerment Finding is developed through a rigorous and transparent analytical process – and therefore why EPA should be especially concerned about the CRU material. If EPA is going to defer weighing the risk of regulating against the risk of not regulating to step two of the regulatory process, then it must make sure at step one that the scientific process has been conducted with integrity and neutrality so as to correctly identify the danger that its step two regulations will address. Otherwise, the Agency will not be able to determine what level of regulation, if any, is justified and it may end up causing large harms for little, if any, benefit.

EPA Reliance on IPCC Reports

Section 202(a) of the CAA plainly requires that the Administrator make a "judgment" as to whether the emission of air pollutants poses a danger to public health and welfare. Throughout the Endangerment Finding, however, the Administrator frankly admitted that the Agency did not itself conduct a comprehensive review of climate change science in making its judgment that anthropogenic GHG emissions create endangerment. Instead, EPA relied primarily on what it termed the "assessment literature" in reaching its scientific conclusions.[16]

Although the "assessment literature" on which the Administrator relied generally consisted of the work of both the IPCC and the U.S. Climate Change Science Program ("CCSP")

[15] Endangerment Finding, 74 Fed. Reg. at 66,515-16.

[16] EPA's reliance on the "assessment literature" is discussed at section III(A)(4) of our Petition.

science assessment reports,[17] she relied primarily on the work of the IPCC on the critical issue of whether anthropogenic GHGs are causing climate change. Most of the TSD examined observed and projected climate and the effect on public health and welfare. Only eight pages of the Endangerment Finding Technical Supporting Document ("TSD"), however, were devoted to the critical "attribution" issue: whether changes to the climate system that EPA says are occurring and will accelerate can be attributed to anthropogenic GHG emissions and not natural forces.[18] The attribution section of the TSD particularly relied on the work of the IPCC, as opposed to the other "assessment literature" or any other studies. We count 67 citations in this section, with 47 to the IPCC. All the graphics in this section were taken from the IPCC, as was the introduction. Plainly, the principal authority for EPA's central conclusion that anthropogenic GHG emissions are causing deleterious climate change was the IPCC.

EPA recognized in the Endangerment Finding that it is responsible for verifying that scientific information on which the Agency relies meets standards for quality, integrity and transparency that are set forth in U.S. law, including the CAA and the IQA.[19] EPA stated that it ensured compliance with these standards here by reviewing the IPCC's written procedures for preparation of that body's science assessment reports.[20] Based on that review, EPA determined that the IPCC had procedures in place to ensure "a basic standard of quality, including objectivity, utility and integrity."[21] Accordingly, EPA concluded that it had "no reason to

[17] The CCSP has now been subsumed into the U.S. Global Change Research Program ("USGCRP"). CCSP issued a series of synthesis and assessment reports ("SAPs"), and these reports, along with the IPCC reports, became the principal basis for the June 2009 USGCRP report GLOBAL CLIMATE CHANGE IMPACTS IN THE UNITED STATES. available at http://downloads.globalchange.gov/usimpacts/pdfs/climate-impacts-report.pdf.

[18] TSD at 47-54.

[19] TSD at 4.

[20] EPA Response to Public Comments ("Resp. to Comm.") Vol. 1 at 9-23.

[21] *Id.* at 57.

believe" that the "assessment reports do not represent the best source material to determine the state of the science and the "consensus" view of the world's scientific experts on the issues central to making an endangerment decision with respect to greenhouse gases."[22]

The CRU material and other recently released information, however, reveals that EPA's trust in the IPCC's written procedures was misplaced. Based on this material, EPA now does have reason to conclude that the IPCC reports were not the product of a rigorous, transparent and neutral scientific process. The effect of EPA's reliance on the IPCC is that the Agency delegated its obligation to make a judgment as to whether GHGs may endanger public health and welfare to an international body that acted in direct contravention of basic U.S. information standards. EPA cannot, consistent with law, regulate based on that foundation.

This concern is particularly acute given that EPA asserts authority to protect the public health against an "air pollutant" that EPA concedes does not create a direct public health concern from inhalation or exposure as is the case for traditional air pollutants. In fact, CO_2 is a naturally-occurring substance that is necessary for life on Earth. As shown in Peabody's Endangerment Finding comments, a large body of peer-reviewed studies shows the benefits of increased CO_2 on plant productivity in general, including agricultural crops. It is thus not a "pollutant" that endangers public health or welfare.

Of course, too much of any substance can lead to damaging consequences – for instance, too much water can lead to flooding. EPA's view is that, because of human activities, there is too much CO_2 and other GHGs in the atmosphere and that, as a result, climate will change leading to indirect effects on health. But these indirect health impacts are asserted to be caused by, at best, a highly uncertain chain of cause and effect. Even if EPA has authority to expand its

[22] Endangerment Finding, 74 Fed. Reg. at 66,511.

regulatory mandate to regulate asserted indirect health effects, which Peabody disputes, such expansion could be justified only if this chain of cause and effect could be proven with a high level of confidence. Just the opposite occurred here, however; EPA's regulatory expansion was based on IPCC reports prepared in a manner that demonstrably did not ensure data quality, integrity and transparency and which therefore cannot be found to produce reliable results.

In sum, EPA must reconsider its Endangerment Finding.

CRU Materials Implicate Key IPCC Actors on the Critical Attribution Issue

A temptation may exist to dismiss the abuses revealed in CRU material as the product of a few scientists at a single English institution. But, the CRU is responsible for the development of critical datasets central to climate change studies and is acknowledged as a "primary data source by climate scientists around the world."[23] Furthermore, the main scientists implicated in these emails were not just minor research scientists at CRU. Those implicated include both leading CRU scientists and senior scientists at leading American climate science institutions who were in close contact with the CRU scientists. They are the leaders in the fields of climate observations and paleoclimate, two of the key areas from which the IPCC (and therefore EPA) drew in determining that anthropogenic GHGs are affecting climate. These scientists include:

- Dr. Phil Jones, Director of the Climatic Research Unit and one of two coordinating lead authors of Chapter 3 of AR4 addressing observed climate.
- Dr. Kevin E. Trenberth, head of the Climate Analysis Section at the National Center for Atmospheric Research ("NCAR") and the other coordinating lead author of Chapter 3 of AR4.
- Dr. Keith R. Briffa of CRU, a lead author of Chapter 6 of AR4.
- Dr. Jonathan Overpeck of the University of Arizona, one of two coordinating lead authors of Chapter 6 of AR4 addressing paleoclimate.

[23] History of the Climate Research Unit *available at* http://www.cru.uea.ac.uk/cru/about/history/.

- Dr. Eystein Jansen of the University of Bergen, Norway, the other coordinating lead author of Chapter 6.

- Dr. Michael Mann, a lead author of Chapter 2 of the TAR, one of the leading figures in the field of paleoclimatology, and the scientist whose "hockey stick" graph became the single most important piece of information in the TAR.

- Dr. Benjamin D. Santer of the Lawrence Livermore National Laboratory and lead author of Chapter 8 of the 1995 IPCC report.

- Dr. Thomas Wigley, former head of the CRU, PhD advisor to Drs. Santer and Jones, senior scientist at the University Corporation for Atmospheric Research and a 1995 IPCC report lead author, a TAR contributing author, and an AR4 contributing author.

- A number of other scientists who worked for and with these scientists.

Moreover, the information revealed in the disclosures is significant enough that investigations have been launched of the actions of the involved scientists by the UEA[24] and The Pennsylvania State University[25] and now by the Science and Technology Committee of the United Kingdom Parliament.[26] As stated, the U.K. government agency responsible for overseeing and enforcing U.K. freedom of information laws has concluded that those laws were broken. Although it has concluded that prosecutions cannot be brought because of the applicable statute of limitations, it says it will seek to change the laws to lengthen the limitations period.[27]

[24] Press Release, CRU Update 3, *Professor Phil Jones has today announced that he will stand aside as Director of the Climatic Research Unit until the completion of an independent Review resulting from allegations following the hacking and publication of emails from the Unit* (Dec. 1, 2009), *available at* http://www.uea.ac.uk/mac/comm/media/press/2009/dec/CRUphiljones.

[25] Public Statement, Penn State University, University Reviewing Recent Reports on Climate Information, *available at* http://www.ems.psu.edu/sites/default/files/u5/Mann_Public_Statement.pdf.

[26] http://www.parliament.uk/parliamentary_committees/science_technology/s_t_pn14_100122.cfm.

[27] *See Climate row unit 'broke data law,'* Jan. 28, 2010 *available at* http://news.bbc.co.uk/2/hi/uk_news/8484385.stm.

Attribution: The Key Findings

In concluding that the "scientific evidence is compelling" that anthropogenic GHG emissions – as opposed to natural forces – are causing dangerous climate change, EPA relied on what it terms "three lines of evidence." The first is based on the "physical" understanding of the climate. The second is the determination that the temperatures of the last "several decades" are unusual and even unprecedented during the current interglacial period known as the Holocene, when the world's climate system has been similar to that of today, and particularly during the last 1000-2000 years when more is known about climate. The third is based on computer model simulations.[28]

All three of these lines of evidence are undermined by the CRU emails, although the second line of evidence as to whether temperatures of the last several decades are unprecedented during the Holocene is the area in which the abuses are most notable.

The Attempt to Present a "Nice Tidy Story" of Unprecedented 20th Century Warmth

Placing current climate in context within the paleoclimate has always been considered to be of exceptional importance in determining whether an anthropogenic GHG influence can be detected in the current climate. After all, if temperature conditions during the paleoclimate were as high or higher than today, it becomes difficult to conclude that anthropogenic GHG emissions are the cause of current temperatures. As the IPCC itself stated, "To determine whether 20th century warming is unusual, it is essential to place it in the context of longer-term climate variability."[29]

[28] TSD at 47.

[29] TAR at § 2.3.1.

In particular, temperature during the Holocene has fluctuated over multi-century periods, and during the last 1000 years, there was a period of warming known as the Medieval Warm Period ("MWP"), followed by a Little Ice Age ("LIA") that lasted through about 1850, followed by two periods in which temperatures rose, 1910-1945 and 1977-1998, and now followed by an eleven-year period of no warming. The magnitudes and duration of the two periods of 20th century warming are statistically similar, with EPA admitting that the first period did not result from the combustion of fossil fuels as there was little increase during this time of atmospheric carbon dioxide.[30] It has also long been recognized that early in the Holocene there were multi-century periods (known as the "Holocene Thermal Maximum") when temperatures were warmer than today.[31]

Recognizing the importance of the paleoclimate issue and driven by an agenda, those involved with writing the paleoclimate sections of both the 2001 TAR and the 2007 AR4 appear to have set out to create a picture of unprecedented 20th century warmth during the Holocene, with particular attention to the last 1000 years.[32] The key way they did this was through unjustified reliance on highly uncertain proxy temperature reconstructions of climate over the last millennium using tree rings and other information as indicators of temperatures. Based on these reconstructions, the TAR and AR4 concluded that any warming of the MWP was localized and not a global phenomenon. This enabled the conclusion that the warming in the 20th century was so unprecedented in the last 1000 years that it was most likely caused by anthropogenic GHG emissions.

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[30] TSD at 45.

[31] *See, e.g.*, IPCC First Assessment Report, CLIMATE CHANGE: THE IPCC SCIENTIFIC ASSESSMENT 202 (Cambridge University Press 1990) ("FAR").

[32] The attempts in the TAR and AR4 to create a clean picture of unprecedented 20th century warmth are discussed more fully in our Petition at section IV(C).

The CRU emails, however, reveal that the authors of this material did not present a neutral view of the science. In particular, they downplayed the considerable uncertainty inherent in trying to approximate temperatures from proxy data over a 1000-year period, they suppressed contrary information, and they suppressed dissenting views in ways that made even their own colleagues uncomfortable. Thus, in one representative email written during the preparation of the TAR, Keith Briffa stated that "***I know there is pressure to present a nice tidy story as regards 'apparent unprecedented warming in a thousand years or more in the proxy data' but in reality the situation is not quite so simple.***"[33] He went on to say that "I believe that the recent warmth was probably matched about 1000 years ago."[34] Similarly, another key researcher, Ed Cook, in a lengthy email bristling at the effort to eliminate the MWP, wrote that "***I do find the dismissal of the Medieval Warm Period as a meaningful global event to be grossly premature and probably wrong.***"[35]

These concerns, however, were brushed aside in the final TAR. The TAR's version of the temperature record of the last 1000 years was based on the now infamous "hockey stick" study of Mann et al., a study that purported to show 1000 years of slightly declining global temperatures followed by a sharp increase in the 20th century. The hockey stick paper concluded that the 1990s were the warmest decade and 1998 was the warmest year in a millennium. The hockey stick graph was the single most important piece of information in the TAR. It was Figure 1 of the Summary For Policymakers of the TAR appearing on page 3, and it was widely relied on by advocates.[36]

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[33] CRU email 938018124.txt (Sep. 22, 1999) (emphasis added).

[34] *Id.*

[35] CRU email 988831541.txt (May 2, 2001) (emphasis added).

[36] *See* discussion in our Petition of this matter at section IV(C)(3).

Despite its prominence in the TAR, the hockey stick has now largely been discredited, with both the National Research Council ("NRC")[37] and the independent Wegman Report[38] rejecting confidence in the conclusion that the 1990s were the warmest decade and 1998 was the warmest year in a millennium. Although the hockey stick paper was cited in AR4, its significance was downplayed, and EPA did not cite the paper in the Endangerment Finding or TSD.

However, the same people who gave that paper such prominence in the TAR – despite the misgivings expressed internally within the group – continued to dominate paleoclimate research and were again the leading authors of the AR4 paleoclimate material. Indeed, perhaps stung by criticisms of the hockey stick and by the appearance of so-called "skeptics" who questioned the central conclusions of the TAR, the drafting of at least the paleoclimate chapter of AR4 became more of a political than a scientific process.[39]

Thus, the two coordinating lead authors of Chapter 6 of AR4, Jonathan Overpeck of the University of Arizona and Eystein Jansen of the University of Bergen in Norway, openly coached contributors to produce materials that would serve a public policy agenda. As just a few examples, the CRU emails show that Overpeck instructed his colleagues to make sure that text was "***FOCUSED on only that science which is policy relevant"*** and that would support pre-conceived summary bullet points.[40] The pair also advised authors to include graphics that would be "compelling" and that the "sign of ultimate success" of a graphic would be that it was so

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[37] National Research Council, SURFACE TEMPERATURE RECONSTRUCTIONS FOR THE LAST 2,000 YEARS (National Academy Press 2006) ("NRC Report").

[38] Edward Wegman et al. AD HOC REPORT ON THE "HOCKEY STICK" GLOBAL CLIMATE RECONSTRUCTION. ("Wegman Report") (July 27, 2006) *available at* http://republicans.energycommerce.house.gov/108/home /07142006_ Wegman_Report.pdf.

[39] This matter is discussed more fully in our Petition at section IV(C)(1)(c).

[40] CRU email 1121392136.txt (Jul. 14, 2005) (capitals in original) (emphasis added).

compelling that it would be selected for use in the policymaker's summary.[41] They told authors to "pls DO please try hard to follow up on my advice" to only refer to the MWP and the Holocene Thermal Maximum in a "dismissive" way.[42] They expressed satisfaction with a graphic that described the MWP as heterogeneous – meaning that warming was not uniform on a planetary scale – not because it was accurate but because it read "much like a big hammer," driving home the point they wished to make.[43] Moreover, although the hockey stick could no longer be relied on as a principal source of authority, authors were instructed that "[w]e're hoping you guys can generate something compelling enough" for the summary material for policymakers, "something that will replace the hockey-stick with something even more compelling."[44] Yet new research that reexamined the data on which the IPCC relied has challenged the IPCC's dismissal of the MWP as non-heterogeneous, concluding that the IPCC's conclusion in this regard was, at least, "premature" and based on limited data.[45]

A representative case in point as to how these scientists treated conflicting information is revealed in emails concerning two studies addressing the Holocene Thermal Maximum. Coordinating lead author Jansen's view was that the extended warm temperatures of that period were caused by orbital wobbles, with the evidence being (in his view) that the warmth was not globally synchronous and was instead dominated by high-latitude summer warming – consistent with the projections of climate models run with the orbital parameters characteristic of that period. That was the explanation that appeared in AR4, and that is the explanation that EPA

[41] *Id.*

[42] CRU email 1105670738.txt (Jan. 13, 2005).

[43] CRU email 1105978592.txt (Jan. 17, 2005).

[44] CRU email 1116902771.txt (May 23, 2005).

[45] Jan Esper and David Frank, *The IPCC on a heterogeneous Medieval Warm Period,* 94 CLIM. CHNG. 267-272 (2009).

adopted in reliance on AR4.[46] Jansen had a problem, however, in that his co-authors brought to his attention two papers published in peer-reviewed literature showing that the warmth of this period was not restricted to the high latitudes. Jansen's solution? He dismissed the papers as wrong and did not allow them even to be referred to in text.[47]

The examples of this type of behavior abound. Jones told Mann in a 2004 "HIGHLY CONFIDENTIAL" email that he "can't see" either of two papers that they didn't like "being in the next IPCC report. ***Kevin and I will keep them out somehow - even if we have to redefine what the peer-review literature is!"***[48] These scientists also acted inappropriately as both contributors and reviewers despite the obvious conflict of interest.[49] They enlisted the aid of their colleagues in the drafting process without disclosing that fact.[50] They manipulated publication deadlines so that papers supporting their views could be included.[51] And they acted as peer reviewers of scientific papers in order to influence the literature on which they intended to rely as IPCC authors.[52]

These actions are not those of neutral scientists trying to present an accurate summary of the findings of paleoscience. They are the actions of advocates building a case. But science is not supposed to be developed on the basis of whether or not it is "policy relevant" – it is supposed to be honest, accurate and neutral – and scientific discussion should not be circumscribed for the purpose of supporting a simple bullet point or presented in ways that are

[46] TSD at 49.

[47] This matter is discussed in more detail in our Petition at section IV(D).

[48] CRU email 1089318616.txt (Jul 8, 2004) (all capitals in original). Although both of these papers were eventually cited in the text, they were referred to dismissively without substantive reason.

[49] *See* our Petition at section VII (B).

[50] *See Id*. at section VII (C).

[51] *See Id*. at section VII (D).

[52] *See Id*. at section VII (E).

considered to be "compelling." Nor should supposedly neutral scientific summaries omit studies containing data that contradict model predictions and undermine the case that the authors wish to present. And, most obvious of all, science reports should not contain demonstrably incorrect information supplied by agenda-driven advocacy groups. The AR4 material, thus, does not comport with good science and should not be treated as such by EPA.

The "Trick" to "Hide the Warming"

Much attention has been placed on Jones' now-famous email in which he stated that "I've just completed Mike's Nature trick of adding in the real temps to each series for the last 20 years (ie from 1981 onwards) amd from 1961 for Keith's to hide the decline."[53] The trick he and Mann performed was to hide a decline in temperatures appearing in tree ring data in the latter part of the 20th century. Unless this trick were used, their multi-century proxy temperature reconstructions would show an embarrassing decline in temperatures at the end of the reconstruction, a decline that was not paralleled in the record of directly measured temperatures, which showed an increase. To hide the decline in the proxy data, Mann and then Jones grafted on actual temperature data to the end of their proxy reconstructions rather than using the same proxy data as had been used throughout the reconstruction.

This trick makes the graphic presentations of the proxy reconstructions misleading, since the effect is to make it seem as if the proxy data shows rising 20th century warming when it doesn't. But the real deception in the trick was in hiding what became known as the "divergence" problem. The accuracy of tree ring data as proxies for temperatures can only be confirmed by comparing the proxy temperatures yielded by the tree rings with temperatures directly measured during the period when direct temperature measurements could be made. If

[53] CRU email 942777075.txt (Nov. 16, 1999).

the proxy data are contradicted by actual data, as they are for a significant period of the time when direct temperature measurements exist, the accuracy of the proxy data over the entire period of the proxy reconstruction is called into question. Thus, the divergence problem undermined faith in the ability of the proxy reconstructions to provide conclusive or even meaningful information about paleoclimate temperature conditions, even as the IPCC was relying on these reconstructions to conclude that temperatures in the 20th century had reached unprecedented levels in the last 1000 years. As one email candidly said, "[t]he issue of why we dont show the proxy data for the last few decades (they dont show continued warming) but assume that they are valid for early warm periods needs to be explained."[54] These concerns, however, were given short shrift. Although divergence was discussed in AR4, the conclusion was reached that the results of the proxy temperature reconstructions remained valid and showed that 20th century warmth was likely unprecedented in 1000 years. If divergence was not a significant issue, however, one wonders why it was necessary to perform "tricks" to hide the problem.[55]

More importantly, after AR4 was issued, at least three studies have been published reanalyzing the data used in the proxy reconstructions cited in AR4, including two by authors whose reconstructions were used in AR4. These studies concluded that, in fact, the divergence problem makes the reconstructions unreliable.[56] According to one study, the divergence problem "serve(s) to impede a robust comparison of recent warming during the anthropogenic period with

[54] CRU email 1150923423.txt (Jun. 21, 2006).

[55] The "trick" and the divergence issue is discussed more fully in our Petition at section IV (C)(2).

[56] These studies are discussed in our Petition at section IV (C)(2)(d).

past natural climate episodes such as the Medieval Warm Period or MWP."[57] Another study found that the divergence problem makes it "impossible to make any statements about how warm recent decades are compared to historical periods."[58] Another concluded that the divergence problem "is of importance, as it limits the suitability of tree-ring data to reconstruct long-term climate fluctuations, particularly during periods that might have been as warm or even warmer than the late twentieth century."[59]

It would seem, therefore, that the IPCC should have been more cautious in dismissing the divergence problem. It would also seem that the IPCC may have understood that there was something to hide after all.

How Do These Flaws in the IPCC Findings on Paleoclimate Affect the Endangerment Finding?

The Endangerment Finding recognizes that there is "significant uncertainty" as to temperatures prior to about 1600, citing both the IPCC and the NRC report *Surface Temperature Reconstructions for the Last 2,000 Years* (2006). Although the IPCC's AR4 did in fact recognize uncertainty in attempts to determine temperatures prior to 1600, it nevertheless found it "likely," which it defined as a 66-90% probability, that the second half of the 20th century was warmer than any comparable period in the Northern Hemisphere over the last 1300 years and that this warmth was more widespread globally than during any other comparable period over the

[57] Rosanne D'Arrigo, et al., *On the 'divergence problem' in northern forests: a review of the tree-ring evidence and possible causes,* 60 GLOB. PLANET. CHNG. 289 (2008).

[58] Craig Loehle, *A mathematical analysis of the divergence problem in dendroclimatology,* 94 CLIM. CHNG. 233 (2009).

[59] Jan Esper and David Frank, *Divergence pitfalls in tree-ring research,* 94 CLIM. CHNG. 261, 262 (2009).

last 1300 years.[60] The CRU emails show that the AR4's statement as to the "likely" unprecedented warming of the 20th century cannot be credited.

More to the point is EPA's reference to the NRC report as to the uncertainty of temperature reconstructions prior to 1600. In fact, the NRC's discussion of uncertainty concluded with the statement that, because of the uncertainty, it is no more than "plausible" that the Northern Hemisphere was warmer during the last few decades of the 20th century than during any comparable period over the preceding millennium.[61]

EPA's reliance on the NRC Report on the issue of uncertainty creates a dilemma for the Agency in defending the Endangerment Finding. As stated above, EPA determined that "compelling" scientific evidence supports the conclusion that anthropogenic GHG emissions are the "root cause of recently observed climate change." According to EPA, one of three lines of such "compelling" scientific evidence is that the temperatures of the last several decades are so unusual in the last 1000-2000 years that anthropogenic GHG emissions are very likely the cause. However, the significant uncertainty that the NRC identified as to temperatures during the MWP, and which is reflected in the CRU emails and subsequent scientific studies, directly undercuts this line of evidence. Evidence that it is no more than "plausible" that temperatures of the last several decades are the highest in 1000 years can hardly be deemed to be "compelling" evidence that anthropogenic GHGs must be the "root cause" of those recent temperatures.

[60] AR4 Ch. 6 at Executive Summary.

[61] NRC Report at 20-21.

What to Make of the Current 11-Year Trend of No Warming?

According to temperature data on which both EPA and the IPCC rely, the earth has experienced no warming over an 11-year period.[62] This lack of warming undermines EPA's other two proffered lines of evidence – in addition to evidence that the current warming is likely unprecedented in the last 1000 years – for its conclusion that anthropogenic GHG emissions are primarily responsible for changes in the climate.

One of these lines of evidence is EPA's "physical understanding" of the climate system. According to that understanding, GHGs trap heat and, therefore, as GHGs accumulate in the atmosphere, the planet should warm. In fact, according to EPA, in reliance on the IPCC, the planet should warm beyond the level that would be produced as a direct response to the radiative effect of the GHGs themselves, as the IPCC believes that the direct radiative effect should produce indirect positive feedbacks in the atmosphere magnifying the warming significantly.[63]

The other line of evidence is the results of computer simulation models, which of course are based on the modelers' physical understanding of the climate. In conformance with that understanding, the models show that increasing concentrations of GHGs produce warming.[64]

What then to make of the current period of no warming? According to EPA, that lack of warming is produced by natural variability. EPA stated that warming caused by anthropogenic GHG emissions will not necessarily be uniform but instead could be muted by natural forces for a period of a decade or two. In particular, EPA cited two recent studies that attempted to show

[62] Resp. to Comm. Vol. 3 at 3.

[63] TSD at 23-26.

[64] *Id.* at § 6(b).

that the GHG models on which the IPCC, and therefore EPA, relied show sufficient natural variability to accommodate periods of no warming.[65]

Each of these studies has flaws discussed in the body of the Petition that result in an overstating of the likelihood that the models can account for the lack of warming. But even taken at face value, these studies should provide little comfort to EPA. One of the studies found that during the first half of the 21st century, there is a 1 in 10 chance of a zero (or negative) trend in temperatures through 10 years of data. The other study found that for the entire 21st century there is a five percent chance of a zero (or negative) trend through 11 years of data. Given these very low odds, and given that this trend occurred in the first decade of the 21st century and we have already experienced an 11-year trend of no warming, these studies hardly provide reassuring support for the underlying accuracy of the models' long-term predictive capacity.[66]

Adding to the questions about the accuracy of climate models are new results that show water vapor variations in the lower stratosphere play a large role in the variability global temperature trends over scales of several decades—influencing recent trends by some 25% to 30%. The physics governing lower stratospheric water vapor content are quite limited in current climate models, and the observed trends are poorly simulated.[67]

In fact, the CRU emails reveal that the lack of warming has caused leading IPCC scientists to question the assumed physical understanding of the climate system on which the models are based. Just last fall, even after the studies that EPA relied on had been produced, Trenberth conceded that the lack of warming exposes science's basic lack of understanding of

[65] Resp. to Comm. Vol. 4 at 23-24.

[66] These studies are discussed more fully in our Petition at section V(B).

[67] Susan Solomon et al., 2010. *Contribution of Stratospheric Water Vapor to Decadal Changes in the Rate of Global Warming*. SCI (forthcoming 2010) published online at http://www.sciencemag.org/cgi/rapidpdf/science.1182488v1.pdf

the climate system: "***Saying it is natural variability is not an explanation. What are the physical processes? Where did the heat go***?"[68] Trenberth concluded that either the understanding of the climate system reflected in the climate models is wrong:

> ***How come you do not agree with a statement that says we are no where close to knowing where energy is going or whether clouds are changing to make the planet brighter. We are not close to balancing the energy budget. The fact that we can not account for what is happening in the climate system makes any consideration of geoengineering quite hopeless as we will never be able to tell if it is successful or not! It is a travesty***![69]

Or else the data is wrong:

> ***The fact is that we can't account for the lack of warming at the moment and it is a travesty that we can't.*** The CERES data published in the August BAMS 09 supplement on 2008 shows there should be even more warming: but the data are surely wrong. Our observing system is inadequate.[70]

Or perhaps both. It is, moreover, particularly relevant that Trenberth stated that "[t]he fact that we can not account for what is happening in the climate system makes any consideration of geoengineering quite hopeless as we will never be able to tell if it is successful or not!" Trenberth's reference to "geoengineering" here includes reducing GHG emissions.[71] In other words, Trenberth stated that the flaws in the climate community's understanding of climatic forces that are exposed by the lack of warming is so fundamental – and the extent of natural variability must be so great – that it cannot be demonstrated that reducing GHG emissions will reduce warming.

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[68] CRU email 1255523796.txt (Oct. 14, 2009) (emphasis added).

[69] *Id.* (emphasis added).

[70] *Id.* (emphasis added).

[71] Trenberth has publicly (and recently) referred to attempts to "reduce emissions... or reduce the amount of carbon dioxide in the atmosphere" as "geoengineering." *See* Physics Today letter 2/09, at http://www.cgd.ucar.edu/cas/Trenberth/trenberth.papers/GeoengineeringPhsToday.pdf.

Trenberth's statement would seem to eviscerate the grounds for EPA's Endangerment Finding. The purpose and effect of that finding is to trigger regulation mandating GHG reductions to eliminate or at least mitigate the danger. But if, as Trenberth says, the science is too uncertain to determine whether GHG reductions will produce a measurable climate response, there is no basis to regulate and no basis to express confidence that anthropogenic GHG emissions are almost certainly the dominant cause of the warming of the last several decades.

Abject Lack of Transparency

The CRU materials also show a determined effort to stonewall attempts by third parties to obtain basic information underlying the scientific studies that were used in the IPCC reports. A considerable volume of transatlantic email traffic between the CRU scientists and their American counterparts was devoted to figuring out strategies to avoid producing information that could be used to critique their work, even when the information was requested under the American or United Kingdom Freedom of Information Acts ("FOIA").[72]

The emails reveal that these scientists refused to disclose information that would allow their studies to be replicated and critiqued because they saw themselves in a battle with "skeptics" who they considered to be "bozos" and "morons" and perpetrators of fraud.[73] They appeared to be particularly concerned that putting their information in the public domain would expose their work to criticism. As Jones said in one now-famous email, "We have 25 years or so invested in the work. ***Why should I make the data available to you, when your aim is to try and find something wrong with it?***"[74] Jones' view was echoed by Mann. As Jones reported, "Mike

[72] This issue is discussed more fully in our Petition at section VI (C).

[73] CRU email 1146062963.txt (Apr. 26, 2006); CRU email 1147435800.txt (May 12, 2006); CRU email 1107899057.txt. (Feb. 8, 2005).

[74] Email provided by Warwick Hughes to whom the email was sent.

Mann refuses to talk to these people and I can understand why. ***They are just trying to find if we've done anything wrong***."[75]

Indeed, "these people" were trying to find something wrong, and well they should. That's how science should work.

The emails reflecting the stonewalling of attempts to obtain underlying data as discussed in the Petition are not taken out of context and reflect a steady course of conduct over a decade-long period by the same network of scientists who were principally responsible for authoring Chapters 3 and 6 of AR4. The stonewalling was comprehensive – anyone considered to be associated with the "skeptical" camp was refused as much underlying information as possible. Most troubling from the point of view of the transparency of the IPCC process, the stonewalling extended to any information concerning the drafting of AR4, with the scientists taking the position that no country's FOIA governs the work of the IPCC, an international body.[76]

Indeed, concern over communications these scientists had had concerning the drafting of AR4 was so great that they mutually agreed to destroy those communications in order to avoid disclosure under FOIA. Thus, on May 29, 2008, Jones sent an email to Mann under the subject line "IPCC & FOI," asking that Mann delete his emails with Briffa and advising that he would make the same request to Eugene Wahl and Caspar Amman. Wahl and Amman co-authored a paper that attempted to rehabilitate the hockey stick. As shown in the Petition, publication deadlines were improperly manipulated in order to include the paper in AR4.[77] Jones wrote:

> ***Can you delete any emails you may have had with Keith re AR4?*** Keith will do likewise. He's not in at the moment — minor family crisis. ***Can you also email Gene and get him to do the same?*** I

[75] CRU email 1091798809.txt (Aug. 6, 2004) (emphasis added).

[76] *See* the Petition at section VI (D).

[77] *See* the Petition at section VII (D).

> don't have his new email address. We will be getting Caspar to do likewise.[78]

Later in the same thread, Mann responded to Jones that he would "contact Gene about this ASAP."[79] Several months later Jones reported that he had in fact "***deleted loads of emails***."[80] One is forced to wonder what damaging admissions were made in these now-deleted emails concerning how AR4, in fact, was prepared.

After the efforts of these scientists to stonewall data requests were exposed to public scrutiny through FOIA and now through release of the CRU material, many of them were forced to admit that their actions were not in the best interests of science. Wigley told Briffa that "***many *good* scientists appear to be unsympathetic***" to the reasons advanced for the stonewalling.[81] Overpeck wrote in relation to one information request that "***it would be nice if he could have access to all the data that we used—that's the way science is supposed to work.***"[82] And now John Beddington, the British government chief scientific adviser, has recently said, "I think, wherever possible, we should try to ensure there is openness and that source material is available for the whole scientific community."[83]

[78] CRU email 1212073451.txt (May 29, 2008) (emphasis added).

[79] CRU email 1212063122.txt (May 29, 2008).

[80] CRU email 1228412429.txt (Dec. 3, 2008) (emphasis added).

[81] CRU email 1254756944.txt (October 5, 2009).

[82] CRU email 1252164302.txt (Sept. 5, 2009) (emphasis added).

[83] *As quoted* in Ben Webster, *Britain's chief scientist John Beddington calls for engagement with climate skeptics,* THE TIMES, Jan. 27, 2010, *available at* http://www.theaustralian.com.au/news/britains-chief-scientist-john-beddington-calls-for-engagement-with-climate-sceptics/story-e6frg6xf-1225823874671.

When the Administrator took office, she properly committed to science that would be "transparent" and conducted "in a fishbowl."[84] This commitment cannot be squared with reliance on IPCC reports that were prepared in such flagrant disregard of those principles.

Improper Editorial and Peer Review Practices in Drafting IPCC Reports

The CRU material reveals a series of improper practices in the drafting of the IPCC reports that confirms that the reports were agenda-driven and not a neutral presentation of science. The report authors rejected inclusion of or dismissed peer-reviewed papers that disagreed with their views, authors simultaneously acted as reviewers, contributing authors were not disclosed, publication deadlines were manipulated to included supporting papers, reviewer comments were rejected based on fabrications of the views of the authors of the relevant literature, and data sources used in unpublished papers that were included in the reports were not made available to reviewers. Perhaps worst of all, scientific conclusions were reached based on secondary material supplied by advocacy groups for the purpose of advancing the policy agendas of the IPCC's authors, conclusions that, perhaps not surprisingly, have now been forced to be retracted.

Publication Abuses

The CRU scientists and their American colleagues engaged in a variety of practices to manipulate the peer-reviewed literature to favor publication of papers that supported their views and to discourage publication of papers that contradicted their views. As Mann told a New York Times reporter, "[a] necessary though not in general sufficient condition for taking a scientific criticism seriously is that it has passed through the legitimate scientific peer review process."[85]

[84] January 23, 2009 memorandum to EPA employees.

[85] CRU email 1254259645.txt (Sep. 29, 2009) (emphasis added).

That being the case, these scientists took steps to ensure that "skeptics" did not have access to peer-reviewed literature.[86]

For instance, enraged that the journal Climate Research had published a paper presenting evidence that the MWP was global and as warm as today, these scientists discussed organizing a boycott to strong-arm the journal board into firing the offending editor. Jones wrote that the journal needed to "rid themselves of this troublesome editor."[87] Wholesale changes ensued at the journal.[88] Similar action was taken at Geophysical Research Letters after publication of an offending letter. Mann reported back to his colleagues that the problem had been solved: "[t]he GRL leak has been plugged up with new editorial leadership there,"[89] as if the appearance of a paper that did not support their view of the science was a "leak" in the peer-reviewed journalistic community that had to be "plugged."[90]

One of the most egregious abuses of the peer-reviewed literature occurred after these scientists found out that the International Journal of Climatology intended on publishing a paper by Douglass et al. demonstrating that data do not show the model-projected "fingerprint" of warming in the tropical troposphere.[91] This is a key issue in the Endangerment Finding, and EPA relied on the response of Santer et al. to the Douglass et al paper.[92] But the way in which the Santer et al. paper was produced was a direct violation of the norms that apply to peer-reviewed scientific literature. Santer and his group and others interfered with the editorial

[86] This issue is discussed more fully in our Petition at section VIII(A).

[87] CRU email 1047388489.txt (Mar. 11, 2003).

[88] The threats to boycott the Journal of Climate Research are discussed in our Petition at section VIII(A).

[89] CRU email 1132094873.txt (Nov. 15, 2005).

[90] The Geophysical Research Letters matter is discussed more fully in our Petition at VIII(A).

[91] This issue is discussed more fully in our Petition at section VIII (A)(2).

[92] EPA relied on Karl et al. (2009) (the USGCRP report), and that report in turn relied on the Santer et al. paper on this point.

process for publishing the Douglass et al. paper in order to slow down its publication and to advance the time when Santer et al. could publish a rejoinder. The purpose of this scheme was to ensure that the Santer et al. response was filed at the same time as the Douglass et al paper so that Santer et al. could have the last word, an outcome that these scientists knew was contrary to normal practice. To ensure quick publication of the Santer et al paper, peer reviewers were selected who were Santer's close associates so that the paper would receive a favorable review, again in complete contradiction of normal editorial practice (they discuss "achieving the quick turn-around time by identifying in advance reviewers who are both suitable and available").[93] This scheme was carried out without the knowledge of Douglass et al, and with the emails of the group expressing the need for extreme confidentiality and a concern about leaks, as if in recognition of their total disregard of normal peer-review journal ethics.

Conclusion

Dr. Briffa had it exactly right when he reported to his colleagues that "the needs of the science and the IPCC" "were not always the same." In fact, the IPCC process has been revealed to be as much about advocacy as about science. And the CRU material is only one thin slice of information concerning the drafting of the TAR and AR4. It seems that every day new revelations appear about flaws in the accuracy of the IPCC's conclusions and in the process that was used to select information that would, and would not, be included in the reports.

Given EPA's extensive reliance on the IPCC, particularly on the critical attribution issue, that taint now extends to the Endangerment Finding. EPA has effectively delegated its judgment under section 202(a) of the CAA to an international body that acted contrary to basic U.S. standards of information quality, integrity and transparency. In the interests of good science and

[93] *Id.* (emphasis added).

policy, and as required by law, EPA must now reconsider its Endangerment Finding in light of the CRU revelations. The importance of low-cost, reliable energy to the economy is too high for EPA to begin regulation based on such an uncertain foundation.

II.

SECTION 307 OF THE CLEAN AIR ACT REQUIRES RECONSIDERATION

Section 307(d)(7)(B) of the CAA provides that the Agency must reconsider a rulemaking decision if (1) a party brings new material to EPA's attention that could not have been raised during the comment period and (2) if such material is centrally relevant to the outcome of the rule. Under section 307(d)(7)(B):

> Only an objection to a rule or procedure which was raised with reasonable specificity during the period for public comments (including any public hearing) may be raised during judicial review. If the person raising an objection can demonstrate that it was impracticable to raise such an objection within such time or if the grounds for such objection arose after the period for public comment (but within the time specified for judicial review) and if such objection is of central relevance to the outcome of the rule, the Administrator shall convene a proceeding for reconsideration of the rule and provide the same procedural rights as would have been afforded had the information been available at the time the rule was proposed.

Since the CRU material was not released until November 2009, plainly the grounds for Peabody's Petition here arose after the June 2009 comment deadline. In addition, as shown below, the grounds for Peabody's Petition are of central relevance to the outcome of the Endangerment Finding. EPA is therefore now required to convene proceedings to reconsider that finding.

On December 2, 2009, the Competitive Enterprise Institute ("CEI") filed brief comments with EPA addressing the CRU material.[94] Apparently desiring to formally bring the CRU material to EPA's attention before EPA issued the Endangerment Finding, which at that point

[94] EPA-HQ-OAR-2009-0171-11537.

was expected soon, CEI's comments were necessarily cursory and did not reflect a point-by-point examination of the CRU material and its relevance to the Endangerment Finding.

EPA's Response to Public Comments document briefly addressed the CEI comments, stating that "it appears" to EPA that "the scientific issues raised in the e-mails were also raised in public comments."[95] Since CEI's comments were filed five days before the Endangerment Finding was released, we are not sure how EPA had time to review the voluminous CRU material in responding to CEI's comments. EPA does not state in its Response to Comments how comprehensive its review was and its discussion of the material is extremely general.

In any event, EPA's discussion of the CRU material is mostly directed towards unspecified statements made by unidentified groups as a part of the public debate that the CRU material has engendered.[96] EPA's brief discussion of unspecified and unattributed extra-record comments about the CRU material cannot substitute for the searching re-analysis of the Endangerment Finding that the CRU material demands.

[95] Resp. to Comm. Vol. 11 at 2.

[96] *Id*. at 2 ("Some groups have also used….We note that many of the concerns about the emails…").

III.

THE CRU MATERIAL CALLS INTO QUESTION EPA'S NEAR TOTAL RELIANCE ON WHAT IT TERMS THE "ASSESSMENT LITERATURE," AND PARTICULARLY THE WORK OF THE IPCC

A. Despite the Section 202(a) Requirement that the Administrator Exercise Her Own Expert Judgment, the Administrator Did Not Independently Judge the Science and Instead Relied Primarily on Summary Scientific Reports Produced by Third Parties

1. Section 202(a) Requires the Administrator to Exercise Independent Judgment

Section 202(a) is crystal clear that the EPA Administrator is obligated to exercise her own judgment in deciding whether or not to make an Endangerment Finding. According to section 202(a), "[t]he Administrator shall by regulation prescribe (and from time to time revise) in accordance with provisions of this section, standards applicable to the emission of any air pollutant from any class or classes of new motor vehicle engines, which may *in his judgment* cause, or contribute to, air pollution which may reasonably be anticipated to endanger public health or welfare."

2. The Administrator Concedes that She Relied Primarily on What She Characterizes as the "Assessment Literature" in Making Her Endangerment Finding

Despite being obligated to exercise her own judgment as to whether anthropogenic GHG emissions pose a danger to public health or welfare, the Administrator did not base her Endangerment Finding on her own review of primary scientific literature and data. Instead, as she readily conceded, she relied almost exclusively on reports produced by others summarizing their views of global climate change science, reports that she refers to as the "assessment literature." As the Endangerment Finding stated, "… the Administrator is relying on the major

assessments of the USGCRP,[97] the IPCC, and the NRC[98] as the primary scientific and technical basis of her endangerment decision."[99] The Administrator's statement of her primary reliance on these reports is repeated throughout the Endangerment Finding, the Technical Support Document ("TSD") and the Response to Public Comments document. For instance, the TSD stated that it "relies most heavily" on this "assessment literature."[100] The Response to Comments stated:

> The endangerment analysis for greenhouse gases under the CAA requires that EPA examine the extent to which the GHGs constitute the air pollution that may be reasonably anticipated to endanger public health or welfare …. The Findings discuss in detail the information that is relevant to the determination and how the Administrator has interpreted it in deciding whether the air pollution is reasonably anticipated to endanger public health or welfare. The scientific literature as synthesized in the TSD provides exactly the kind of information that can help inform these issues. For example, the TSD summarizes the conclusions of the assessment reports with respect to: 1) current emissions of GHG emissions; 2) how these emissions are changing the composition of the atmosphere; 3) how such changes in the atmosphere are affecting the global and regional climate; and 4) the potential impacts of such changes in climate on human health and welfare, for current and future generations. In its scope and quality, the assessment literature is relevant and appropriate for addressing the scientific issues under the CAA.[101]

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[97] As stated above, the USGCRP refers to the United States Global Change Research Program. In January 2009, the USGCRP was established as an integrating body for U.S. funded research efforts in the area of climate change. USGCRP subsumed the work of the U.S. Climate Change Science Program ("CCSP"), which had previously coordinated such research. As of January 16, 2009, the CCSP had produced 21 synthesis and assessment reports ("SAPs"), and these reports, along with the IPCC reports, became the principal basis for the June USGCRP report GLOBAL CLIMATE CHANGE IMPACTS IN THE UNITED STATES. At times, the Endangerment Finding and TSD refer to the Agency's reliance on both the USGCRP and the CCSP reports, but in actuality this represents essentially the same information. Accordingly, our discussion below generally refers to the relevant CCSP material and not the USGCRP.

[98] The main National Research Council report on climate change is the 2001 study CLIMATE CHANGE SCIENCE: AN ANALYSIS OF SOME KEY QUESTIONS. That study is obviously dated. A more recent 2006 NRC study, SURFACE TEMPERATURE RECONSTRUCTIONS FOR THE LAST 2,000 YEARS, is relevant to the issues discussed in our Petition for Reconsideration and is discussed below.

[99] Endangerment Finding, 74 Fed. Reg. at 66,510.

[100] TSD at 4.

[101] Resp. to Comm. Vol. 1 at 5.

Similarly, EPA stated that:

> EPA disagrees that review of the scientific and technical information contained in the TSD was inadequate. *EPA did not develop new science as part of this action and instead summarized the existing peer-reviewed assessment literature*.[102]

3. The Administrator's Reliance on the "Assessment Literature" to Satisfy Procedural Obligations

The Administrator's reliance on the "assessment literature" was so complete that she used that literature to satisfy a number of her procedural obligations in developing the Endangerment Finding.

a. Obligations as to Data Availability

The Administrator recognized that she is required to docket the information on which she relies.[103] She maintained, however, that since she is "reasonably relying on the major assessments of the USGCRP, IPCC, and NRC as the primary scientific and technical basis of her endangerment decision," she is not required to docket material that these reports themselves relied on.[104] She took the position that "[i]nformation regarding the underlying data, models, and studies used by the IPCC, USGCRP, CCSP, and NRC in developing their assessment reports can be accessed by consulting these reports."[105] Similarly, the Administrator stated that she "did not conduct new research or modeling in developing the TSD, and instead relied upon the findings of the "assessment literature," including data and modeling studies presented in those reports. The information mentioned by the Commenter can be accessed by consulting these assessment

[102] *Id.* at 7 (emphasis added).

[103] Resp. to Comm. Vol. 1 at 54.

[104] *Id.*

[105] *Id.*

reports and the underlying studies."[106] She went on to say that "[o]ur comprehensive referencing of the assessment literature ensures transparency regarding the source of the data used. …."[107]

b. Obligations as to Quality and Transparency

The Administrator also relied on the "assessment literature" to satisfy her obligations under the IQA as to the quality and transparency of information she relied on in the Endangerment Finding. She made clear, however, that she did not make her own expert determination as to the quality and transparency of the information summarized in the "assessment literature." Instead, she reviewed the procedures used by the entities that prepared the "assessment literature" to confirm that those entities, in her view, had adequately taken steps to ensure information quality and transparency. She stated that "[o]ur approach is consistent with these [EPA's IQA] guidelines because we thoroughly reviewed and evaluated the author selection, report preparation, expert review, public review, information quality, and approval procedures of IPCC, USGCRP/CCSP, and NRC to ensure the information adhered "to a basic standard of quality, including objectivity, utility and integrity."[108]

[106] *Id.* at 56 (quotations added).

[107] *Id.* (quotations added).

[108] *Id.* at 57.

c. Obligations as to Peer Review

A number of commenters questioned the independence of the personnel EPA selected to peer review the Endangerment Finding, pointing out that all of the peer reviewers were government scientists and many had worked directly on the "assessment literature" on which EPA relied.[109] The Administrator recognized that she was obligated to provide for independent peer review. She nevertheless maintained that her near complete reliance on the "assessment literature" meant that she was justified in selecting peer reviewers not on the basis of their independence from EPA or the "assessment literature" but on the basis of their familiarity with that literature. As she stated, "[g]iven our approach to the scientific literature … the purpose of the federal expert review was to ensure that the TSD accurately summarized the conclusions and associated uncertainties from the assessment reports."[110] In other words, it was not important to the Administrator that she receive an independent critique of her own Endangerment Finding; her concern was merely to ensure that she had accurately summarized the conclusions of that literature.

d. Comment Deadline

A number of commenters sought an extension of the 60-day comment period for the Endangerment Finding, complaining that 60 days was insufficient to review and comment on the complex climatological and policy issues raised in the proposed finding.[111] EPA denied the extension requests, however, on the ground that it had provided a 120-day comment period in the

[109] *See* comments responded to at Resp. to Comm. Vol. 1 at 7.

[110] *Id.* at 7.

[111] Endangerment Finding, 74 Fed. Reg. at 66,502-3.

Advance Notice of Proposed Rulemaking ("ANPR")[112] regarding potential GHG regulation and that commenters in any event had had an opportunity to comment directly on the "assessment literature" on which EPA relied when that literature was prepared. She said that:

> the major scientific assessments that the EPA relied upon in the TSD released with the ANPR had previously each gone through their own public review processes and have been publicly available for some time. In other words, EPA has provided ample time for review, particularly with regard to the technical support for the Findings.[113]

Thus, according to EPA, the ability of the public to comment on the "assessment literature" during the processes in which that literature was developed guided EPA's decision in determining how much time the public should be given to comment on the proposed Endangerment Finding.[114]

4. EPA Particularly Relied on the IPCC for Determining that 20th Century Warming Was Primarily Caused by Anthropogenic GHG Emissions

Most of the TSD examines observed and projected climate and the effect on public health and welfare. Only eight pages of the TSD, however, are devoted to the critical "attribution" issue: whether changes to the climate system that EPA says are occurring and will accelerate in

[112] Advance Notice of Proposed Rulemaking: Regulating Greenhouse Gas Emissions under the Clean Air Act (ANPR), 73 Fed. Reg. 44,353 (Jul. 30, 2008).

[113] Endangerment Finding, 74 Fed. Reg. at 66,503.

[114] EPA is incorrect in saying that the public's opportunity to comment during the 120-day ANPR comment period substantially satisfied its right to comment on the proposed Endangerment Finding. In the first place, the ANPR did not contain any proposed Endangerment Finding or indeed any meaningful discussion of conclusions that might be drawn from the TSD. Moreover, although the TSD in the ANPR was similar to the TSD in the proposed Endangerment Finding, there were important differences between the two. Additionally, a number of the CCSP assessment reports on which the ANPR TSD relied had not been through the public comment period for those reports and were not final at the time of the ANPR comment period. Thus, the 120-day comment period on the ANPR did not provide an opportunity for the public to comment on these reports *to EPA*. The only opportunity to comment on these reports to EPA was during the 60-day comment period on the proposed Endangerment Finding. Nevertheless, Peabody's Petition does not ask EPA to reconsider the Endangerment Finding based on the insufficient comment deadline. The Petition's point here is simply that EPA's use of the public comment periods in the processes that led to development of the "assessment literature" as justification for the short comment period here is further evidence of EPA's near total reliance on that literature.

the future can be attributed to anthropogenic GHG emissions and not natural forces.[115] The attribution section of the TSD particularly relies on the work of the IPCC, as opposed to the other "assessment literature" or any other studies. We count sixty-seven citations in this section, with forty-seven to the IPCC. All the graphics in this section are taken from the IPCC, as is the introduction. Plainly, the principal authority for EPA's central conclusion that anthropogenic GHG emissions are causing deleterious climate change is the IPCC.

B. The CRU Material Undermines the Administrator's Reliance on the "Assessment Literature"

As discussed in section IX.A.1 below, Peabody believes that the Administrator's near total reliance on the "assessment literature" contradicts her statutory obligation to exercise judgment. However, even assuming *arguendo* that the Administrator could have, in theory, relied on that literature to the extent that she did, the CRU material shows that her reliance here was misplaced and that she must now reconsider her finding.

1. The IPCC Reports on Which EPA Relied for Its Attribution Finding Are Not Subject to Peer Review as Typically Used in Scientific Journals

At the outset, it is important to recognize that the IPCC (and CCSP) reports are not prepared using the same-peer review process required for publication of an article in a scientific journal. The journal peer-review process typically requires the author of a draft article to respond to critical reviews to the journal editor. The journal editor serves as an impartial referee and decides whether the author must modify his or her draft in response to criticism. By contrast, IPCC contributing authors are the ones who decide whether to accept or reject critical reviews. Hence, there is no neutral scientist standing between the author and reviewer to ensure that reviews are judged dispassionately. Thus, when the Administrator states that the

[115] TSD at 47-54.

"assessment literature" was developed through "rigorous and transparent processes,"[116] she is not referring to the same rigor as practiced in the peer review journals.

Moreover, although authors of journal articles are asked to recommend peer-reviewers, they are discouraged from recommending close associates. No such prohibition exists in either the IPCC or CSSP processes. As a result, authors and reviewers of these reports in many cases are close associates and indeed have collaborated on papers.

Another typical practice in the preparation of these reports that may undermine objectivity is the citation by report authors of their own papers and those of report reviewers. Particularly in areas where there may be scientific debate in the literature, this practice may lead to the report providing a less than completely neutral summary of the science.

The CRU material reveals that the IPCC authors were aware that citing their own papers could be seen as using the IPCC process to advance their own views rather than to present a neutral overview of the science. According to Chapter 6 coordinating lead author Overpeck:

> Also, please not that, in the US, the US Congress is questioning whether it is ethical for IPCC authors to be using the IPCC to champion their own work/opinions. Obviously, this is wrong and scary, but if our goal is to get policy makers (liberal and conservative alike) to take our chapter seriously, it will only hurt our effort if we cite too many of our own papers (perception is often reality). PLEASE do not cite anything that is not absolutely needed, and please do not cite your papers unless they are absolutely needed. Common sense, but it isn't happening. Please be more critical with your citations so we save needed space, and also so we don't get perceived as self serving, or worse.
>
> Again, we can debate this if anyone thinks I've gone off the deep end.[117]

[116] Resp. to Comm. Vol. 1 at 6.

[117] CRU email 1120014836.txt (Jan. 28, 2005).

These concerns were brushed aside, however. As one commenter on EPA's Endangerment Finding showed as to the overlapping relationships of IPCC authors and reviewers and their citation to their own papers in the context of Chapter 9 of AR4, the chapter that addressed attribution:

- Forty of the fifty-three authors of this chapter co-authored papers with each other.
- More than half of the contributing authors co-authored papers with lead authors or coordinating lead authors.
- The review editor of the chapter contributed to thirteen papers cited in the chapter and had co-authored these papers with ten other authors of Chapter 9.
- About 40% of the papers cited in Chapter 9 were written by Chapter 9 authors.
- Of the published papers in Chapter 9:
  - Ø Ninety-four were authored by two or more of that chapter's author;
  - Ø One cited paper had six chapter authors;
  - Ø Five cited papers had five chapter authors;
  - Ø Twenty-six papers had three chapter authors, including six papers written entirely by chapter authors;
  - Ø Fifty of the cited papers listed two chapter authors each, and ten of these papers were written entirely by chapter authors.[118]

EPA's response to this commenter and to other similar comments was generic and did not substantively address the fact that the lack of formal scientific peer review means that the "assessment literature" may have been less than fully independent and rigorous. EPA's response to concerns about how the IPCC process worked in fact was to quote verbatim official IPCC policies and procedures on the development of the IPCC reports.[119] EPA, however, did not come

[118] EPA-HQ-OAR-2009-0171-3187.5. The commenter also provided a great deal of additional information which we will not summarize here rebutting the conception that all, or even most, of the authors and reviewers of the IPCC reports endorse those reports' central conclusions.

[119] Resp. to Comm. Vol. 1 at 19-23.

to grips with the fact that IPCC chapter authors routinely cited their own work and that of their co-authors, and that reviewers were frequently co-authors and colleagues of authors – meaning that there were built-in conflicts of interests that had the potential for undermining the reliability of the reports, a potential that, as we will see, unfortunately was borne out in the IPCC chapters on observed climate and paleoclimate.[120]

2. The CRU Material Demonstrates that EPA's Confidence in the Processes by Which the "Assessment Literature" Was Developed Was Misplaced

The CRU material shows that there were widespread abuses in the development of the portions of the IPCC reports that address the critical attribution issue. Having placed so much reliance on how the IPCC reports *should* have been prepared, EPA must now respond to the evidence of how the IPCC reports actually *were* prepared. This evidence is set forth in great detail below in the succeeding sections of this Petition.

EPA cannot respond to this evidence by maintaining that it applies only to certain portions of the IPCC reports. We do not maintain here that all IPCC authors were guilty of the same practices as we discuss below. We do maintain, however, that these abuses materially affected the conclusions reached in the portions of these reports that address the key question of whether anthropogenic GHG emissions are, in fact, as EPA concludes in reliance on this material, primarily responsible for climate change.

Moreover, EPA cannot dismiss these abuses as the product of a few isolated scientists at the CRU who were not in a position to influence the IPCC reports. The main scientists implicated in these emails were not just minor research scientists at CRU. Those implicated include both leading CRU scientists and senior scientists at leading American climate science

[120] The interconnection among authors, contributors, and editors is discussed in greater detail below in section VII.

institutions who were in close contact with the CRU scientists. They are the leaders in the fields of climate observations and paleoclimate, two of the key areas from which the IPCC (and therefore EPA) drew in determining that anthropogenic GHGs are affecting climate. These scientists include:

- Dr. Phil Jones, Director of the Climatic Research Unit and one of two coordinating lead authors of Chapter 3 of AR4.
- Dr. Kevin E. Trenberth, head of the Climate Analysis Section at the National Center for Atmospheric Research ("NCAR") and the other coordinating lead author of Chapter 3 of AR4.
- Dr. Keith R. Briffa of CRU, a lead author of Chapter 6 of AR4.
- Dr. Jonathan Overpeck of the University of Arizona, one of two coordinating lead authors of Chapter 6 of AR4.
- Dr. Michael Mann, a lead author of Chapter 2 of the 2001 IPCC Third Assessment Report ("TAR"), one of the leading figures in the field of paleoclimatology, and the scientist whose "hockey stick" graphic became the single most important item of information in the TAR.
- Dr. Benjamin D. Santer of the Lawrence Livermore National Laboratory and lead author of Chapter 8 of the 1995 IPCC report.
- Dr. Thomas Wigley, former head of the CRU, PhD advisor to Drs. Santer and Jones, senior scientist at the University Corporation for Atmospheric Research ("UCAR"), and a 1995 IPCC report lead author, 2001 IPCC report contributing author, and 2007 IPCC report contributing author.
- A number of other scientists who worked for and with these scientists.

An independent analysis confirmed that these scientists are closely networked with each other and dominate their field.[121] As discussed below, Mann's publication of his hockey stick paper generated a great deal of controversy concerning his statistical methods. As a result, the House Committee on Energy and Commerce asked Dr. Edward J. Wegman, one of the country's foremost statisticians, to assemble an independent team to review both Mann's work and

[121] Wegman Report, Social Network Analysis of Authorship in Temperature Reconstruction at 38-47.

critiques of it. Wegman worked without compensation and had no affiliation with any interested parties. His conclusions regarding Mann's work are discussed in section IV(C)(3) below, but he also concluded that the paleoclimate field is dominated by interlinked scientists who no longer produce research that has the desired degree of independence.

According to the Wegman's Testimony to the House subcommittee:

> We found that at least 43 authors have direct ties to Dr. Mann by virtue of coauthored papers with him. *Our findings from this analysis suggest that authors in the area of this relatively narrow field of paleoclimate studies are closely connected.* Dr. Mann has an unusually large reach in terms of influence and in particular Drs. Jones, Bradley, Hughes, Briffa, Rutherford and Osborn. . . Because of these close connections, independent studies may not be as independent as they might appear on the surface. Although we have no direct data on the functioning of peer review within the paleoclimate community, but with 35 years of experience with peer review in both journals as well as evaluation of research proposals, peer review *may not have been as independent as would generally be desirable*. . .Figure 8 is a graphic that depicts a number of papers in the paleoclimate reconstruction area together with some of the proxies used. We note that many of the proxies are shared. Using the same data also *suggests a lack of independence*.[122]

The Wegman Report also concluded that, despite the large amount of statistical analysis that undergirds paleoclimate research, this group of scientists did not engage independent statistical experts.[123] The Wegman Report concluded, moreover, that this group of scientists was no longer in a position to fairly judge their own work. Referring to the hockey stick analysis which underlies the 2001 TAR, the Wegman Report concluded, "*The MBH98/99 work has been*

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[122] Before Subcomm. on Oversight and Investigation of Comm. on Energy and Commerce, 109th Cong. 7-8 (2006) (testimony of Edward Wegman on the hockey stick) ("Wegman Testimony") (emphasis added) *available at* http://archives.energy commerce.house.gov/reparchives/108/Hearings/07192006hearing1987/Wegman.pdf.

[123] *Id.* at 7.

sufficiently politicized that this community can hardly reassess their public positions without losing credibility."[124]

The Wegman Report further found that, because this network both dominates the research and controls the IPCC on paleoclimatology, there is an inherent conflict of interest in the drafting of the IPCC reports. Wegman's first recommendation was to assure that the IPCC is truly independent:

> Recommendation 1. Especially when massive amounts of public monies and human lives are at stake, academic work should have a more intense level of scrutiny and review. It is especially the case that authors of policy-related documents like the IPCC report, C*limate Change 2001: The Scientific Basis*, should not be the same people as those that constructed the academic papers.[125]

These recommendations, however, were completely ignored in the drafting of AR4. As was shown above, IPCC lead authors routinely cited papers that they authored themselves or co-authored with other IPCC authors and reviewers.

In sum, the CRU abuses were not isolated to a few unimportant scientists with little influence. In fact, these scientists were highly influential in the development of the IPCC reports and much of the literature on which the IPCC (and therefore EPA) relied for its conclusion that the current climate conditions are the result of anthropogenic GHG emissions. Moreover, the investigations that have followed the release of the CRU information by the U.K. Commissioner's Information Office that led to the finding that U.K. freedom of information laws were violated,[126] and now by the U.K. Parliament's Committee on Science and Technology,[127]

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[124] *Id.* at 8 (emphasis added).

[125] Wegman Report at 51.

[126] *See Climate row unit 'broke data law,'* Jan. 28, 2010 *available at* http://news.bbc.co.uk/2/hi/uk_news/8484385.stm.

[127] http://www.parliament.uk/parliamentary_committees/science_technology/s_t_pn14_100122.cfm.

confirm the seriousness of this matter. Thus, even if *in theory* EPA was justified in relying so heavily on the "assessment literature," that reliance is now called into question by the CRU revelations.

IV.

THE CRU MATERIAL UNDERMINES CONFIDENCE IN THE KEY IPCC CONCLUSION THAT CURRENT CLIMATE CONDITIONS ARE LIKELY UNPRECEDENTED AND OUTSIDE THE RANGE OF NATURAL VARIABILITY

"I know there is pressure to present a nice tidy story as regards 'apparent unprecedented warming in a thousand years or more in the proxy data' but in reality the situation is not quite so simple."

Dr. Keith Briffa

A. The Importance of Placing Today's Temperatures in Context

One of the key questions in climate science is whether current temperatures are so unprecedented or unusual in the context of the paleoclimate as to lead to the conclusion that they must be caused by anthropogenic GHG emissions. It is known that temperatures have varied greatly over Earth's history, including during the current interglacial period known as the Holocene. If temperatures have been as warm or warmer than today during the Holocene, when the Earth's climate system has been similar to today's system but before GHGs increased in the atmosphere because of human emissions, it becomes more difficult to say that human GHG emissions are causing global warming.

Placing the current temperatures in historical context, particularly in the context of the last 1000-2000 years where more is known about climate than earlier in the Holocene, has always been considered to be a critical issue in assessing the extent to which anthropogenic GHGs may be affecting the climate. For instance, § 2.3 of the TAR is entitled, "Is the Recent Warming Unusual?" Section 2.3.1 "Background" of the TAR stated that, "To determine whether 20th century warming is unusual, it is essential to place it in the context of longer-term climate variability." Similarly, § 2.3.2 of the TAR stated that, "The past 1,000 years are a particularly important time-frame for assessing the background natural variability of the climate for climate

change detection." Emphasizing the importance of placing the current warming in context, AR4 contained a separate chapter, Chapter 6, devoted exclusively to the paleoclimate.

EPA itself recognizes the importance of the paleoclimate in attributing climate change to anthropogenic GHG emissions. The TSD stated that one of three lines of evidence supporting attribution is evidence that "arises from indirect, historical estimates of past climate changes that suggest that the changes in global surface temperature over the last several decades are unusual (Karl et al, 2009)."[128] EPA's citation to Karl et al, 2009 is to the USGCRP report *Global Climate Change Impacts in the United States.* That report stated that "[t]he second line of evidence [for human attribution] is from indirect estimates of climate changes over the last 1,000 to 2,000 years."[129]

B. The IPCC's Evolving Position on the Climate of the Last 1000 Years

When the IPCC published the FAR in 1990, it acknowledged the existence of a long MWP with temperatures exceeding those at the time of the report for several hundred years. According to the IPCC, the existence of the MWP was "notable in that there is no evidence that it was accompanied by an increase of greenhouse gases."[130] The IPCC stated that the MWP was then followed by the LIA, and that period was followed by more warming beginning around 1850, with the MWP being the warmest period of the three phases.[131] Below is figure 7.1(c) from Chapter 7 of the FAR, "*Observed Climate Variation and Change,"* representing global temperature variations over the last 1000 years.*[132]*

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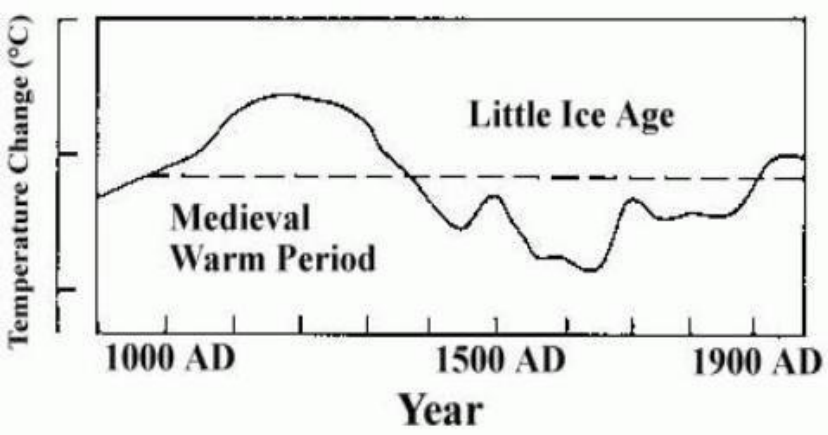
[128] TSD at 47.

[129] U.S. Global Change Research Program. GLOBAL CLIMATE CHANGE IMPACTS IN THE UNITED STATES 19 (Cambridge University Press 2009).

[130] FAR at 202.

[131] *Id.*

[132] *Id.*



Given this temperature cycle, the IPCC posited that the warming since 1850 "could be a recovery from the Little Ice Age rather than a direct result of human activities."[133]

However, in 2001 the IPCC released the TAR, and that document essentially eliminated the MWP and the LIA as global phenomena. The IPCC instead concluded that the temperatures in the latter part of the 20th century were likely unprecedented in the last millennium.[134] The evidence that the IPCC relied on for this new declaration was principally proxy data reconstructions of global temperatures, primarily tree ring (dendochronology) reconstructions.[135] The tree rings are thought to be indicative of temperatures in a region as the trees grew over time.

The most influential proxy temperature reconstruction that the TAR relied on was a reconstruction of temperatures in the Northern Hemisphere back to the year 1000AD by Michael Mann, Raymond Bradley, and Malcolm Hughes ("Mann et al" or "MBH") known as MBH99.[136]

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[133] *Id.* at 203.

[134] TAR at § 2.3.3.

[135] *Id.* at § 2.3.2.1.

[136] Michael Mann et al., *Northern Hemisphere Temperatures During the Past Millennium: Inferences, Uncertainties, and Limitations,* 26 GEOPHYS. RES. LETT. 759 (1999).

Building on the previous research reported in MBH98,[137] MBH99 showed temperatures from 1000AD slightly decreasing until about 1900 when the temperatures dramatically increased.[138] MBH99 presented the 1990s as the warmest decade in 1000 years and 1998 at the warmest year of the millennium.[139] The "hockey stick" image of temperature trends seen below was featured as Figure 1 on page three of the IPCC Summary for Policymakers of the Working Group 1 science report and were used widely by the IPCC and various advocates to show the urgent need for global warming regulation.

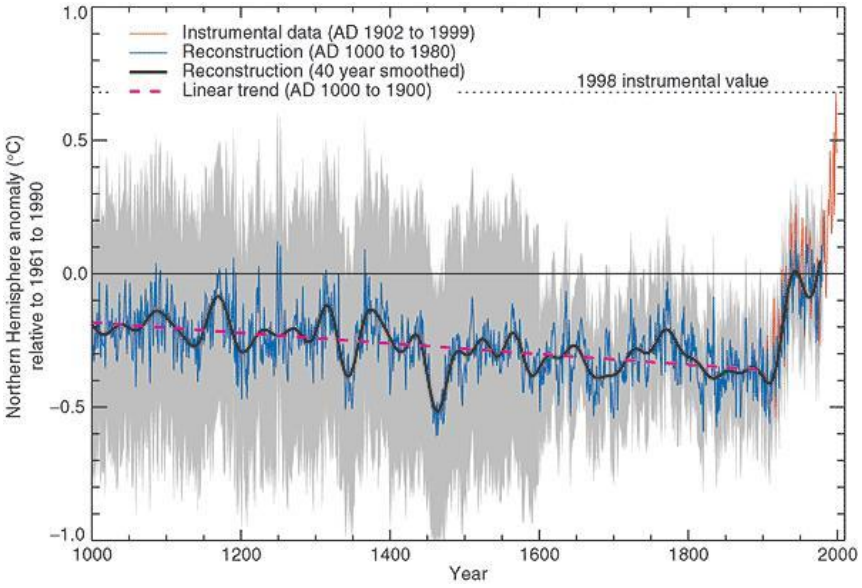

Figure 2.20: Millennial Northern Hemisphere (NH) temperature reconstruction (blue) and instrumental data (red) from AD 1000 to 1999, adapted from Mann et al. (1999). Smoother version of NH series (black), linear trend from AD 1000 to 1850 (purple-dashed) and two standard error limits (grey shaded) are shown.[140]

Based on the hockey stick, the IPCC now asserted that the "Medieval Warm Period appears to have been less distinct, more moderate in amplitude, and somewhat different in timing

[137] Michael Mann et al., *Global Scale Temperature Patterns and Climate Forcing Over the Past Six Centuries,* 392 NATURE 779 (1998).

[138] MBH98 at 31.

[139] MBH99 at 762.

[140] TAR at § 2.3.2.1.

at the hemispheric scale than is typically inferred."[141] According to the TAR, the terms "Little Ice Age" and "Medieval Warm Period" were no longer useful in describing hemispheric or global temperature means in the past centuries because the phrases were not supported by evidence. The TAR stated that any warming during the MWP was likely limited to Europe and North America and was not global in nature. The TAR concluded therefore that "20th century warming is likely to have been the largest of the millennium, with the 1990s and 1998 likely to have been the warmest decade and year, respectively, in the Northern Hemisphere."[142] According to the IPCC, the new long-term trend in hemispheric temperatures appears to be one of "a modest and irregular cooling from AD 1000 to around 1850 to 1900, followed by an abrupt 20th century warming."[143]

Owing to the substantial controversy that soon arose as to the scientific validity of the hockey stick, as discussed below, the IPCC's 2007 AR4 report relied to a much lesser extent on Mann's hockey stick study.[144] However, AR4 retained the overall conclusion that average Northern Hemisphere temperatures of the last fifty years were likely the warmest of the millennium, and that the MWP was neither as warm as the 20th century nor a global phenomenon. According to AR4, "… it is likely that the 20th century was the warmest in at least the past 1.3 kyr."[145] These conclusions relied on proxy temperature reconstructions performed primarily by the networked group of scientists identified in the Wegman Report and who are directly implicated in the CRU material. These reconstructions include a 2006 study by Osborn

[141] *Id.* at § 2.3.3.

[142] *Id.* at § 2.3.5.

[143] *Id.* at § 2.3.3.

[144] AR4 at § 6.6.1.1.

[145] *Id.*

and Briffa,[146] both of CRU, a 2003 study by Mann and Jones,[147] perhaps the scientists most implicated by the CRU materials, and a 2007 study by Caspar Ammann of NCAR and Eugene Wahl of the National Oceanic and Atmospheric Administration ("NOAA") in 2007,[148] a study that required the manipulation of IPCC publication deadlines to get included in AR4 (see discussion section VII(D).

C. The Disregard of Uncertainties and Conflicting Information as Part of an Attempt to Create a "Nice, Tidy" Version of the 20th Century as the Warmest in 1000 Years

The CRU material shows repeated abuses of the scientific process in the preparation of both the TAR and AR4 in order to build what the offending scientists in their own words described as a "nice, tidy"[149] case that the warming of the 20th century was unprecedented in the last 1000 years. This effort began around the time the hockey stick study was produced, which corresponded with the preparation of the TAR. It continued in the period between publication of the TAR and publication of AR4 and it continued at least through release of the CRU material last fall. As a result of these efforts, the discussion in both the TAR and AR4 of the paleoclimate over the last millennium – and particularly the reliance on proxy temperature reconstructions to essentially eliminate the MWP and LIA as important global phenomena – is incomplete and misleading. In particular, as we show below, these scientists drew more definitive conclusions from the proxy temperature reconstructions than were warranted given the inherent uncertainties

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[146] Tim Osborn and Keith Briffa, *The Spatial Extent of 20th Century Warmth in the Context of the Past 1200 Years,* 311 SCI. 811 (2006).

[147] Michael Mann and Phil Jones, *Global Surface Temperatures over the Past Two Millennia,* 30 GEOPHYS. RES. LETT. CLM. 5-1 (2003).

[148] Eugene Wahl and Caspar Ammann, *Robustness of the Mann, Bradley, Hughes reconstruction of Northern Hemisphere surface temperatures: Examination of criticisms based on the nature and processing of proxy climate evidence,* 85 CLIM. CHNG. 33 (2007).

[149] *See e.g.,* CRU email 1092167224.txt (Aug. 10, 2004).

in trying to reliably determine temperature information from proxies over a thousand years, uncertainties that these scientists were aware of but chose largely to discount.

1. Supposedly neutral scientists acting as advocates

a. TAR

The emails reflecting preparation of both the TAR and AR4 show scientists being pressured by the lead authors to advance a pre-conceived agenda of unprecedented 20th century warmth rather than presenting a neutral summary of the science. For instance, on September 22, 1999, Mann wrote to Briffa, Folland, and Jones in the course of a wide-ranging discussion of a number of issues related to the TAR. In particular, he addressed whether a Briffa tree ring proxy should be included because it differed from other proxies and therefore did not support the "consensus" that they advance in support of the hockey stick. He stated:

> So if Chris and Tom (?) are ok with this, I would be happy to add Keith's series. That having been said, it does raise a conundrum: We demonstrate (through comparing an extratropical averaging of our northern hemisphere patterns with Phil's more extratropical series) that the major discrepancies between Phil's and our series can be explained in terms of spatial sampling/latitudinal emphasis (seasonality seems to be secondary here, but probably explains much of the residual differences). But that explanation certainly can't rectify why Keith's series, which has similar seasonality *and* latitudinal emphasis to Phil's series, differs in large part in exactly the opposite direction that Phil's does from ours. ***This is the problem we all picked up on (everyone in the room at IPCC was in agreement that this was a problem and a potential distraction/detraction from the reasonably concensus viewpoint we'd like to show w/ the Jones et al and Mann et al series.***[150]

In the same email to Mann, Folland and Jones, Briffa referred to the desire to present a "nice tidy story" about 20th century warmth, but expressed discomfort with whether there was scientific support for that viewpoint:

[150] CRU email 938018124.txt (Sept. 22, 1999) (emphasis added).

> ***I know there is pressure to present a nice tidy story as regards 'apparent unprecedented warming in a thousand years or more in the proxy data' but in reality the situation is not quite so simple.*** We don't have a lot of proxies that come right up to date and those that do (at least a significant number of tree proxies) some unexpected changes in response that do not match the recent warming. I do not think it wise that this issue be ignored in the chapter.
>
> *For the record, I do believe that the proxy data do show unusually warm conditions in recent decades. I am not sure that this unusual warming is so clear in the summer responsive data.* ***I believe that the recent warmth was probably matched about 1000 years ago****. I do not believe that global mean annual temperatures have simply cooled progressively over thousands of years as Mike appears to and I contend that that there is strong evidence for major changes in climate over the Holocene (not Milankovich) that require explanation and that could represent part of the current or future background variability of our climate.*[151]

Briffa was not alone in harboring doubts about the asserted unprecedented nature of 20th century warmth. On July 10, 2000, Bradley, one of Mann's co-authors in the hockey stick paper, also expressed concerns about presentation of a scenario that excluded the MWP. He wrote to Oldfield, Hughes, Mann, Briffa and others:

> Furthermore, it may be that Mann et al simply don't have the long-term trend right, due to underestimation of low frequency info. in the (very few) proxies that we used. We tried to demonstrate that this was not a problem of the tree ring data we used by re-running the reconstruction with & without tree rings, and indeed the two efforts were very similar -- but we could only do this back to about 1700. *Whether we have the 1000 year trend right is far less certain (& one reason why I hedge my bets on whether there were any periods in Medieval times that might have been "warm", to the irritation of my co-authors!).* So, possibly if you crank up the trend over 1000 years, you find that the envelope of uncertainty is comparable with at least some of the future scenarios, *which of course begs the question as to what the likely forcing was 1000 years ago*.[152]

[151] *Id.* (emphasis added).

[152] CRU email 963233839.txt (Jul. 10, 2000) (emphasis added).

Similarly, on May 2, 2001 in an email thread involving Mann, Jones, Briffa, and Crowley, Cook provided a detailed explanation of his work, at times bristling at Mann's interference:

> *So, at this stage I would argue that the Medieval Warm Period was probably a global extra-tropical event, at the very least, with warmth that was persistent and probably comparable to much of what we have experienced in the 20th century.* However, I would not claim (and nor would Jan) that it exceeded the warmth of the late 20th century. We simply do not have the precision or the proxy replication to say that yet. ***This being said, I do find the dismissal of the Medieval Warm Period as a meaningful global event to be grossly premature and probably wrong.***[153]

These concerns, however, were brushed aside in the final TAR. As seen, the TAR's version of the temperature record of the last 1000 years was the hockey stick, and the TAR discussion of paleoclimatology expressed confidence that temperatures in the last part of the 20th century were unprecedented in the last millennium.

b. Between the TAR and AR4

The period in between publication of the TAR and AR4 saw Mann and his colleagues actively engaged in trying to manage the peer-reviewed literature to support their vision of paleoclimate. Their efforts in this regard are discussed in more detail in section VIII below, but several emails from this period are particularly relevant in showing that those involved in drafting Chapters 3 and 6 of AR4 had an agenda-driven view of the science.

First, an email from December 17, 2001 provides an example of Briffa encouraging Cook to toe the line. Briffa was acting as referee on a paper submitted to Science by Esper and Cook. He wrote:

[153] CRU email 0988831541.txt (May 2, 2001) (emphasis added).

> *I simply would not like to see you write a paper that puts out a confused message with regard to the global warming debate, leaving ambiguity as to your opinion on the validity of the Mann curve...* I am totally confident that after a day's rephrasing this paper can go back and be publishable to my satisfaction by Science.[154]

In another example, on March 22, 2002 Mann sternly admonished Briffa and Osborn for publishing a paper in *Science* that was critical of Mann's work. He also criticized Cook for comments made to the Associated Press:

> Sadly, your piece on the Esper et al paper is more flawed than even the paper itself. Ed, the AP release that appeared in the papers was even worse. Apparently you allowed yourself to be quoted saying things that are inconsistent with what you told me you had said. *You three all should have known better.* Keith and Tim: Arguing you can scale the relationship between full Northern Hemisphere and extratropical Northern Hemisphere is *much* more problematic than even any of the seasonal issues you discuss, and this isn't even touched on in your piece. The evidence of course continues to mount (e.g., Hendy et al, Science, a couple weeks ago) that the tropical SST in the past centuries varied far more less in past centuries. Hendy et al specifically point out that there is little evidence of an LIA in the tropics in the data. The internal inconsistency here is remarkably ironic. The tropics play a very important part in our reconstruction, with half of the surface temperature estimate coming from latitudes below 30N. You know this, and in my opinion *you have knowingly misrepresented our work in your piece. This will be all be straightened out in due course. In the meantime, there is a lot of damage control that needs to be done and, in my opinion, you've done a disservice to the honest discussions we had all had in the past, because you've misrepresented the evidence.* Many of us are very concerned with how Science dropped the ball as far as the review process on this paper was concerned. This never should have been published in Science, for the reason's I outlined before (and have attached for those of you who haven't seen them). I have to wonder why the functioning of the review process broke down so overtly here.[155]

[154] CRU email 1008619994.txt (Dec. 17, 2001) (emphasis added).

[155] CRU email 1018045075.txt (Apr. 5, 2002) (emphasis added).

Mann's scathing rebuke is disturbing on a number of levels: ***First,*** it is obvious that Mann has a deeply personal attachment to his own work, which calls into question his ability to serve as an impartial researcher, author, contributor and editor. ***Second,*** the magnitude of Mann's alarm is troubling. He accused colleagues, some of whom are IPCC lead authors, of having "knowingly misrepresented our work" which calls into question his colleagues' integrity. ***Third,*** Mann's main interest was obviously preserving the consensus view of paleoclimate that was presented in his hockey stick study and the TAR. He was unwilling to examine new information that challenged that carefully constructed consensus.

The period between the publication of the TAR and AR4 also showed the leading paleoclimatologists expressing doubts about the supposedly unprecedented nature of 20th century warming. For example, in early April of 2002, the scientists engaged in a wide-ranging discussion of data reliability and related issues. In a candid email, Hughes wrote to Cook, Mann and others:

> All of our attempts, so far, to estimate hemisphere-scale temperatures for the period around 1000 years ago are based on far fewer data than any of us would like. None of the datasets used so far has anything like the geographical distribution that experience with recent centuries indicates we need, and no-one has yet found a convincing way of validating the lower-frequency components of them against independent data.[156]

Similarly, in an email dated September 3, 2003, Cook wrote to Briffa and proposed a paper that he wanted to develop, evaluating a number of proxy based studies against one another:

> Without trying to prejudice this work, but also because of what I almost think I know to be the case, the results of this study will show that we can probably say a fair bit about <100 year extra-

[156] CRU email 1018647333.txt (Apr. 12, 2002).

> tropical NH temperature variability (at least as far as we believe the proxy estimates), but honestly know fuck-all about what the >100 year variability was like with any certainty (i.e. we know with certainty that we know fuck-all).
>
> Of course, none of what I have proposed has addressed the issue of seasonality of response. So what I am suggesting is strictly an empirical comparison of published 1000 year NH reconstructions because many of the same tree-ring proxies get used in both seasonal and annual recons anyway. So all I care about is how the recons differ and where they differ most in frequency and time ***without any direct consideration of their TRUE association with observed temperatures.***[157]

Cook's profanity shows his frustration with the fact that the proxy reconstructions in which so much effort had been devoted may not prove anything regarding true long-term temperature trends. Indeed, as he stated, his study would only compare various proxy temperature reconstructions, but such study will prove nothing about whether any of the reconstructions reflect, in his word, a "TRUE" association with actual temperatures.

Moreover, in the same email, he questioned whether Mann's and Jones' bias made them unable to be objective:

> I am afraid the Mike and Phil are too personally invested in things now (i.e. the 2003 GRL paper that is probably the worst paper Phil has ever been involved in - Bradley hates it as well), but I am willing to offer to include them if they can contribute without just defending their past work - this is the key to having anyone involved. Be honest. Lay it all out on the table and don't start by assuming that ANY reconstruction is better than any other.[158]

The Mann and Jones paper nevertheless was one of the key papers cited in the AR4 discussion of proxy temperature reconstructions and was also cited by EPA in the Endangerment

[157] CRU email 1062592331.txt (Sep. 3, 2003) (emphasis added).

[158] *Id.* (capitals in original).

Finding.[159]

On June 24, 2005, Wigley wrote to Mann in the context of a letter that Mann received from the Subcommittee on Oversight and Investigations of the House Committee on Energy and Commerce concerning the hearings that ultimately led to the Wegman Report. The subcommittee sought information from Mann about his hockey stick analysis in light of the McIntyre and McKitrick critiques. Wigley offered advice to Mann about his response in which Wigley indicated his own discomfort with the "wide" divergence in proxy temperature reconstructions:

> A word of warning. I would be careful about using other, independent paleo reconstruction work as supporting the MBH reconstructions. I am attaching my version of a comparison of the bulk of these other reconstructions. Although these all show the hockey stick shape, the differences between them prior to 1850 make me very nervous. *If I were on the greenhouse deniers' side, I would be inclined to focus on the wide range of paleo results and the differences between them as an argument for dismissing them all.*[160]

c. AR4

By the time the drafting of AR4 was underway, it was clear that the paleoclimate chapter would be more of a political than a scientific document. The two coordinating lead authors of Chapter 6 of AR4, Jonathan Overpeck of the University of Arizona and Eystein Jansen of the University of Bergen in Norway, coached contributors to produce materials that would serve a public policy agenda. The CRU emails show that Overpeck instructed his colleagues to include science that would be "policy relevant" and that would support pre-conceived summary material. The pair also advised authors to include graphics that would be "compelling" and that could be

[159] Resp. to Comm. Vol. 2 at 43.

[160] CRU email 1119957715.txt (Jun. 24, 2005) (capitals in original, emphasis added).

used in the policymaker's summary. For example, in an email dated July 14, 2005, Overpeck outlined his objectives to his contributors in a series of numbered paragraphs:

> 4) With respect to text, try hard to get it down to size (see below), and to ensure that it is FOCUSED on ***only that science which is policy relevant. ALL TEXT should support an Exec Summary Bullet.*** If it doesn't the text should be removed, or a bullet created for discussion with our team. . .
> 5) With respect to the figures (and table***), make sure each one is as compelling as possible***. To save space (see below) you might decide a figure has to go. You might decide a new figure has to be included (only if there is space!). Work to get the figure redrafted where needed to be perfect - a sign of ultimate success will be that our figs get into the TS/SPM docs. Peck will be on that team and will push hard, but figures ***MUST BE POLICY RELEVANT AND COMPELLING***.[161]

Science, however, is not supposed to be developed on the basis of whether or not it is policy relevant – it is supposed to be honest, accurate and neutral – and scientific discussion should not be circumscribed for the purpose of supporting a simple bullet point.

Other emails show that Overpeck was particularly concerned with establishing that temperatures during the late 20th century were unprecedented compared to other periods during the past 1000 to 2000 years, most notably the MWP – to the point where scientific analysis morphed into advocacy. On January 13, 2005, Overpeck wrote to Briffa and Osborn about various subsections of Chapter 6, encouraging them to dismiss the significance of the MWP and the Holocene Thermal Maximum:

> In reading Valerie's Holocene section, I get the sense that I'm not the only one who would like to deal a mortal blow to the misuse of supposed warm period terms and myths in the literature. The sceptics and uninformed love to cite these periods as natural analogs for current warming too - pure rubbish.
> So, pls DO try hard to follow up on my advice provided in previous email. No need to go into details on any but the MWP,

[161] CRU email 1121392136.txt (Jul. 14,2005) (emphasis added, all capitals in original).

> but good to mention the others in the same dismissive effort. "Holocene Thermal Maximum" is another one that should only be used with care, and with the explicit knowledge that it was a time-transgressive event totally unlike the recent global warming.[162]

A few days later, on January 17, 2005, Overpeck returned to this point, expressing satisfaction with a graphic that would describe the MWP as heterogeneous—meaning that warming was not uniform on a planetary scale. His congratulations were not based on the fact that the graphic was accurate but that it hammered home the advocacy point he wished to make. The final version of the "MWP box" appeared on pages 468 and 469 of AR4:

> Hi all - attached is Keith's MWP box w/ my edits. ***It reads just great - much like a big hammer***. Nice job.[163]

On May 23, 2005, Overpeck emailed Briffa, pushing him to develop something even more compelling than the hockey stick:

> We're hoping you guys can generate something compelling enough for the TS and SPM - ***something that will replace the hockey-stick with something even more compelling***.[164]

The CRU emails show that other lead authors, in addition to Overpeck, viewed their work as advocacy, rather than scientific analysis, which could be marshaled against opponents. After seeing an earlier draft of the MWP sidebar, Mann wrote an email to Jones on February 2, 2005:

> I saw the paleo draft (actually I saw an early version, and sent Keith some minor comments). It looks very good at present--will be interesting to see how they deal w/ the contrarian criticisms--there will be many. I'm hoping they'll stand firm (I believe they will--I think the chapter has the right sort of personalities for that)...[165]

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[162] CRU email 1105670738.txt (Jan. 13, 2005).

[163] CRU email 1105978592.txt (Jan. 17, 2005).

[164] CRU email 1116902771.txt (May 23, 2005) (emphasis added).

[165] CRU email 1107454306.txt (Feb. 2, 2005) (emphasis added).

The degree to which the process of writing Chapter 6 became politicized is evident from the following emails in which Susan Solomon, co-chair of Group 1 of the IPCC, asked the authors to address the fact that there was a period around 1450 when temperatures globally were as warm as today. Despite the fact that the 15th century is not generally considered to be within the MWP, the authors talked about how they could redefine the MWP to include the 15th century, presumably so that they could "dismiss" the c. 1450 warming in the same way that they had "dismissed" the MWP:

Overpeck wrote a number of authors:

> Hope you're not going to kill me, but I was talking with Susan Solomon today, and she impressed me with the need to make several points if we can.
> One issue … is whether we can extend the MWP box figure to include the 15th century. I don't read the blogs that regularly, but I guess the skeptics are making hay of their being a global warm event around 1450AD. I agree w/ Susan that it is our obligation to weigh in on issues like this, so… can we extend the fig to extend up to 1500AD?[166]

Briffa and Osborn responded:

> there is a period around 1400 when the proxy records we've used in this MWP figure do indicate a warm period - and all records show positive anomalies at the same time. Thus it couldn't/shouldn't be dismissed in the same way as the MWP…[167]

To which Overpeck replied:

> this means that the MWP box needs to talk about the period around 1400 - can you make sure that's on Keith's radar screen. I believe that historians talk about the Medieval Period going to at least 1450, so what the heck…[168]

[166] CRU email 1123268256.txt (Aug. 5, 2005).

[167] CRU email 1123513957.txt (Aug. 8, 2005).

[168] *Id.*

Again, the emphasis here is not on producing neutral science. The emphasis is presenting a strong case.

d. Subsequent Rejection of AR4 Conclusion that MWP Was Heterogeneous

Interestingly, despite Overpeck's satisfaction with the MWP box in the AR4 and its "hammer[ing]" home the desired point that the warming was heterogeneous (i.e., regional) rather than global, research published since the AR4 has not upheld that finding. Paleoclimate researchers Jan Esper and David Frank recently published a paper which specifically looked at the IPCC's heterogeneity findings and concluded that AR4 Chapter 6 overreached the available evidence:

> In their 2007 report, IPCC working group 1 refers to an increased heterogeneity of climate during medieval times about 1000 years ago. This conclusion would be of relevance, as it implies a contrast in the spatial signature and forcing of current warmth to that during the Medieval Warm Period. Our analysis of the data displayed in the IPCC report, however, shows no indication of an increased spread between long-term proxy records. ***We emphasize the relevance of sample replication issues, and argue that an estimation of long-term spatial homogeneity changes is premature based on the smattering of data currently available***.[169]

The fact that Esper and Frank introduced no new data, but instead simply perform a more thorough analysis of the data used by the IPCC (and arrive at a different, more cautious conclusion), suggests that Overpeck was willing to accept results for the IPCC AR4 which agreed with his stated desire to dismiss the MWP, without a proper critical evaluation. Indeed, the fact that Chapter 6's "big hammer" has been called into question so quickly after AR4 was published demonstrates the agenda-driven nature of that chapter.

[169] Jan Esper, and David Frank, *The IPCC on a heterogeneous Medieval Warm Period,* 267 (2009).

2. The Divergence Problem and Tricks to Hide the Decline

The paleoclimate community also actively took steps to hide weaknesses in their data. One of the most controversial emails to emerge from the CRU database is dated November 16, 1999 in which Jones stated to Bradley and Hughes that he had completed "Mike's Nature trick" to "hide the decline."[170] After the CRU material was released, Jones asserted that the word "trick" was being misinterpreted and that he was referring not to subterfuge but instead to a clever way of solving a problem. A close inspection of the "trick" in question leads more toward the former interpretation than the latter. Indeed, both Mann and Jones appear to have been actively engaged in trying to hide information that contradicted their view of unprecedented 20th century global warming.

a. The divergence problem

The "trick" that Mann and Jones used was necessary to mask what is known as the "divergence problem."[171] Over the last fifty years of the 20th century many tree ring records fail to correspond with climate warming observed in the instrument record. Over this fifty year period, and particularly in the last twenty years, tree rings suggest cooling temperatures, while the instrument record shows increasing temperatures. This "divergence" casts doubt upon the validity of tree ring series as proxies for past climates in that it has the potential to reduce the mean and range of reconstructed values compared to what they actually were. As we will show, many recent (post-AR4) paleoclimate studies confirm that the divergence problem makes comparing the temperatures during the MWP with recent observed temperatures virtually impossible. Yet despite the fact that the divergence problem was recognized even before the

[170] CRU email 942777075.txt (Nov. 16, 1999).

[171] *See, e.g.* Craig Loehle, *A Mathematical Analysis of the Divergence Problem in Dendroclimatology*, 233 (2008).

TAR in 2001, the authors of the sections of the TAR and AR4 on paleoclimate suppressed or discounted those concerns in order to support the "nice tidy" version of unprecedented 20th century warming that they wished to present.

b. Hiding the decline

In order to "hide the decline" in proxy temperatures appearing in the tree ring record – in other words to conceal the divergence problem – both Mann and Jones inserted actual temperature data at the end of their proxy reconstructions. By doing so, they were able to create an up-turned curve at the end of a long, proxy-based temperature record. Visually, this diagram misleads a viewer to believe that proxy records are (1) internally consistent, (2) generally consistent with the instrument record for the period when that record exists, and (3) supportive of unprecedented 20th century warming.

i. Jones uses the "trick"

For ease of reference, Jones' now-famous email reads as follows:

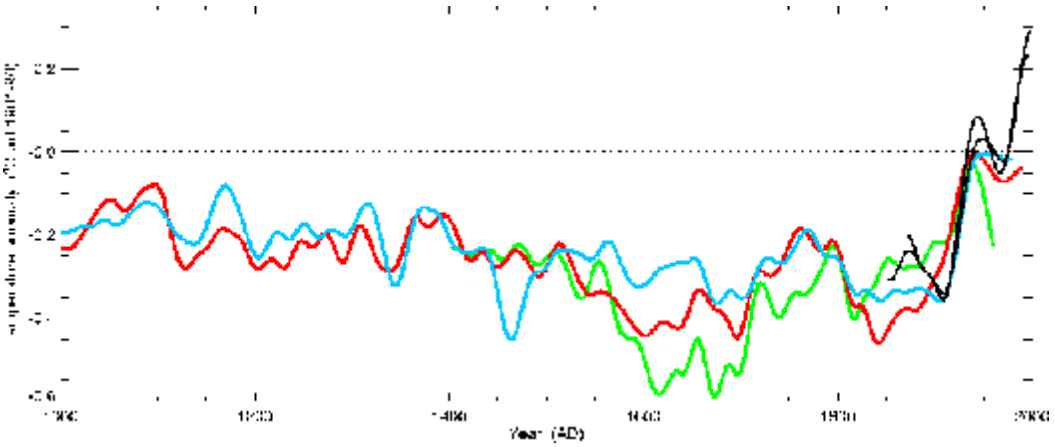
> I've just completed Mike's Nature trick of adding in the real temps to each series for the last 20 years (ie from 1981 onwards) amd from 1961 for Keith's to hide the decline.[172]

At the time that Jones wrote this email, he was apparently working on a chart for a forthcoming World Meteorological Organization report, "WMO Statement on the Status of the Global Climate in 1999."[173] The graph used one of his own reconstructions, as well as one each from Mann and Briffa. Through his email, Jones informed his colleagues that he had used the trick described above on each series. For Briffa's series, Jones grafted the instrument record beginning in 1961. On Mann's series, he spliced the instrument record beginning in 1981. The

[172] CRU email 942777075.txt (Nov. 16, 1999).

[173] WMO-No. 913, World Meteorological Organization (2000), *available at* http://www.wmo.ch/pages/prog/wcp/wcdmp/statemnt/wmo913.pdf.

earlier cut-off of Briffa's data was apparently necessary because the series trended downward – diverged – around 1960, further frustrating correlation with the instrument record.

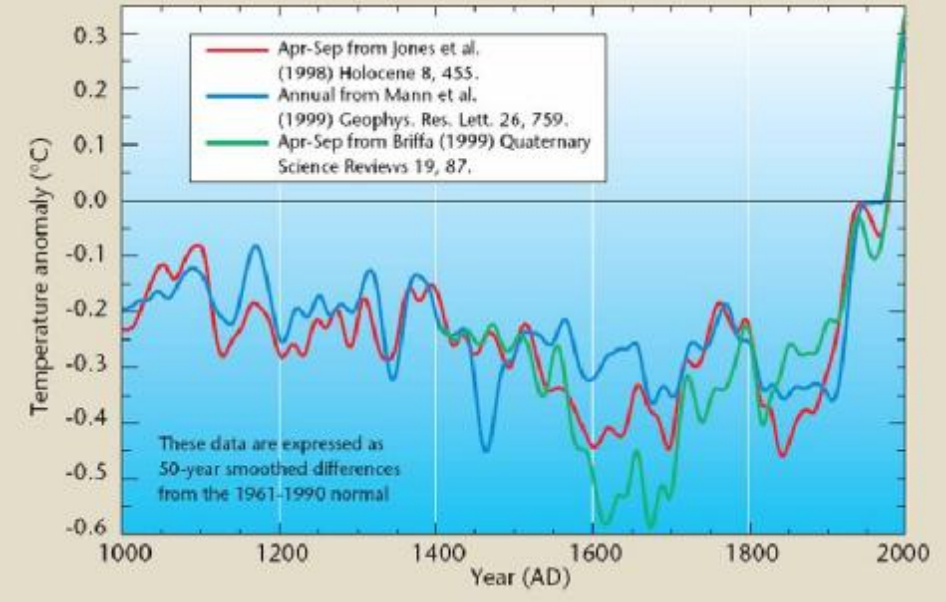


The pre-"trick" graph[174] is set forth above, showing all three proxies and the instrument record. Breaking from the instrument record, Briffa's series (green) trends sharply downward in the mid-20th century. The Jones series (red) also drops, but then starts to rise. The Mann series (blue) peaks and then basically flattens. None corresponds with recent 20th century temperatures captured by the direct measurements (black).

A markedly different graph was published in the World Meteorological Organization report.[175] Each proxy series has been manipulated to harmonize with the instrument record, but the instrument record itself is omitted. Moreover, the proxy records are made to agree with one another, which they otherwise would not, and to show a steep rise in the last twenty years, which again they would not.

[174] University of East Anglia, *CRU Update 2*, Nov. 24, 2009 available at http://www.uea.ac.uk/mac/comm/media/press/2009/nov/CRUupdate

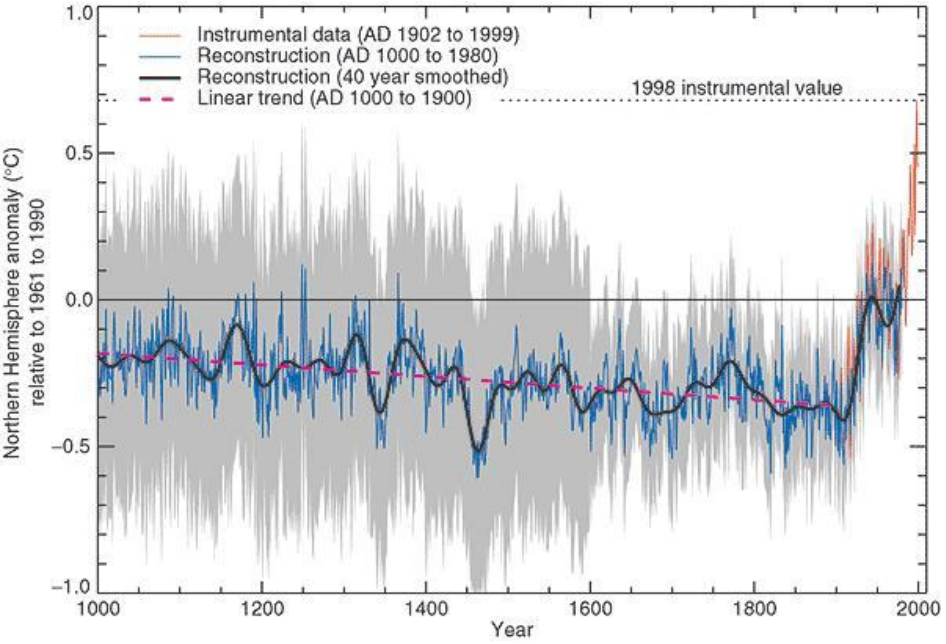
[175] *Id.* at front cover.



This shows the clear use of a deceptive tactic to show data using an improper combination of reconstructed and observed temperature data.

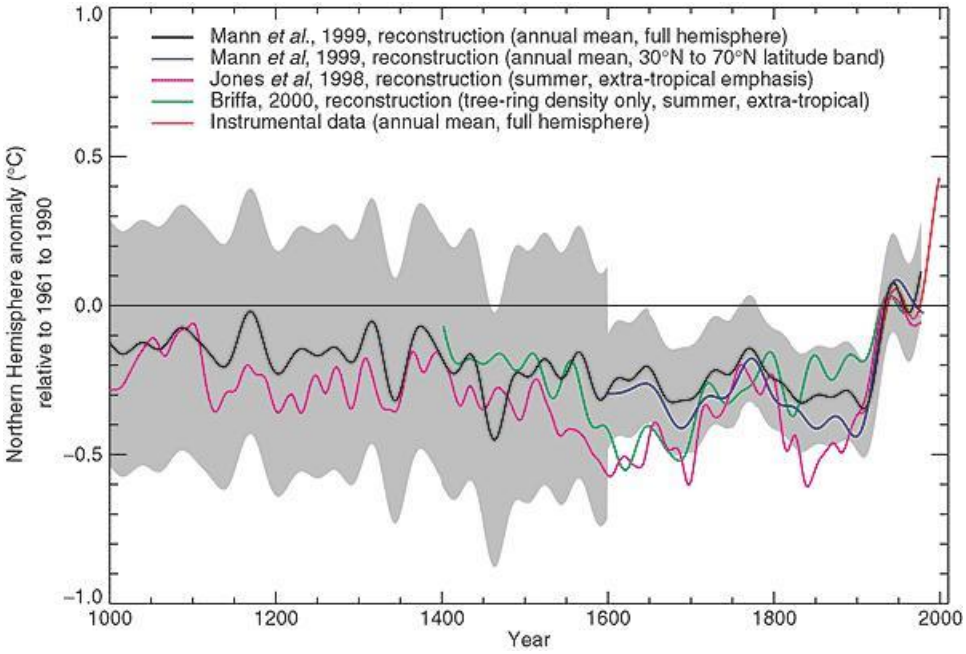
ii. Mann's "trick" as used in TAR

Mann's trick was more subtle than Briffa's. For convenience, here is the Mann hockey stick as it appeared in the TAR:



In this figure, the blue reconstruction annual series terminates in 1980 with a relatively flat period for the prior forty years or so. Yet the black "smoothed" version of the reconstruction shows an upturn at the end. This upturn is only made possible by appending the observed data to the end of the reconstructed record and calculating the end of the smooth curve using the observed temperatures. Otherwise, the black curve would be flat or slightly trending downward at the end.

The figure shown below appeared on the same page of the TAR. It also employs the trick. The Briffa et al. series is truncated in 1960 to "hide the decline," and the end of the MBH99 (black) series is smoothed through the appended observed series post-1980.



TAR 2001 WG1 Fig 2.21

The overall effect is to minimize the ***apparent*** divergence between the long proxy temperature reconstructions and the actual observed temperatures, thereby justifying the inclusion of the observed temperature series in both TAR charts 2.20 and 2.21, with the effect of enhancing the "blade" of the hockey stick and making the late 20th century temperature appear

unusual. If the divergence were fully revealed, there would be less confidence that the observed temperatures and the proxy temperatures represent an apples-to-apples comparison and therefore would undermine the depiction of temperature trends of the last 1000 years as having the shape of a hockey stick.

The "trick" has continued to have far-reaching consequences. Thus, in AR4, charts showing temperatures over the last 1000 years similarly superimpose instrument data onto proxy data to underscore warming in the 20th century.[176]

c. The CRU emails, divergence, and the trick

Although these charts are self-evidently deceptive, their main damage is the extent to which the "trick" masks the "divergence problem" and implicitly (and erroneously) validates the tree ring data. The validity of the tree ring data can be determined only by comparing it to the relatively short instrument record (approximately 150 years). Thus, the divergence of the tree ring record has serious implications for its reliability as a proxy. Because the TAR and AR4 relied so extensively upon the tree ring record to significantly downplay the MWP and LIA, their conclusions that current warming is likely unprecedented in 1000 years are cast in doubt.

In the CRU emails, the divergence issue arose frequently as a subject of uncertainty. Unfortunately, the record reveals a tendency of the leading paleoclimatologists to discount or to gloss over the problem. Their decision to minimize the divergence problem appears to be a reflection of their animosity to scientists who disagreed with them, labeled as "skeptics." In their desire to prevent the skeptics from undermining their "nice tidy" picture of unprecedented 20th century warming, however, they crossed the line from science to advocacy.

[176] *See e.g.* AR4 Figure 6.10 (showing "the various instrumental and proxy climate evidence of the variations in average large-scale surface temperatures over the last 1.3 kyr" overlapping and showing the same trend in data.)

The divergence problem was recognized internally in advance of the TAR. For example, in an email dated July 14, 1999, Cook focused on a paper published in Nature, and he wrote to Briffa:

> Also, there is no evidence for a decline or loss of temperature response in your data in the post-1950s (I assume that you didn't apply a bodge here). This fully contradicts their claims, although I do admit that such an effect might be happening in some places.[177]

"Bodge" is slang in that means a fix or a patch. Based on the context, Cook apparently perceived a discrepancy between the Nature publication, which contained a divergence issue, and a paper by Briffa using the same tree ring data, which had no such issue. By saying he assumed Briffa had not applied a "bodge," he seemed to be asking Briffa to confirm that Briffa had not masked divergence in the data through statistical legerdemain.

An email from Bradley dated July 10, 2000 similarly acknowledged the divergence problem, showing concern that it could become a foothold for climate change skeptics. Bradley wrote to Frank Oldfield, at the time the executive director at PAGES, a paleoclimate research project, and copied a number of his colleagues including Mann and Briffa. Oldfield had posed a number of "big picture" questions, including:

> How can we justify bridging proxy-based reconstruction via the last bit of instrumental time series to future model-based scenarios...
>
> I am more or less assuming that this can already be addressed from data available and calculations completed, by pointing to robust calibration over the chosen time interval and perhaps looking separately at variability pre 1970, if the last 3 decades really do seem to have distorted the response signatures for whatever reasons. I imagine developing this line of argument could feed into the 'detection' theme in significant ways.[178]

[177] CRU email 0932158667.txt (Jul. 16, 1999).

[178] CRU email 0963233839.txt (Jul. 10, 2000).

Bradley responded, in part, that the divergence problem was significant because it prevented validation of the proxy data:

> The results were good, giving me confidence that if we had a comparable proxy data set for post-1980 (we don't!) our proxy-based reconstruction would capture that period well. ***Unfortunately, the proxy network we used has not been updated, and furthermore there are many/some/ tree ring sites where there has been a "decoupling" between the long-term relationship between climate and tree growth, so that things fall apart in recent decades....this makes it very difficult to demonstrate what I just claimed***. We can only call on evidence from many other proxies for "unprecedented" states in recent years (e.g. glaciers, isotopes in tropical ice etc..). But there are (at least) two other problems -- Keith Briffa points out that the very strong trend in the 20th century calibration period accounts for much of the success of our calibration and makes it unlikely that we would be able be able to reconstruct such an extraordinary period as the 1990s with much success (I may be mis-quoting him somewhat, but that is the general thrust of his criticism). *Indeed, in the verification period, the biggest "miss" was an apparently very warm year in the late 19th century that we did not get right at all.* This makes criticisms of the "antis" difficult to respond to (they have not yet risen to this level of sophistication, but they are "on the scent"). *Furthermore, it may be that Mann et al simply don't have the long-term trend right, due to underestimation of low frequency info.*[179]

The divergence problem and other issues within paleoclimatology did not wane during the period leading up to the release of AR4. On the contrary the significance of these problems increased as they became known to a larger audience and the NRC began to investigate the issue.

In an email dated March 8, 2006, Richard Alley raised concerns about the divergence problem with Overpeck, based on concerns that the NRC might not support the conclusions that Overpeck wanted to place in AR4 as to unprecedented 20th century warmth. At the time, Alley

[179] *Id.* (emphasis added). Note the complete disregard of objective science in Bradley's statement that the "antis" had not yet realized a possible flaw in the reconstructions but were "on the scent." Bradley appears much more interested in preserving the views of himself and his colleagues than in an objective and transparent scientific process.

was a lead author of AR4 Chapter 4: "Observations: Changes in Snow, Ice and Frozen Ground." His email reveals that if tree rings missed the warming of the latter half of the 20th century, they may also have missed the warming in the MWP:

> Know anything about the 'divergence problem' in tree rings? D'Arrigo talked to the NRC yesterday. I didn't get to talk to her afterward, but it looked to me that they have redrilled a bunch of the high-latitude tree rings that underlie almost all of the high-res reconstructions, and ***the tree rings are simply missing the post-1970s warming, with reasonably high confidence.*** She didn't seem too worried, but she apparently has a paper just out in JGR. It looked to me like she had pretty well killed the hockey stick in public forum—they go out and look for the most-sensitive trees at the edge of the treeline, flying over lots and lots of trees that are lesss sensitive but quite nearby, and when things get a little warmer, the most-sensitive trees aren't anymore, and so the trees miss the extreme warming of the recent times, and ***can't reliably be counted as catching the extreme warmth of the MWP if there was extreme warmth then. Because as far as I can tell the hockey stick really was a tree-ring record, regardless of how it was labelled as multiproxy, this looks to me to be a really big deal. And, a big deal that may bite your chapter...***[180]

In the same thread, but in a different email of the same date, Alley wrote to Overpeck:

> The big issue may be that you don't just have to convince me now; if the NRC committee comes out as being strongly negative on the hockey stick owing to RD'A's talk, then the divergence between IPCC and NRC will be a big deal in the future regardless. The NRC committee is accepting comments now (I don't know for how long)... As I noted, my observations of the NRC committee members suggest rather strongly to me that they now have serious doubts about tree-rings as paleothermometers ***(and I do, too...at least until someone shows me why this divergence problem really doesn't matter).***[181]

In an email dated March 11, 2006, Alley returned to the divergence issue in a note to several colleagues, including Osborn, Briffa and Jones. He suggested that the NRC would not

[180] CRU email 1141398437.txt (Mar. 8, 2006) (emphasis added).

[181] CRU email 1141849134.txt (Mar. 8, 2009) (emphasis added).

endorse tree ring proxies as valid means for assessing past temperatures. He proposed changes to relevant portions of AR4, and then discussed divergence and related problems:

> My impression is that, for good reasons, the US NRC panel looking at the record of temperatures over the last millennium or two is not going to strongly endorse the ability of proxies to detect warming above the level of a millennium ago, and that a careful re-examination of the Chapter 6 wording and its representation in the TS and SPM would be wise. . .
>
> *These considerations do somewhat affect the confidence that can be attached to the best estimate of recent warmth versus that of a millennium ago. If the paleoclimatic data could be confidently be interpreted as paleotemperatures, then joining the paleoclimatic and instrumental records would be appropriate, and the recent warmth would clearly be anomalous over the last millennium and beyond. By demonstrating that some tree-ring series chosen for temperature sensitivity are not fully reflecting temperature changes, the divergence issue widens the error bars and so reduces confidence in the comparison between recent and earlier warmth.*[182]

Divergence-inspired doubts are again recorded in an email dated June 21, 2006. In this message, John Mitchell, a climatologist working at the Hadley Center, wrote to Overpeck, Briffa, Osborne and others:

> There needs to be a clear statement of why the instrumental and proxy data are shown on the same graph. *The issue of why we dont show the proxy data for the last few decades (they dont show continued warming) but assume that they are valid for early warm periods needs to be explained. . . .*
>
> I have not had time to check the original chapter, but the comments give the impression that the recent 50 yr warming is unprecedented over the last 500years (seems reasonable) and elsewhere over the last 1000years (less clear).[183]

Taken together, the foregoing emails present a record of serious and sustained doubt about the validity of the proxy record, and particularly, as developed from tree rings. The

[182] CRU email 1142108839.txt (Mar. 11, 2006) (emphasis added).

[183] CRU email 1150923423.txt. (Jun. 21, 2006) (emphasis added).

divergence problem plainly concerned a number of respected researchers leading them to question not only the continued use of tree ring data in the science of paleoclimatology, but also the key theory based on that data: that recent warming of the 20th century is truly unprecedented and unmatched over a period of at least 1000 years.

Nevertheless, AR4 expressed confidence in the ability of proxy temperature reconstructions to accurately represent past temperatures. Although the divergence issue had become by now enough of a publicly-recognized problem to warrant a short discussion in Chapter 6, that Chapter nevertheless found, based primarily on these reconstructions that the second half of the 20th century was likely warmer than at any time in the last 1300 years and that this warmth was more likely widespread than during any fifty-year period in 1300 years.[184]

d. Divergence Turns Out to Be a Serious Problem After All

Recent research (published since AR4) confirms that the concerns the researchers were expressing among themselves over the divergence issue were not misplaced and should have been given much more prominence than they were in the TAR and AR4. D'Arrigo continued her work on the issue (discussed in the emails above), and in 2008 published a comprehensive review of the topic. She wrote that:

> The divergence problem has important consequences for the utilization of tree-ring records from temperature-limited boreal sites in hemispheric-scale proxy temperature reconstructions (Jones et al., 1998; Mann et al.,1999; Briffa, 2000; Briffa et al., 2001; Esper et al., 2002; Cook et al., 2004a; Moberg et al., 2005; D'Arrigo et al.,2006; Hegerl et al., 2006). ***The principal difficulty is that the divergence disallows the direct calibration of tree growth indices with instrumental temperature data over recent decades (the period of greatest warmth over the last 150 years), impeding the use of such data in climatic reconstructions.*** Consequently, when such data are included, a bias is imparted

[184] AR4, Chapter 6, Executive Summary.

during the calibration period in the generation of the regression coefficients. Residuals from such regression analyses should thus be assessed for biases related to divergence, as this bias can result in an overestimation of past temperatures and an underestimation of the relative magnitude of recent warming (Briffa et al., 1998a,b).

As a result of the divergence problem, attempts to directly estimate large-scale temperatures for the recent period in dendroclimatic reconstructions have generally not been successful (Briffa et al., 1998a,b; Briffa, 2000; Briffa et al., 2001; Esper et al., 2002; D'Arrigo et al., 2006, see Fig. 3). The inability of many reconstruction models to verify in the recent period has compelled a number of researchers to eliminate recent decades from their calibration modeling, effectively shortening the available periods for direct calibration and verification testing between tree rings and climate (e.g., Briffa et al., 2001; Cook et al., 2004a; Rutherford et al., 2005; D'Arrigo et al., 2006). Another alternative is to use an empirical correction for the divergence effect (e.g., Briffa, 1992; Osborn et al., submitted for publication, Glob. Planet. Change). Compounding the problem is that many of the tree-ring records available for use in such reconstructions have been sampled at different times over the past few decades, so that their common period does not extend through to the present. This results in weaker replication of the recent period, just when stronger replication is most needed to address the divergence issue. Updating of these chronologies, many of which are from remote locations, is ongoing but requires considerable effort and resources. ***These difficulties serve to impede a robust comparison of recent warming during the anthropogenic period with past natural climate episodes such as the Medieval Warm Period or MWP*** **(Esper et al., 2005)**.[185]

And, in reference to the NRC report on paleoclimate reconstructions, she wrote:

There has been expressed concern that the divergence problem challenges the uniformitarianism assumption in tree rings (e.g., National Research Council, 2006). However, if the divergence is in fact anthropogenic in origin then it will only directly impact reconstructions within the past few decades. Some evidence suggests that this is the case, and that the divergence is limited, and unique to this recent period (Briffa et al., 1998a; Cook et al.,

[185] General findings from Rosanne D'Arrigo, et al., *On the 'divergence problem' in northern forests: a review of the tree-ring evidence and possible causes* (2008) (emphasis added).

> 2004a). Nevertheless, there are still significant implications for the development of dendroclimatic reconstructions, as we have noted in this paper. ***For example, reconstructions based on northern tree-ring data impacted by divergence cannot be used to directly compare past natural warm periods (notably, the MWP) with recent 20th century warming, making it more difficult to state unequivocally that the recent warming is unprecedented.*** [186]

Similar conclusions were recently published by Loehle:

> Tree rings provide a primary data source for reconstructing past climates, particularly over the past 1,000 years. However, divergence has been observed in twentieth century reconstructions. Divergence occurs when trees show a positive response to warming in the calibration period but a lesser or even negative response in recent decades. The mathematical implications of divergence for reconstructing climate are explored in this study. Divergence results either because of some unique environmental factor in recent decades, because trees reach an asymptotic maximum growth rate at some temperature, or because higher temperatures reduce tree growth. If trees show a nonlinear growth response, the result is to potentially truncate any historical temperatures higher than those in the calibration period, as well as to reduce the mean and range of reconstructed values compared to actual. This produces the divergence effect. *This creates a cold bias in the reconstructed record and makes it impossible to make any statements about how warm recent decades are compared to historical periods.*[187]

Loehle attempted a reconstruction without *any* tree-ring proxies and found a quite pronounced MWP (and LIA). He included a comment concerning the comparison of its results with *late* 20th century temperatures indicating that even extending the results of his study with observed temperatures still showed that the MWP was comparable to late 20th century conditions: "While instrumental data are not strictly comparable, the rise in 29 year-smoothed global data from NASA GISS (http://data.giss.nasa.gov/gistemp) from 1935 to 1992 (with data

[186] *Id.* at 301 (emphasis added).

[187] Craig Loehle, *A Mathematical Analysis of the Divergence Problem in Dendroclimatology,* 232 (2008) (emphasis added).

from 1978 to 2006) is 0.34 Deg C. Even adding this rise to the 1935 reconstructed value, the MWP peak remains 0.07 Deg C above the end of the 20th Century values, though the difference is not significant."[188] Loehle's reconstruction without tree ring data is as follows:

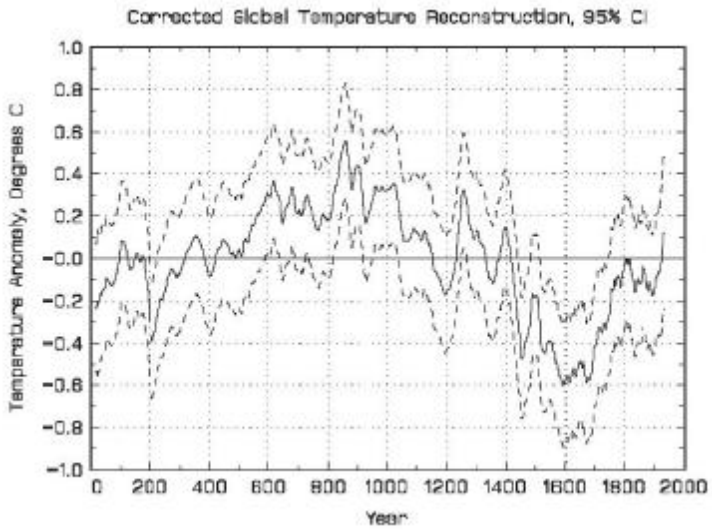


Figure: 2,000 year temperature reconstruction by Loehle and McCulloch constructed with no tree-ring proxies.

Similarly in Esper and Frank:

> DP [divergence problem] was first described over a decade ago by Jacoby and D'Arrigo (1995) and since then has been reported from a variety of sites mainly concentrated towards the Northern Hemisphere boreal forest zone (see D'Arrigo et al. 2008 for a review). DP effectively describes a disassociation of late twentieth century (typically post-1960) tree growth parameters, such as ring width or maximum latewood density, from regional temperature trends. This disassociation does not necessarily comprise a weakening of the high-frequency climate signal. That is, inter-annual tree-ring variation may be predominantly controlled by temperatures, but the long-term warming trend is not (fully) retained in the tree-ring time series. ***Such a situation is of importance, as it limits the suitability of tree-ring data to reconstruct long-term climate fluctuations, particularly during***

[188] Craig Loehle, *Correction to: A 2000 year global temperature reconstruction* 18 ENERGY & ENVM'T 93, 98 (2008).

periods that might have been as warm or even warmer than the late twentieth century.[189]

In light of this manifest uncertainty (both past and current) in using tree-rings as a temperature proxy, one would have expected caution and qualified assessments to be reflected in both TAR and AR4. Strangely, this did not happen and the CRU email database explains why.

3. Problems with the Hockey Stick

The hockey stick was first exposed as a flawed statistical analysis by two Canadian researchers, Stephen McIntyre and Ross McKitrick, in two articles published in Energy & Environment and Geophysical Research Letters.[190] McIntyre's and McKitrick's critique of the hockey stick was subsequently confirmed by the Wegman Report in 2006. Wegman concluded that McIntyre and McKitrick had "valid and compelling" criticisms of the MBH98 and MBH99 reports.[191]

Terming the MBH analysis to be "somewhat obscure and incomplete,"[192] Wegman concluded that the hockey stick shape produced by MBH's model was the result of a statistical artifact produced by flaws in the model. Specifically, MBH did not select a data set to calibrate their model that was representative of the entire 1000 year data set used in the model. Instead, according to Wegman, MBH selected 1902-1995 data to calibrate the model, and this limited data set was not "fully appropriate" for calibration purposes. Selection of this limited data set created the hockey stick; selection of a more representative data set would not have created that shape. As Wegman concluded, "[o]verall, our committee believes that Mann's assessment that

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[189] Jan Esper and David Frank, *Divergence pitfalls in tree-ring research,* 261- 262 (2009) (emphasis added).

[190] Ross McKitrick and Steve McIntyre, *Corrections to the Mann et al. (1998) Proxy Database and Northern Hemispheric Average Temperature Series,* 14 ENERGY & ENVM'T 751 (2003).

[191] Wegman Report at *Findings* 4 and 48.

[192] *Id.*

the decade of the 1990s was the hottest of the millenium and that 1998 was the hottest year of the millenium cannnot be supported by his analysis."[193]

The National Research Council ("NRC") reached the same conclusion in its 2006 report *Surface Temperature Reconstructions for the Last 2,000 Years*. According to the NRC, it had less confidence in conclusions drawn from large-scale surface temperature changes prior to about AD 1600, as compared to its high level of confidence in the Little Ice Age cooling and 20th century warming and "[e]ven less confidence . . . in the original conclusions by Mann et al. (1999) that 'the 1990s are likely the warmest decade, and 1998 the warmest year, in at least a millennium' because the uncertainties inherent in temperature reconstructions for individual years and decades are larger than those for longer time periods and because not all of the available proxies record temperature information on such short timescales."[194]

The hockey stick study was downplayed in AR4 and EPA does not cite it in the Endangerment Finding or TSD. Indeed, Briffa, who was a contributing author to Chapter 2 of the TAR and a lead author of Chapter 6 of AR4, and whose own proxy temperature reconstruction were cited in both chapters, also expressed regret in retrospect that the TAR had relied on Mann's conclusions from the hockey stick study, admitting the very large uncertainties in any of the proxy temperature reconstructions:

> The TAR was, in my opinion, wrong to say anything about the precedence (or lack thereof) of the warmth of the individual year 1998.
>
> The reason is that all reconstructions have very wide uncertainty ranges bracketing individual-year estimates of part temperature. Given this, it is hard to dismiss the possibility that individual years in the past did exceed the measured 1998 value. These errors on

[193] *Id.*

[194] NRC Report at 4.

> the individual years are so wide as to make any comparison with the 1998 measured value very problematic, especially when you consider that most reconstructions do not include it in their calibration range … and the usual estimates of uncertainty calculated … would not provide a good estimate of the likely error associated with it even if data did exist.[195]

Importantly, Wegman not only found errors in the MBH analysis, he found that these errors resulted from the fact that the paleoclimate community was networked and insular and isolated from independent, mainstream statistical experts. According to Wegman, the errors in the MBH analysis were not obvious but could have been detected had Mann sought the input of experts who were not his associates. Wegman also commented that the paleoclimate community was politicized to the point that they "can hardly reassess their public positions without losing credibility."[196]

Wegman's comments in this regard are important in light of the fact that EPA has evidently decided to disassociate the Endangerment Finding from the hockey stick. However, although EPA may not specifically rely on the hockey stick, it does rely heavily on AR4, and the work of that body on paleoclimate is heavily dominated by the same community of scientists that are responsible for authoring and reviewing the hockey stick and for giving that analysis such a dominant role in the TAR. As the CRU emails show, the Wegman Report did not cause this community to change its tactics. To the contrary, the criticisms of the hockey stick only further inflamed them.

[195] CRU email 1154484340.txt (Aug. 1, 2006).

[196] Wegman Report at 4.

4. The NRC Report Does Not Justify EPA's Conclusion that Temperatures in the Last Several Decades Are Unusual

EPA maintains that it did not rely completely on AR4 for its conclusions on paleoclimate and that, in particular, it relied on the NRC report *Surface Temperature Reconstructions for the Last 2,000 Years.*[197] EPA may therefore be tempted to argue that criticisms of the TAR and AR4 material on paleoclimate are irrelevant because EPA looked primarily to the NRC rather than the IPCC for support for its conclusions as to how unusual the current warm period is (despite EPA's repeated citation to the IPCC on this issue).

The NRC report, however, does not support the ultimate conclusion that EPA apparently seeks to draw from it. To reiterate, EPA's overall Endangerment Finding concludes that "[t]he scientific evidence is compelling that elevated concentrations of heat-trapping gases are the root cause of recently observed climate change."[198] One of EPA's three lines of evidence supporting this statement "arises from indirect, historical estimates of past climate changes that suggest that the changes in global surface temperature over the last several decades are unusual (Karl et al., 2009)."[199] EPA's citation to Karl et al, 2009 reflects EPA's belief that the period over which recent temperatures are unusual is "the last 1,000 to 2,000 years."[200]

The NRC report, however, does not reach the conclusion that there is "compelling" evidence that the temperatures of the last several decades are "unusual" in the last 1000-2000 years. According to the NRC, there is a "high level of confidence that global mean surface temperature was higher during the last few decades of the 20th century than during any

[197] Resp. to Comm. Vol. 2 at 44.

[198] Endangerment Finding, 74 Fed. Reg. at 66,518.

[199] TSD at 47.

[200] U.S. Global Change Research Program, GLOBAL CLIMATE CHANGE IMPACTS IN THE UNITED STATES 19 (Cambridge University Press 2009).

comparable period during the preceding four centuries."[201] That is not a surpassing statement given that, for most of the last 400 years, the earth experienced the LIA, the existence of which the NRC confirmed from a variety of evidence.

On the other hand, according to the NRC, "[l]ess confidence can be placed in large-scale surface temperature reconstructions for the period from A.D. 900 to 1600."[202] According to the NRC, "[e]vidence for regional warmth during medieval times can be found in a diverse but more limited set of records including ice cores, tree rings, marine sediments, and historical sources from Europe and Asia, but the exact timing and duration of warm periods may have varied from region to region, and the magnitude and geographic extent of the warmth are uncertain."[203] Thus, the most that the NRC could say as to whether temperatures today are warmer than the MWP is that "the committee *finds it plausible* that the Northern Hemisphere was warmer during the last few decades of the 20th century than during any comparable period over the preceding millennium."[204]

Going back even farther, the NRC states that "[v]ery little confidence can be assigned to statements concerning the hemispheric mean or global mean surface temperature prior to about A.D. 900 because of sparse data coverage and because the uncertainties associated with proxy data and the methods used to analyze and combine them are larger than during more recent time periods."[205]

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[201] NRC Report at 3.

[202] *Id.*

[203] *Id.* at 2.

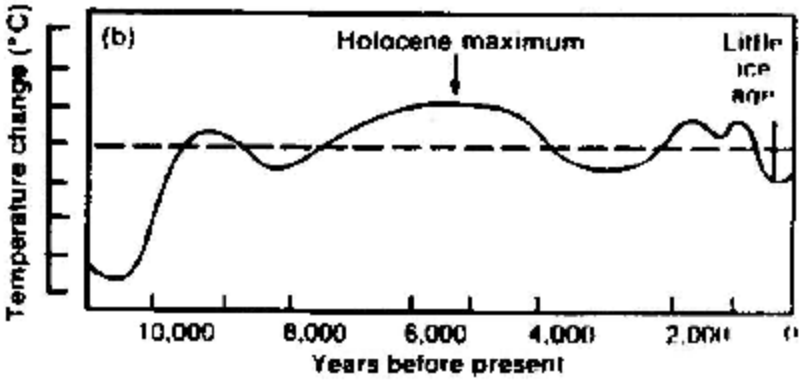
[204] *Id.* at 4 (emphasis added).

[205] *Id.* at 3.

These statements do not provide "compelling" evidence that the temperatures of the last several decades are unusual in the last 1000-2000 years and that, therefore, it can be concluded that anthropogenic GHG emissions are the "root cause" of climate change. At best, these statements support the conclusion that temperatures have fluctuated over multi-century periods, that we are in a warm period as compared to the LIA, that it is "plausible" that temperatures are warmer today than they were during the MWP, but that there is a high degree of uncertainty in that regard. This is hardly "compelling" evidence of climate conditions so unusual over one to two millennia as to justify EPA's conclusion that anthropogenic GHGs are almost certainly primarily responsible.

D. The CRU Material Undermines EPA's and the IPCC's Conclusion that the Much Warmer Periods Earlier in the Holocene Were the Result of Orbital Wobbles

The 1992 IPCC First Assessment Report expressed the long-standing scientific understanding that temperatures have oscillated throughout the Holocene, with temperatures substantially exceeding those of today early in the Holocene. The following is taken from that report:



Obviously, warmer temperatures in the early Holocene when GHG concentrations were lower than those today would seem to further undermine the conclusion that today's temperatures are unprecedented and, therefore, must be the result of anthropogenic GHG

emissions. For that reason, the authors of Chapter 6 of AR4 appear to have been determined both to provide an explanation for the early Holocene warming that preserved their view of the 20th century warming as caused by humans and to suppress evidence to the contrary.

Relying entirely on that chapter, EPA's Response to Comments document attempted to explain that the early Holocene warmth was not globally synchronous, and was instead dominated by high-latitude summer warming – consistent with the projections of climate models run with the orbital parameters characteristic of that period. This is important because (1) it suggests that globally, the early Holocene was not warmer than present, and (2) science completely understands why the climate behaved the way it did during that period (variations in the earth's orbit around the sun).

Here is the excerpt from the EPA's Response to Comment 3-55 on this issue:

> Although temperatures in some regions were warmer than present during earlier parts of the Holocene, the IPCC additionally noted that these local warm periods were very likely not globally synchronous and that that the tendency for high-latitude summer temperature maxima to occur early in the Holocene (8,000 to 10,000 years ago) points to a direct influence of orbital forcing. Jansen et al. (2007) find: "When forced by 6 ka [kiloannum] orbital parameters, state-of-the-art coupled climate models and EMICs [Earth System Model of Intermediate Complexity] capture reconstructed regional temperature and precipitation changes…whereas simulated global mean temperatures remain essentially unchanged (<4°C; Masson-Delmotte et al., 2005b) [the IPCC wrote "<0.4°C"], just as expected from the seasonality of the orbital forcing."
>
> In any case, the TSD summarizes the assessment science regarding the significant uncertainties associated with large-scale surface temperature reconstructions. With specific regard to temperatures from earlier parts of the Holocene, we report the following in the TSD (see Box 5.1): "According to the IPCC (Jansen et al., 2007), current data limitations limit the ability to determine if there were

> multi-decadal periods of global warmth compared to the last half of the 20th century prior to about 1000 years ago."[206]

However, the CRU material undermines EPA's reliance on AR4 Chapter 6 (i.e., Jansen et al., 2007). In fact, the IPCC intentionally omitted reference to peer-reviewed scientific studies which were inconsistent with the IPCC's contention that climate models run with changing orbital parameters accurately capture the reconstructed temperature patterns. Because EPA relied exclusively on the IPCC in this regard, the Agency did not refer to or try to explain away these studies.

There is a threaded discussion among various authors of Chapter 6 about how to handle articles in the scientific literature that presented evidence counter to the orbital theory for explaining early Holocene warmth. Climate models run with the orbital parameters occurring during that period projected only high latitude warmth, while the overall planetary temperatures were little changed. Yet there were at least two papers that reported that glacial evidence suggested that the tropics and the Southern Hemisphere were also relatively warm at the same time. These papers were:

- Stephen C. Porter, *Onset of Neoglaciation in the Southern Hemisphere*. 15 Journal of Quaternary Science, 395-408 (2000);
- Lonnie G. Thompson et al., *Abrupt tropical climate change: past and present*. 103 Proceedings of the National Academy of Sciences, 10536-10543 (2006).

The Chapter 6 authors discussed the difficulties of the evidence presented in these papers, and neither was ultimately cited in the final version of IPCC AR4. Apparently an attempt to work in the Thompson et al. (2006) results was made (but it must have been later abandoned, because it was not included), while the Porter (2000) evidence was excluded early on.

[206] Resp. to Comm. Vol. 3 at 44-45.

Here is the description of the problem these papers presented for the predetermined view of unprecedented 20th century warmth, as explained by Chapter 6 Lead Author Olga Solomina:

> I attach here a version of glacier box and suggestions (in red) how to include there the reference to the new Thompson et al., 2006 paper.
>
> *In this relation - I am getting more and more concern about our statement that the Early Holocene was cool in the tropics - this paper shows that it was, actually, warm* - ice core evidences+glaciers were smaller than now in the tropical Andes. The glaciers in the Southern Hemisphere (Porter, 2000, review paper) were also smaller than at least in the Neoglacial. *We do not cite Porter's paper for the reason that we actually do not know how to explain this - orbital reason does not work for the SH, but if we do cite it (which is fair) we have to say that during the Early to Mid Holocene glaciers were smaller than later in both Northen, and Southern Hemisphere, including the tropics, which would contradict to our statement in the Holocene chapter and the bullet.* It is probably too late to rise these questions, but still just to draw your attention.[207]

Another Lead Author of Chapter 6, Valerie Masson-Delmotte (not coincidentally the author of the paper that Chapter 6 cited *in support of* the orbit hypothesis) suggested to Solomina that there were many uncertainties associated with tropical glacial reconstructions and that they were not reliable temperature proxies. She suggested that Chapter 6 should include further discussions of these uncertainties (if it was to include the results of Thompson et al. and/or Foster - which run counter to her paper (cited in the AR4 text as Masson-Delmotte et al., 2005b):

> It seems to me that there is still a large uncertainty about the temperature versus precipitation effect on these tropical glaciers. Other indications from south America are related to lake levels with contrasted views in the low versus highlands.
>
> Several references suggest that there is the end of a wet period after the early Holocene in tropical south America; this is expected to induce an increase of 18O signals. One review was conducted

[207] CRU email 1154353922.txt (Jul. 31, 2006) (emphasis added).

> several years ago within the PEPI project (http://wwwpaztcn.wr.usgs.gov/pcaw/ and references herein).
>
> I think that the state of the art is that we have no reliable proxy record that is sensivite to temperature only on the tropical lands for the Holocene; therefore the statement that was written for the Holocene was based on areas of the tropical oceans where SST reconstructions were published**.**
>
> Do we have to write more explicitely about the uncertainty?[208]

Ultimately, the Coordinating Lead Author of Chapter 6, Eystein Jansen, decided, *contrary to the contentions of Thompson et al. and Foster*, that the early Holocene evidence from the tropics and Southern Hemisphere is not a reliable temperature indicator, and to just leave the text the way it was – that is, without including the published evidence from Thompson et al. (2006), Foster (2000), or any discussion surrounding these studies that runs contrary to the orbital hypothesis:

> I agree with Valerie that the ice core evidence is ambiguous. I would personally place more weight on the alkenone data, which is a reasonable well calibrated SST proxy. Foraminifer transfer function based SSTs and some Mg/Ca results that are available suggest a similar picture as far as I know. Of course it is possible and plausible that the tropical oceans are behaving in a non consistent manner and not all areas are showing the same signal, but a sizeable portion appear to do so in order to conclude as we do in the chapter in my opinion. Some signals may be due to changes in in trade wind induced coastal upwelling strength, but there are enough cores with alkenone data outside of these areas. If we were to say more about the uncertainties it may be the fact that proxies are seasonally skewed.
>
> *My conclusion is to let the chapter say what we say at the moment.*[209]

[208] *Id.*

[209] *Id.*

In other words, since he could not reconcile the physical evidence presented by Thompson et al. (2006) and/or Porter (2000) with the model-based understanding, one of the two AR4 Chapter 6 Coordinating Lead Authors made a decision not to include a discussion of these papers in the chapter. His editorial bias was shared by Overpeck, the other Coordinating Lead Author for Chapter 6. On January 13, 2005, Overpeck wrote to Briffa and Osborn about various subsections of Chapter 6, encouraging them to dismiss the significance of both early Holocene warming and the MWP: "Holocene Thermal Maximum is another one that should only be used with care, and with the explicit knowledge that it was a time-transgressive event totally unlike the recent global warming."[210]

Thus, it is clear that the IPCC AR4 is not an accurate assessment of the scientific literature, but instead includes only a selection of the literature that supports a particular viewpoint – one either held by the chapter IPCC authors, or which had been dictated to them by more influential IPCC authorities. This is perhaps the type of behavior that Chapter 6 Lead Author Briffa was referring to when he told a colleague "***I tried hard to balance the needs of the science and the IPCC, which were not always the same***."[211]

E. Sum as to Paleoclimate

The IPCC's analysis is far from the fair and neutral examination of the science that one would expect in a report so extensively relied on by EPA. It appears to have been driven as much by an advocacy agenda as by science, and it plainly overstated its conclusion that temperatures in the latter part of the 20th century were likely unprecedented in 1000 years. In fact, neither AR4 nor the NRC report provides "compelling" evidence that temperatures in the

[210] CRU email 1105670738.txt (Jan. 13, 2005).

[211] CRU email 1177890796.txt (Apr. 29, 2007) (emphasis added).

latter part of the 20th century were so unusual in the last 1000-2000 years as to inevitably lead to the conclusion that anthropogenic GHGs must be the cause**.**

V.

APART FROM THE PALEOCLIMATE, EPA'S OTHER EVIDENCE OF ATTRIBUTION IS UNDERMINED BY THE CRU MATERIAL AND OTHERWISE DOES NOT PROVIDE A RATIONAL BASIS FOR THE ENDANGERMENT FINDING

"How come you do not agree with a statement that says we are no where close to knowing where energy is going or whether clouds are changing to make the planet brighter. We are not close to balancing the energy budget. The fact that we can not account for what is happening in the climate system makes any consideration of geoengineering quite hopeless as we will never be able to tell if it is successful or not! It is a travesty!"

Dr. Kevin Trenberth[212]

In addition to evidence from the paleoclimate, the Endangerment Finding cited two other "lines of evidence" for its conclusion that anthropogenic GHGs are the "root cause" of recently observed climate changes. One line of evidence is "our basic physical understanding of the effects of changing concentrations of greenhouse gases, natural factors, and other human impacts on the climate system." The other is based on computer model simulations of the global climate system.[213] We discuss both of these lines of evidence below.

A. Climate Response to Increasing GHGs

EPA's "physical understanding" of the effect of increased atmospheric GHGs on the climate begins with the simple formulation that (a) GHGs have increased in the atmosphere compared with pre-industrial times and (b) GHGs are known to trap heat.[214] Those facts alone obviously are not sufficient to attribute current climate conditions to anthropogenic GHG emissions – otherwise, there would be no need for most of the discussion in the Endangerment Finding, TSD and the "assessment literature" that EPA cites. In fact, the direct heat-trapping

[212] CRU email 1255523796.txt (Oct. 14, 2009).

[213] Endangerment Finding, 74 Fed. Reg. at 66,518.

[214] TSD at 23-24.

properties of GHGs are insufficient to account for the warming that occurred in the last several decades of the 20th century or that EPA projects in the future. To tie anthropogenic GHGs to the late 20th century and projected future warming, it must be assumed that increased atmospheric GHG concentrations will have a positive feedback effect in the climate system thereby significantly amplifying the warming response.[215]

However, whether or not GHGs, in fact, produce such a positive feedback effect depends on an understanding of complex global climatic factors, and in this area the scientific community's understanding is considerably less certain than its understanding that GHGs tend to trap heat. As a result, in order to attribute current observed climate changes to anthropogenic GHG emissions, EPA relied on certain indicia, or "fingerprints," in observed climate changes in the recent past to conclude that anthropogenic GHGs are causing these climate changes. EPA's view is that the character of certain observed climate changes is the character one would expect from climate change caused by increased atmospheric GHGs, and hence the changes can be attributed to such increases.[216]

The key fingerprint that EPA relies on is temperature, and in particular the amount and rate of warming in the last several decades of the 20th century and in the asserted increase in observed warming with altitude in the troposphere. Although EPA refers to other climate fingerprints, these asserted fingerprints were addressed in comments on the Endangerment Finding and will not be further addressed here. As to temperature, however, both on the issue of late 20th century warming and warming in the tropical troposphere, the evidence EPA relied is

[215] *See, e.g.*, AR4 at § 8.6.

[216] TSD at 50.

undermined by the CRU material and otherwise does not furnish the logical support that EPA claims.

1. Late 20th Century Warming

a. Conclusions Undermined by Lack of Warming from 1998-2008

Temperatures began to be measured more or less systematically on a global basis around 1850. That period coincided roughly with the end of the LIA, and thus global temperatures were depressed at that time.

Since 1850, there have been two periods of apparently pronounced temperature increases, 1910-1945 and 1977-1998. Since 1998, there has been an eleven-year period of no warming. The magnitude and duration of the two periods of 20th century warming are statistically similar. As EPA stated, "[a]s noted by the commenters, the thirty-year rate of warming for the period from the 1910s to the 1940s is very similar to the rate of warming for the 1970s to the 2000s."[217] EPA also stated that the first period did not result from the combustion of fossil fuels, as there was little increase during this time of atmospheric carbon dioxide. According to EPA, "[f]rom 1850 to 1950, there was a decrease in volcanic activity, an increase in solar radiative forcing, and an increase in forcing due to long-lived gases from anthropogenic activities (presumably mostly land-use change and agriculture during that period)."[218]

Logically, the fact that (a) the warming trend in the second period was the same as experienced in the first period and (b) the warming in the first period was not primarily caused by anthropogenic GHGs might lead to the conclusion that the warming in the second period was also not primarily caused by anthropogenic GHGs. Nevertheless, EPA expressed confidence in

[217] Resp. to Comm. Vol. 3 at 33.

[218] *Id.* at 45.

its conclusion that anthropogenic GHGs are responsible for the second period of warming based on EPA's professed understanding of the natural and anthropogenic climate forcings during these respective periods. According to the Agency's citation to the IPCC, in contrast to the first period of warming, the second warming period was accompanied by a reduction in solar radiation and by increased levels of anthropogenic and natural aerosols (from volcanoes) that should have cooled the planet. Thus, EPA concluded that the latter period of warming was not the result of natural forces and instead must have been caused primarily by anthropogenic GHG emissions.[219]

EPA's claimed understanding of the causes of the warming in the latter part of the 20th century, however, is contradicted by the lack of warming over the 1998-2008 period. EPA did not cite any natural forces that might explain the lack of warming during this period, a period when atmospheric GHG concentrations continued to increase. There has been no climatologically important volcano eruption during the past eleven years nor any pronounced net increase in anthropogenic emissions of aerosols (although there has likely been variability in regional trends). EPA's only explanation was that there appears to be enough natural variability in the climate system to accommodate decade-long periods of no warming superimposed on an underlying, longer-term anthropogenically-caused warming trend. EPA maintained in this regard that "observations over such [decade-long] periods examined in isolation may be misleading in the interpretation of the longer-term trend in temperatures."[220]

EPA's statement that conclusions cannot be drawn from climate events occurring at the scale of a decade or less is odd because EPA did just that when discussing Arctic sea ice and global sea level rise. For instance, the Agency cited the statement that:

[219] TSD at 50.

[220] Resp. to Comm. Vol. 3 at 3.

> However, climate change is happening even faster than previously estimated; global CO2 emissions since 2000 have been higher than even the highest predictions, Arctic sea ice has been melting at rates much faster than predicted, and the rise in the sea level has become more rapid.[221]

EPA cannot have it both ways. It cannot insist that the cessation of warming over the last eleven years must be the result of poorly understood natural variability; whereas, other climate phenomena during that period conclusively demonstrate man's impact on the climate and are not explained by the same variability.

Moreover, EPA's discussion of the period over which warming must be sustained to provide confidence that the warming is not natural is confusing. According to EPA, "[b]oth the IPCC and the TSD note that 'difficulties remain in attributing temperature changes on smaller than continental scales and over time scales of less than fifty years,' and that with limited exceptions attribution at these scales has not yet been established."[222] Since the warming of the second period lasted only about thirty years before it ceased, and since the plus-fifty year warming trend of the 20th century includes a period in which the warming did not result primarily from anthropogenic GHGs, it would appear that EPA cannot justify its conclusion that the warming of the last thirty years can be definitively attributed to anthropogenic GHGs.

In fact, the CRU emails show that key scientists believe that because science's understanding of how the climate responds to natural variability is poor, science's understanding as to what caused the lack of warming during the past ten years is likewise poor. They go so far as to concede that such lack of warming demonstrates that science does not, in fact, understand

[221] Resp. to Comm. Vol. 1 at 41.

[222] Resp. to Comm. Vol. 3 at 3.

how the climate system works. On October 14, 2009, Trenberth wrote to Wigley about the lack of warming and said that:

> Here are some of the issues as I see them: *Saying it is natural variability is not an explanation. What are the physical processes? Where did the heat go?* We know there is a build up of ocean heat prior to El Nino, and a discharge (and sfc T warming) during late stages of El Nino, but is the observing system sufficient to track it? Quite aside from the changes in the ocean, we know there are major changes in the storm tracks and teleconnections with ENSO, and there is a LOT more rain on land during La Nina (more drought in El Nino), so how does the albedo change overall (changes in cloud)? At the very least the extra rain on land means a lot more heat goes into evaporation rather than raising temperatures, and so that keeps land temps down: and should generate cloud. But the resulting evaporative cooling means the heat goes into atmosphere and should be radiated to space: so we should be able to track it with CERES data. The CERES data are unfortunately wonting and so too are the cloud data. The ocean data are also lacking although some of that may be related to the ocean current changes and burying heat at depth where it is not picked up. If it is sequestered at depth then it comes back to haunt us later and so we should know about it.[223]

Trenberth also wrote to Wigley on the same day**:**

> *How come you do not agree with a statement that says we are no where close to knowing where energy is going or whether clouds are changing to make the planet brighter. We are not close to balancing the energy budget. The fact that we cannot account for what is happening in the climate system makes any consideration of geoengineering quite hopeless as we will never be able to tell if it is successful or not! It is a travesty!*[224]

He also sent an email to a number of colleagues, stating:

> *The fact is that we can't account for the lack of warming at the moment and it is a travesty that we can't. The CERES data published in the August BAMS 09 supplement on 2008 shows there*

[223] CRU email 1255523796.txt (Oct. 14, 2009) (emphasis added).

[224] *Id.* (emphasis added).

> *should be even more warming: but the data are surely wrong. Our observing system is inadequate.*[225]

In other words, in one day just last fall, one of the two coordinating lead authors of the AR4 chapter on observed climate (a chapter that EPA relied on heavily in its own discussion of observed climate) admitted that (1) natural variability is not a satisfactory or sufficient explanation for the lack of warming over the course of the last decade, (2) the lack of warming is a "travesty" because it means that we cannot tell where the energy in the climate system is going, and (3) the lack of warming is a "travesty" because the models predict that there should be "even more warming," so either the models are wrong or "[o]ur observing system is inadequate" – or both.

It is also particularly relevant that Trenberth stated that "[t]he fact that we can not account for what is happening in the climate system makes any consideration of geoengineering quite hopeless as we will never be able to tell if it is successful or not!" Trenberth's reference to "geoengineering" here included reducing GHG emissions.[226] In other words, Trenberth stated that the flaws in the climate community's understanding of climatic forces that are exposed by the lack of warming is so fundamental – and the extent of natural variability must be so great – that it could never be demonstrated that reducing GHG emissions will reduce warming.

Trenberth's statement eviscerates the grounds for EPA's Endangerment Finding. The purpose and effect of that finding is to trigger regulation mandating GHG reductions to eliminate or at least mitigate the danger. But if, as Trenberth says, the science is too uncertain to determine whether GHG reductions will produce a measurable climate response, then there is no

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[225] *Id.* (emphasis added).

[226] Trenberth has publicly (and recently) referred to attempts to "reduce emissions... or reduce the amount of carbon dioxide in the atmosphere" as "geoengineering." *See* Physics Today letter 2/09, at http://www.cgd.ucar.edu/cas/Trenberth/trenberth.papers/GeoengineeringPhsToday.pdf

basis to regulate and no basis to express confidence that anthropogenic GHG emissions are primarily responsible for the warming of the last several decades.

Finally, a recent paper confirms that the recent lack of warming exposes fundamental gaps in science's understanding of the climate system. Solomon et al. (2010) concluded that the lack of recent warming was a result of a drop in the water vapor content of the stratosphere, which undergoes substantial variation on decadal time scales.[227] The causes for this are unclear. This is different than the usual explanation for temperature variations on this time scale, which are usually related to El Nino, solar activity and sporadic volcanism. Her findings demonstrate an entirely new factor—one with only limited representation in climate models-- that is capable of modulating the surface temperature several tenths of a degree, and provide another example of how unsettled global warming science actually is.

b. Are the Climatological Records Accurate?

Of course, the supposition that the last several decades of the 20th century were unusual depends on the reliability of measurements during that period. Recent information calls into question just how reliable those measurements and associated compendia were.

i. Problems in the CRU Data

One of the principal surface temperature records is the HadCRUT3 data set produced by CRU. We discuss below in section VI(E) the revelation that CRU is unable to replicate how it adjusted raw surface temperature records in developing its HadCRUT3 data set and indeed destroyed at least some of the underlying raw temperature data. As the ability to replicate

[227] Susan Solomon et al., 2010. *Contribution of Stratospheric Water Vapor to Decadal Changes in the Rate of Global Warming*.

research is a minimum indicator that the results are reliable, the HadCRUT3 data set can no longer be relied on for scientific conclusions.

But in addition to problems with the HadCRUT3 dataset, the CRU material illuminates in detail, profound and pervasive problems with another primary climatological dataset developed by the CRU, that being version 2.1 and 3.0 of their time series ("TS") dataset which contains gridded climate data on a fine spatial scale. The TS2.1/3.0 datasets contain observations of monthly maximum and minimum temperature, precipitation, rainday counts, vapor pressure, cloudiness and wind speed. According to the CRU, this dataset "has provided many researchers, in the UK and overseas, with their basic data for a whole range of studies."[228] Further it (and its pre-curser) is directly cited in the AR4 Chapter 9.[229] and incorporated into other research cited by the IPCC.

Myriad problems with the TS2.1/3.0 dataset are detailed in the form of the "Harry_Read_Me" files that were released as a part of the CRU material that became available last fall. The files appear to be coder notes by a CRU technical employee working with the TS2.1/3.0 data. The notes appear to reveal a shocking state of anarchy in the TS2.1/3.0 data. As just a few examples, the coder states:

- "But what are all those monthly files? DON'T KNOW, UNDOCUMENTED. Wherever I look, there are data files, no info about what they are other than their names. And that's useless …"[230]

- "It's botch after botch after botch."[231]

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[228] History of the Climate Research Unit *available at http://www.cru.uea.ac.uk/cru/about/history/*

[229] AR4, Chapter 9 at References. Timothy D. Mitchell, Philip D. Jones, *2005: An improved method of constructing a database of monthly climatological observations and associated highresolution grids,* 25 INT. J. CLIMATOL., 693. (2005) and M.G. New , et al., *2000: Representing twentiethcentury space-time climate variability. Part II: development of 1901-96 monthly grids of terrestrial surface climate,* 13 J. CLIM. 2217 (2000).

[230] Harry_Read_Me at 17.

- The biggest immediate problem was the loss of an hour's edits to the program, when the network died … no explanation from anyone, I hope it's not a return to last year's troubles … This surely is the worst project I've ever attempted. Eeeek."[232]

- "Oh, GOD, if I could start this project again and actually argue the case for junking the inherited program suite."[233]

- "… this should all have been rewritten from scratch a year ago!"[234]

- "Am I the first person to attempt to get the CRU databases in working order?!!"[235]

- "As far as I can see, this renders the (weather) station counts totally meaningless."[236]

- "COBAR AIRPORT AWS (data from an Australian weather station) cannot start in 1962, it didn't open until 1993!"[237]

- "What the hell is supposed to happen here? Oh yeah — there is no 'supposed,' I can make it up. So I have : –)"[238]

- "You can't imagine what this has cost me — to actually allow the operator to assign false WMO (World Meteorological Organization) codes!! But what else is there in such situations? Especially when dealing with a 'Master' database of dubious provenance …"[239]

- "So with a somewhat cynical shrug, I added the nuclear option — to match every WMO possible, and turn the rest into new stations … In other words what CRU usually do. It will allow bad databases to pass unnoticed, and good databases to become bad …"[240]

- "OH F— THIS. It's Sunday evening, I've worked all weekend, and just when I thought it was done, I'm hitting yet another problem that's based on the hopeless state of our databases." [241]

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[231] *Id.* at 18.

[232] *Id.* at 31.

[233] *Id.* at 37.

[234] Id. at 41.

[235] *Id.* at 47.

[236] *Id.* at 57.

[237] *Id.* at 71.

[238] *Id.* at 98.

[239] *Id.*

[240] *Id.* at 98-99.

[241] *Id.* at 241.

- "This whole project is SUCH A MESS …"[242]

Peabody does not have the resources to try to untangle the coder notes and the TS2.1/3.0 data files to determine the magnitude of the errors in those files. Given the state of the record at this point, the TS2.1/3.0 dataset and the HadCRUT3 cannot legitimately be relied on nor can any study or modeling which utilized these data.

ii. EPA's Rationales

EPA was aware at the time it published the Endangerment Finding that some of the raw data used in HadCRUT3 had been destroyed but dismissed concerns about the reliability of these records on essentially three grounds. First, it uncritically adopted CRU's public position that since (a) ninety-five percent of the raw data has long been available to researchers on the Global Historical Climatology Network ("GHCN") and (b) CRU's adjustment methodology is available, then (c) researchers could independently replicate and confirm the appropriateness of CRU's adjustments.[243] But as shown in section VI(E) below, the reason CRU and independent researchers cannot replicate CRU's adjustments is because CRU can no longer determine which data from the GHCN it used. EPA's unwillingness to make its own assessment of this issue further illustrates that EPA has ceded its own judgment to third parties.

Second, EPA relies on the fact that CRU's adjustment methodology was peer-reviewed.[244] The fact that the adjustment methodology was peer-reviewed, however, does not indicate that the methodology was correctly applied in practice. If CRU cannot replicate its adjustments (as in the case with their HadCRUT3 data), or if adjustments were made after the publication of peer-reviewed methodology (as in the case with their TS2.1/3.0 data), there is no

[242] *Id.* at 266.

[243] Resp. to Comm. Vol. 2 at 28-29.

[244] *Id.* at 27.

basis to say that the adjustments, in fact, were properly applied. CRU – and EPA – in essence are saying "trust me," but any basis to do so seems to have collapsed with the Harry_Read_Me files.

Moreover, as is evident in several emails,[245] the CRU temperature data set is in a constant state of flux, with the data set constantly being modified both in terms of the inclusion/exclusion of data as well as the methods employed. Thus, the datasets now available have not been directly peer-reviewed.

Third, and most importantly, EPA says that the NOAA and NASA surface temperature records show "similar trends" as the HadCRUT3 data set and hence the IPCC's and EPA's reliance on the HadCRUT3 records is not unreasonable.[246] This contention does not hold up. The HadCRURT3 data was relied on extensively by the IPCC and in numerous studies cited by both the EPA and all of the "assessment literature" that EPA cites. It is not enough for EPA to say that the HadCRUT3 data reveals "similar trends" as other data. The specific amount of warming that occurred during the last several decades of the 20th century, and how that warming compares to other periods in the temperature record, was obviously critically important in all of these studies. Many of these studies involved complex statistical analyses of the underlying data. Those studies may have yielded different results had they used the NASA or NOAA data instead of the HadCRUT3 data.

Moreover, there are now significant questions about whether the NASA and NOAA temperature records are truly independent of the HadCRUT3 data set. In this regard, the investigation of the Science and Technology Committee of the United Kingdom Parliament that

[245]CRU email 1247199598.txt (Jul. 10, 2009), CRU email 1252090220.txt (Sept. 4, 2009).

[246] Resp. to Comm. Vol. 2 at 27-28.

was recently initiated to investigate CRU's conduct includes investigation into the question of "How independent are the other two international data sets?"[247] This is a critically important question given EPA's view, based on the IPCC, that the rate and extent of warming in the last several decades of the 20th century is the key "fingerprint" of an anthropogenic GHG cause. If that warming has been overstated in all three data sets, then the "fingerprint" disappears, or grows more faint even if all three data sets show "similar" warming.

In sum, the problems in the CRU data call into question not just those data but all of the many studies that relied on that data.

2. Human Fingerprint on the Tropical Troposphere

The Agency continues to point in the final Endangerment Finding, as it did in the proposed version, to a predicted tropical troposphere "fingerprint" as evidence of anthropogenic warming.[248] After a number of comments noted the findings of multiple studies, based on actual observed atmospheric data, that tend to disprove the existence of this fingerprint, EPA sought to bolster the evidence in the final Endangerment Finding – thin indeed in the proposed Endangerment Finding and accompanying TSD – with references to studies by Haimberger et al. (2008),[249] Allen and Sherwood (2008),[250] and Santer et al. (2008).[251]

The reliability of these studies and their conclusions is called into substantial question, however, by information contained in the CRU materials discussed below. The timing of at least

[247] http://www.parliament.uk/parliamentary_committees/science_technology/s_t_pn14_100122.cfm.

[248] Resp. to Comm. Vol. 3 at 5.

[249] Leopold Haimberger et al., *Towards Elimination of the Warm Bias in Historic Radiosonde Temperature Records—Some New Results from a Comprehensive Intercomparison of Upper Air Data,* 21 J. CLIM. 4587 (2008).

[250] Robert Allen and Steve Sherwood, *Warming maximum in the tropical upper troposphere deduced from thermal wind observations,* 1 NAT. GEOSCI. 399 (2008).

[251] Benjamin Santer et al., *Consistency of Modelled and Observed Temperature Trends in the Tropical Troposphere,* 28 INT. J. CLIMATOL. 1703 (2008).

some of these studies in relation to those cited by the commentators on the proposed Endangerment Finding appears to have been carefully manipulated to ensure that the advocates of a fingerprint had the last word. This effort was characterized by conduct that undermines the integrity of the scientific method. The Agency should therefore consider anew the studies and data that tend to disprove the existence of the predicted tropical fingerprint.

All climate models predict that warming resulting from GHGs would produce a fingerprint – a warming trend that increased with altitude – in the tropical troposphere. The CCSP 2006 study drew that conclusion, as did the policymakers' summary for AR4. The actual data in CCSP 2006, however, showed just the opposite, as confirmed in a 2007 paper published by Douglass, Christy, Pearson and Singer in the International Journal of Climatology ("IJC") ("Douglass et al. 2007").[252]

In the TSD supporting the proposed Endangerment Finding, the Agency largely ignored this fundamental discrepancy. The TSD cited to Karl (2006) as evidence of the fingerprint, but simply noted that "an important inconsistency may have been identified in the tropics."[253] Noting that "observational data sets show more warming at the surface than in the troposphere, while almost all model simulations have larger warming aloft than at the surface," the Agency, nevertheless, concluded that the inconsistency could possibly be explained by "error in the observations," while noting that the issue was still under investigation.[254]

Commenters pointed out that this explanation was entirely insufficient. These commenters noted that in its proposed finding, EPA offered no basis for concluding that any

[252] David Douglass et al., *A comparison of tropical temperature trends with model predictions,* 28 INT. J. CLIMATOL.1693 (2007).

[253] TSD at 50.

[254] *Id.*

relevant data were erroneous, nor did the Agency explain why the more likely explanation – that the fingerprint of anthropogenic warming was missing – should be rejected. The Commenters discussed the studies by Christy et al. (2007)[255] and Douglass et al. (2007)[256] that showed substantial disparities between the model predictions and actual data observed.[257]

In its response to these comments, EPA stated that it "is aware of . . . the challenges in identifying the anthropogenic fingerprint in the tropics."[258] The Agency nevertheless believed that "[t]he TSD's characterization of this issue is consistent with the assessment literature as well as the most recent studies . . . ." EPA asserted that those studies found that "when uncertainties in models and observations are properly accounted for, newer observation data sets are in agreement with climate model results."[259]

The Agency has acknowledged the Christy et al. (2007) and Douglass et al. (2007) studies, but believes they are answered by the work of Haimberger et al. (2008),[260] Allen and Sherwood (2008),[261] and Santer et al. (2008).[262] The Haimberger study purported to find that temperature trends reflected in RICH-RAOBCORE v. 1.4, a homogenized weather balloon (radiosonde) data record, are "more consistent with trends from recent climate model runs than earlier radiosonde data sets," and concluded that "[i]n the upper tropical troposphere, . . . there is

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[255] John Christy et al., *Tropospheric Temperature Change Since 1979 From Tropical Radiosonde and Satellite Measurements,* 112 J. GEOPHYS. RES. D06102 (2007).

[256] David Douglass et al., *A Comparison of Tropical Temperature Trends with Model Predictions* (2007).

[257] EPA-HQ-OAR-2009-0171-3596 at 40-41.

[258] Resp. to Comm. Vol. 3 at 5.

[259] *Id.*

[260] Leopold Haimberger et al., *Towards Elimination of the Warm Bias in Historic Radiosonde Temperature Records—Some New Results from a Comprehensive Intercomparison of Upper Air Data* (2008).

[261] Robert Allen and Steve Sherwood, *Warming maximum in the tropical upper troposphere deduced from thermal wind observations,* 1 NAT. GEOSCI. 399 (2008).

[262] Benjamin Santer et al., *Consistency of Modelled and Observed Temperature Trends in the Tropical Troposphere* (2008).

no significant discrepancy between trends from RICH-RAOBCORE v. 1.4 and the range of temperature trends from climate models."[263] This result, Haimberger asserted, "directly contradicts the conclusions of a recent paper by Douglass et al. (2007)."[264]

Allen and Sherwood conceded that "direct temperature observations from radiosonde and satellite data have often not shown this expected trend," i.e., a trend of faster warming in the upper tropical troposphere than the surface.[265] They found, however, that "non-climatic biases" have been found in such measurements, and that when the "thermal-wind equation" is applied to wind measurements from radiosonde data, "warming trends are consistent with model predictions except for small discrepancies close to the tropopause."[266] Lastly, Santer et al. are cited by the EPA for the assertion that, based on their comparison of observational trends with models, "[t]here is no longer a serious and fundamental discrepancy between modeled and observed trends in tropical lapse rates, despite [the Douglass et al. (2007)] incorrect claim to the contrary."[267]

EPA built on an unstable foundation in relying upon these three studies. The CRU materials reveal that Santer and his allies – a group that included Sherwood and Haimberger, authors of the other articles on which EPA relied to explain away the disagreement between the models and the tropical troposphere data – exerted improper influence to pressure the IJC to delay publication of Douglass et al. (2007) and to expedite publication of their response, thereby giving them the "last word" and depriving Douglass and his co-authors of the opportunity that

[263] Resp. to Comm. Vol. 3 at 5 (Haimberger quoted).

[264] *Id.*

[265] Resp. to Comm. Vol. 3 at 5.

[266] *Id* at 6.

[267] *Id.*

they would normally have to respond. Dozens of emails among these individuals reveal an effort extending over nearly a year, and including such inappropriate conduct as (a) unusual cooperation between authors and editor that undermined the independence of each, (b) misstatement of known facts, (c) outright character assassination, (d) avoidance of traditional scientific give-and-take, (e) use of confidential information, (f) misrepresentation, or at a minimum misunderstanding, of the scientific question posed by Douglass et al. (2007), (g) withholding of material data, and more. All of this was done under a cloak of confidentiality and with a concern about leaks. Such conduct places the work of these scientists on a questionable footing and stands in stark contrast to the Douglass et al. (2007) study, which underwent a traditional, rigorous peer-review process.

More specifically, the editor of IJC appears to have breached confidentiality obligations by disclosing the page proofs of Douglass et al. (2007) to New York Times reporter Andrew Revkin, who then disclosed them to three of these scientists a week before initial publication on line: "Sorry to take your time up, but really do need a scrub of singer/christy/etc effort ."[268]

The three recipients of this message then began to organize a response from the larger group of Jones, Santer, Thorne, Sherwood, Lanzante, Taylor, Seidel, Free, and Wentz. In an email dated December 4, 2007, Santer replied to all:

> I'm forwarding this to you in confidence. We all knew that some journal, somewhere, would eventually publish this stuff. Turns out that it was the International Journal of Climatology.[269]

Jones then responded to Santer on the same day:

[268] CRU email 1196795844.txt (Dec. 4. 2007).

[269] *Id.*

> It sure does! Have read briefly -- the surface arguments are wrong. I know editors have difficulty finding reviewers, but letting this one pass is awful -- and IJC was improving.[270]

The next day, December 5, 2007, the day the Douglass et al. (2007) paper appeared on-line, Santer sent an e-mail to Peter Thorne with copies to Carl Mears, Leopold Haimberger, Karl Taylor, Tom Wigley, Phil Jones, Steve Sherwood, John Lanzante, Dian Seidel, Melissa Free, Frank Wentz, and Steve Klein. Santer indicated that the strategy should be to get a response published in IJC in a way that customary conventions are ignored and the authors are prevented from responding:

> Peter, I think you've done a nice job in capturing some of my concerns about the Douglass et al. paper ... I don't think it's a good strategy to submit a response to the Douglass et al. paper to the International Journal of Climatology (IJC). As Phil [Jones] pointed out, IJC has a large backlog, so it might take some time to get a response published. Furthermore, Douglass et al. probably would be given the final word.[271]

The following day, Free responded with a cautious note. She had presented a paper with Lanzante and Seidel at an American Meteorological Society conference (18th Conference on Climate Variability and Change) acknowledging the existence of a discrepancy between observations and models – the basic conclusion of the Douglass et al. (2007) paper. She said "[w]hat about the implications of a real model-observation difference for upper-air trends? Is this really so dire?[272] Santer then responded with the key reason for attacking Douglass et al. (2007) "[w]hat is dire is Douglass et al.'s willful neglect of any observational datasets that do not support their arguments."[273] This "willful neglect" of "observational datasets" refers to the

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[270] *Id.*

[271] CRU email 1196877845.txt (Dec. 5, 2007).

[272] CRU email 1196956362.txt (Dec. 6, 2007).

[273] *Id.*

absence of two balloon data sets, RAOBCORE v1.3 and v1.4. As discussed below, Douglass et al. (2007) explained in an addendum that these data sets are faulty.

Another e-mail from Jones discussed options to beat Douglass et al. (2007) into print.[274] Wigley entered the email dialogue to accuse Douglass et al. (2007) of "fraud" and asserted that under "normal circumstances," this would "cause him [Douglass] to lose his job." [275] Wigley also suggested telling this to Chris Mooney, an environmental journalist known for aggressive attacks on those deemed to be "skeptics": "I have suggested that someone like Chris Mooney should be told about this."[276]

Tim Osborn, a colleague of Jones at CRU, a member of the group that worked on Chapter 6 of AR4 as seen above, and a member of the editorial board of IJC, then inserted himself into the process, declaring a bias on the issue and stating that Douglass's previous papers "appear to have serious problems."[277] Santer responded with gratitude for the "heads up," again making the incorrect claim that Douglass et al. (2007) ignored certain balloon data sets.[278]

Writing to Santer and Jones, Osborn then discussed the "downside" of the normal comment-reply process, in which Douglass et al. (2007) would be given an "opportunity to have a response."[279] He explained that he had contacted the senior editor of IJC, Glenn McGregor, to "see what he can do." According to Osborn, McGregor "promise[d] to do everything he can to achieve a quick turn-around." He also stated:

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[274] CRU email 1196964260.txt (Dec. 6, 2007).

[275] CRU email 1197325034.txt (Dec. 10, 2007).

[276] *Id.*

[277] CRU email 1197507092.txt (Dec. 12, 2007).

[278] *Id.*

[279] CRU email 1199988028.txt (Jan. 10, 2008).

> (and please treat this in confidence, which is why I emailed to you and Phil only) that he [McGregor] may be able to hold back the hardcopy (i.e. the print/paper version) appearance of Douglass et al., possibly so that any accepted Santer et al. comment could appear alongside it.[280]

He [McGregor] intended to "correct the scientific record" and to identify in "advance reviewers who are both suitable and available," perhaps including "someone on the email list you've been using."[281] This appears to be a flagrant abuse of the review process, where reviewers are not permitted to be close associates of the author.

Santer responded with his conditions, highlighting the intent that he must have the last word:

> 1) Our paper should be regarded as an independent contribution, not as a comment on Douglass et al. ...
>
> 2) If IJC agrees to 1), then Douglass et al. should have the opportunity to respond to our contribution, and we should be given the chance to reply. Any response and reply should be published side-by-side, in the same issue of IJC.
>
> I'd be grateful if you and Phil could provide me with some guidance on 1) and 2), and on whether you think we should submit to IJC. Feel free to forward my email to Glenn McGregor.[282]

Douglass and his co-authors were never informed of this process, which specifically addressed their paper, nor were they contacted for an explanation on any point raised in these negotiations.[283]

On January 10, 2008, Jones told the team (Wigley, K. Taylor, Lanzante, Mears, Bader, Zwiers, Wentz, Haimberger, Free, MacCracken, Jones, Sherwood, Klein, Solomon, Thorne,

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[280] *Id.*

[281] *Id.*

[282] *Id.*

[283] Posting by David Douglass and John Christy, *A Climatology Conspiracy*? http://www.americanthinker.com/ 2009/12/ a_climatology_conspiracy.html. (Jan. 29, 2010).

Osborn, Schmidt, and Hack) a "secret" he had learned from Osborn: that one of the recipients on the Santer e-mail list was one of the original reviewers of Douglass et al. (2007) who did not reject the article.

> The problem !! The person who said they would leave it to the editor's discretion is on your email list! I don't know who it is - Tim does - maybe they have told you? I don't want to put pressure on Tim. He doesn't know I'm sending this. It isn't me by the way - nor Tim ! Tim said it was someone who hasn't contributed to the discussion - which does narrow the possibilities down![284]

The relationship between Osborn and McGregor was clearly not unbiased, and the journal was persuaded to disregard proper peer-review procedures in order to hurry the team's response into print. Osborn then wrote to Santer and Jones:

> I just contacted the editor, Glenn McGregor, to see what he can do. He promises to do everything he can to achieve a quick turn-around time (he didn't quantify this) and he will also "ask (the publishers) for priority in terms of getting the paper online asap after the authors have received proofs." He genuinely seems keen to correct the scientific record as quickly as possible.
>
> ***He also said (and please treat this in confidence, which is why I emailed to you and Phil only) that he may be able to hold back the hardcopy (i.e. the print/paper version) appearance of Douglass et al., possibly so that any accepted Santer et al. comment could appear alongside it***. Presumably depends on speed of the review process.
>
> If this does persuade you to go with IJC, Glenn suggested that I could help (because he is in Kathmandu at present) with ***achieving the quick turn-around time by identifying in advance reviewers who are both suitable and available.*** Obviously one reviewer could be someone who is already familiar with this discussion, because that would enable a fast review - i.e., someone on the email list you've been using - though I don't know which of these people you will be asking to be co-authors and hence which won't be available as possible reviewers…[285]

[284] CRU email 1199999668.txt (Jan. 10, 2008).

[285] *Id.* (emphasis added).

Osborn, a member of both CRU and the editorial board of IJC, went so far as to accept from Santer a list of people to send the article for review.[286]

The final approval of the strategy (Santer's conditions) to deny Douglass and his collaborators an opportunity to respond in the normal way was acknowledged by Osborn to Santer and Jones in that Osborn wrote that McGregor, as editor, is "prepared to treat it as a new submission rather than a comment on Douglass et al." and "[McGregor's] offer of a quick turn around time etc still stands."[287] Osborn also reminded Santer and Jones of the impropriety of this situation: "the only thing I didn't want to make more generally known was the suggestion that print publication of Douglass et al. might be delayed ... all other aspects of this discussion are unrestricted."[288]

McGregor informed Santer that he had received one set of comments and though he "... would normally wait for all comments to come in before providing them to you, I thought in this case I would give you a head start in your preparation of revisions."[289] Later, Santer wrote to Jones on July 10, 2008, that the two subsequent reviews were in, but reviewer number two was "somewhat crankier."[290] Santer indicated that McGregor had told him that he would not resend the coming revised manuscript to the "crankier" reviewer in another apparent effort by McGregor to accommodate Santer.[291]

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[286] CRU email 1200059003.txt (Jan. 11, 2008).

[287] CRU email 1200076878.txt (Jan. 11, 2008).

[288] *Id.*

[289] CRU email 1209080077.txt (Apr. 24, 2008).

[290] CRU email 1215712600.txt (Jul. 10, 2008).

[291] *Id.*

On July 21, 2008, Santer heard that his paper was formally accepted, and expressed his gratitude to Osborn for "all your help with the tricky job of brokering the submission of the paper to IJoC."[292]

On October 10, 2008, the Santer et al. paper was published online. Thirty-six *days* later, Santer et al. appeared in print, immediately following Douglass et al. (2007), whose authors had waited over eleven months for their paper to appear in print. The strategy of preventing Douglass and his collaborators from having any opportunity for a simultaneous response to Santer et al. had been achieved. The gamesmanship behind this strategy diverted the process of scientific inquiry from its proper path and tainted the materials on which the Agency now seeks to rely.

3. Sum as to Temperature "Fingerprints"

In sum, the CRU material undermines the temperature the "fingerprint" that the IPCC and EPA see in the warming of the late 20th century. The amount and rate of warming exposes, in Trenberth's words, the "travesty" of the significant gaps in science's understanding of the climate system. And the proof on which the IPCC and EPA rely of an anthropogenic GHG influence in the tropical troposphere is tainted by the inappropriate and indeed unethical way in which that proof was published.

[292] CRU email 1216753979.txt (Jul. 22, 2008).

B. The CRU Material Contradicts EPA's Explanation of Why the Failure of the Planet to Warm Over the Last Decade Is Consistent with Climate Models

The CRU material also contradicts EPA's assertion that the results of climate models constitute a third line of evidence that can be relied on to attribute climate change to anthropogenic GHG emissions. In fact, as this material demonstrates, the failure of the planet to warm over the last eleven years raises serious questions about the accuracy of the models, questions that EPA failed to address adequately while relying on information that was not discussed in the proposed Endangerment Finding.

EPA's conclusion that the lack of recent warming is consistent with the models is based on the Agency's view that the natural variability of the climate system can produce a decade-long period of no warmth superimposed on a longer, anthropogenically-caused warming trend. We discussed the inconsistencies of EPA's discussion in this regard above at section V(B). Additionally, EPA calls on two studies, Easterling and Wehner (2009)[293] and Knight et al. (2009),[294] as support for the notion that the climate models can produce short periods of temperature trends that run counter to the overall long-term warming trend.[295] The Knight study was published after the comment deadline, and so the public did not have an opportunity to comment on it. The Easterling and Wehner study was published in April just before the comment deadline, but the public did not have an opportunity to fully comment on it because it was published after the Endangerment Finding proposal, and so was not relied on by EPA.

As important, because the failure of the planet to warm is such a recent event, the peer review literature has not yet matured to the point that the issue has received extensive analysis.

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[293] David Easterling and Michael Wehner, *Is the climate warming or cooling?* 36 GEOPHYS. RES. LETT. (2009).

[294] John Knight, et al., *Do global temperature trends over the last decade falsify climate predictions?* [in "State of the Climate in 2008"], 90 BULL. AMER. METEOR. SOC. S22-S24, (2009).

[295] TSD at 22-23.

Thus, although there is not yet peer review literature that contradicts these two studies, EPA should carefully consider the methods used and the conclusions reached in these studies and any potentially conflicting information.

The need for EPA to carefully consider its reliance on these studies is reinforced by the CRU material. Despite the availability of these two papers in the scientific literature, Trenberth was unconvinced that the recent lack of warming was consistent with the scientific understanding of the climate system on which the models are based, as his emails last October discuss. To reiterate, he commented that it is a "travesty" that the recent warming cannot be explained, and "[t]he fact is that we can't account for the lack of warming at the moment and it is a travesty that we can't."[296]

1. Easterling and Wehner and Knight et al. Do Not Provide Comfort That the Models Can Account for the Lack of Warming

As discussed in the next section below, both of these studies have flaws that result in an overstating of the likelihood that the models can account for the lack of warming. But even taken at face value, these studies should provide little comfort to EPA. Easterling and Wehner found that during the first half of the 21st century, there is a one in ten chance of a zero (or negative) trend in temperatures through ten years of data. Knight et al. found that for the entire 21st century there is a 5% chance of a zero (or negative) trend through eleven years of data. Given these very low odds, and given that we are only in the first decade of the 21st century and have already experienced an eleven-year trend of no warming, these studies hardly provide reassuring support for the underlying accuracy of the models' long-term predictive capacity.[297]

[296] CRU email 1255352257.txt (Oct. 12, 2009).

[297] These studies are discussed more fully in our Petition at section V(B).

2. Problems with Easterling and Wehner and Knight et al.

In any event, good grounds exist to question the two studies. As to Easterling and Wehner, according to EPA:

> A very recent NOAA study (Easterling and Wehner, 2009) demonstrates this using historical data. The study states: It is true that if we fit a linear trend line to the annual global land-ocean surface air temperature (Smith et al., 2005)…for the period 1998 to 2008 there is no real trend, even though global temperatures remain well above the long-term average….However, if we fit a trend line to the same annual global land-ocean temperatures for the 1977-1985 period or the 1981–1989 period we also get no trend, even though these periods are embedded in the 1975–2008 period showing a substantial overall warming.[298]

Several things are wrong with this statement. First, the lack of a warming trend from 1998 to 2008 takes place over an eleven-year period; the earlier periods that EPA cites, 1977-1985 and 1981-1989, are only nine-year periods. While this difference may seem slight, the length of the period of no trend has a large impact on the comparison to model projections – the longer the period of no trend, the smaller the model-derived probability of its occurrence. Therefore, no overall global warming during the eleven-year period 1998-2008 is a much more unlikely event than no global warming during the nine-year periods, 1977-1985 and 1981-1989.

Further, the lack of warming during the 1977-1985 period was driven by the eruption of the El Chichon volcano in 1981, which acted to cool the earth and which lowered the global average temperature for several years thereafter. Climate models that included this eruption were largely able to replicate the lack of warming during the 1977-1985 period.[299] And, regarding the 1981-1989 example, there is a factual error in Easterling and Wehner, because this was not a period of no warming – the warming rate calculated through monthly surface

[298] Resp. to Comm. Vol. 2 at 31.

[299] AR4 at 684, Figure 9.5.

observations from January 1981 through December 1989 is +0.08C/decade in both the GISS and CRU-compiled temperature data sets.[300]

A second basis to question the result of Easterling and Wehner (2009) is that they characterized the climate model projections using an emissions scenario (SRES A2) that is an underestimate of the actual level of GHG emissions that have occurred in the 21st century - a fact recognized by the EPA: "global CO2 emissions since 2000 have been higher than even the highest predictions…"[301] Model projections that use an emissions scenario that underestimates actual GHG emissions necessarily leads to an underestimate of the model-projected rate of warming than if the actual (higher) emissions were used instead. Models projecting a greater rate of warming also project a smaller probability of a prolonged period (greater than ten years) of a general lack of warming (such as 1998-2008).[302] For this reason, the probability of a ten-year period of no warming, as determined by Easterling and Wehner from climate model projections and given as 10%, is too large. If the actual emissions were used to drive the climate models, the probability of occurrence of a ten year period of no trend would be smaller, and the mismatch between model expectations and observations would be higher.

A third basis to question the results of Easterling and Wehner (2009) appears to be a flaw in the methodology of calculating the overall distribution of model-projected ten year trend expectations, from which their probability of a ten year period of no trend was determined.

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[300] As is shown through a simple least-squares regression fit to the monthly data from the CRU and NASS/GISS records from January 1981 through December 1989.

[301] Resp. to Comm. Vol. 1 at 41.

[302] The modeled natural variability does not systematically change from emissions scenario to emissions scenario (see for instance IPCC AR4 WGI Figure 10.26 (p. 803), which shows that the uncertainty range of future temperature projections is similar across all of the SRES scenarios). Thus, those scenarios that produce a greater trend from greenhouse gases will have less chance of a zero (or negative) trend of a particular length than those scenarios which produce a smaller trend.

Easterling and Wehner (2009) apparently combined the results from all available climate model runs into one large distribution. However, since some individual models had more available model runs than other models, the models with a greater number of individual runs – particularly those models with a large degree of internal noise – had a greater influence on the overall distribution, acting in effect to disproportionately broaden the tails of the distribution and produce too large an estimate of the probability of occurrence of a zero (or less) trend. The proper method would have been to properly weight the results of multiple runs from a single model, such that the overall distribution of trends represented an equal contribution from each climate model. This would have resulted in a smaller estimate of the probability of occurrence of a ten year period of no trend.

Properly accounting for these issues would have lowered the probability (and thus the confidence) that climate models could explain the recent lack of global warming. Therefore, the results of Easterling and Wehner (2009) should not be considered a reliable source for establishing that the recent warming is consistent with climate model expectations.

The study from Knight et al. (2009) also suffers from methodological flaws that lead to an over-estimate of the probability that model results are consistent with recent observed trends. This renders the results unreliable for determining the level of consistency between recent observed trends and climate model projected trends. Knight et al. (2009) doesn't suffer from an improper combination of multiple model results because they only examine the results of a single climate model – the HadCM3 model. However, Knight et al. (2009) did not compare observed trends with the results of the HadCM3 model when run with an emissions scenario that is closest to observed emissions (SRES A1B). Doing so would have shown that the observed trend since the beginning of the 21st century was lower than any similar length trend projected by the

HadCM3 model, and thus was unanticipated by the climate model. Instead, they artificially enhanced the level of variability by combining the results of the model when run with various other emissions scenarios and also by changing various parameters within the climate model itself. These changes (which do not occur in nature, i.e. actual variability is only produced by one emissions pathway and a single set of physics) acted to broaden the tails of the distribution of model projected trends, which, as with Easterling and Wehner (2009), lead to an artificially inflated probability of occurrence of low (or no) trends in global temperatures.

Again, properly accounting for these issues would have lowered the probability (and thus the confidence) that climate models could explain the recent lack of global warming. Therefore, the results of Knight et al. (2009) cannot be considered a reliable source for establishing that the recent warming is consistent with climate model expectations.

In sum, even discounting the methodological issues in Easterling and Wehner and Knight et al., the chances that the models can properly account for an eleven year period of no warming are very small. But when those methodological issues are taken into account, the models demonstrably do not take account of the current no-warming trend. The models, therefore, must have flaws reflecting, as Trenberth said, a fundamental lack of understanding of the climate system.

VI.

CONTRARY TO EPA'S PERCEPTION THAT THE IPCC WAS AN OPEN PROCESS, THE CRU MATERIAL REVEALS A PATTERN OF BLOCKING ACCESS TO UNDERLYING DATA AND EVEN DESTROYING DATA

"We have 25 years or so invested in the work. Why should I make the data available to you, when your aim is to try and find something wrong with it?"

Dr. Phillip Jones

The CRU material also reveals a disturbing practice of the CRU scientists and their American counterparts of refusing to allow those with different scientific viewpoints from gaining access to the data, computer codes, and other underlying information that they relied on in their studies. Without this information, the studies could not be replicated or critiqued. The attempts to evade producing information led these scientists to stonewall FOIA attempts and even to delete data and emails in order to prevent them from becoming public. As stated, these efforts by CRU to evade FOIA have now been determined to have been illegal.

Although these abuses are most noticeable as to research involving the paleoclimate, they extend to other research relevant to the attribution question. The abuses are especially alarming both because many of these scientists were either government employees or government-funded and because they were so highly influential in the IPCC on an issue of such overarching public importance. Indeed, the material that they destroyed pertained directly to their work for the IPCC and the material that they otherwise hid from public view pertained to studies that they used in the IPCC reports.

Plainly, the reason why these scientists refused to divulge underlying information was to avoid criticism of their work. The most illuminating email exchange as to why data requests were refused occurred between Warwick Hughes, an Australian scientist, and Jones. In early

2005, Hughes emailed Jones, asking for raw temperature data. Jones famously replied, "*We have 25 years or so invested in the work. Why should I make the data available to you, when your aim is to try and find something wrong with it?*"[303] Jones' widely-reported statement is presaged in the CRU material: On August 6, 2004, he commented to Janice Lough of the Australian Institute of Marine Science about releasing data: "Mike Mann refuses to talk to these people and I can understand why. *They are just trying to find if we've done anything wrong*."[304]

But, of course, science advances only when research is exposed to critical analysis; that is the entire purpose of the scientific process.

The practice of science in the shadows as reflected in the CRU material is directly contrary to the Administrator's undertaking that science relied on by EPA will be subject to the highest standards of transparency and openness. The Administrator cannot, consistent with these undertakings, rely on the AR4 material prepared by these authors.

A. Data Requests Involving the Hockey Stick

The refusals to release underlying data and information used in the paleoclimatology studies began after McIntyre first sought information from Mann about the hockey stick paper in 2003. McIntyre's data requests to Mann from this time period are not captured by the CRU materials, and it may be that Mann did not immediately liaise with his CRU colleagues. Therefore, we rely primarily during this period on (1) McIntyre's subsequent reporting of the events on his blog, ClimateAudit,[305] (2) January 27 and 28, 2005 articles in Canada's National

[303] Email provided by Warwick Hughes to whom the email was sent.

[304] CRU email 1091798809.txt (Aug. 6, 2004) (emphasis added).

[305] *See* http://climateaudit.org/.

Post,[306] and (3) the chapter of a book written by McKitrick.[307] We also discuss relevant CRU emails from the period.

As related by McIntyre, he began to contact CRU, and eventually Jones, in the spring of 2003, seeking information about the hockey stick paper.[308] At least initially, Mann was responsive to McIntyre's requests. McIntyre requested data files that he could not locate online or through other sources. This exchange is described by McIntyre in the first National Post article:

> Mann replied that he had forgotten the location … However, he said that he would ask his colleague Scott Rutherford to locate the data. Rutherford then said that the information did not exist in any one location, but that he would assemble it for me. I thought this was bizarre. This study had been featured in the main IPCC policy document. I assumed that they would have some type of due-diligence package for the IPCC on hand, as you would have in a major business transaction.[309]

Rutherford then provided data that he said was used in MBH98.[310] According to McIntyre, this initial set of data only raised more questions. McIntyre made an effort to check Mann's critical principal component calculations, but found that he could not, using Mann's data. Eventually, McIntyre (now working with McKitrick) sent the data set back to Mann to

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[306] Marcel Crok, *Breaking the hockey stick: The famous graph that supposedly shows that recent temperatures are the highest in a thousand years has now been shown by careful analysis to have been based on faulty data,* NATN'L POST FP11, Jan 27, 2005, *and* Marcel Crok, *Breaking the hockey stick,* NATN'L POST FP15, Jan. 28, 2005. (Crok's work was published as part of a two-part series.).

[307] *The Mann et al. Northern Hemisphere "Hockey Stick" Climate Index: A Tale of Due Diligence* in Patrick Michaels, ed. SHATTERED CONSENSUS: THE TRUE STATE OF GLOBAL WARMING. Rowman and Littlefield, (2006).

[308] *See* 2002 Request to CRU, *available at* http://climateaudit.org/2009/08/06/a-2002-request-to-cru/.

[309] Marcel Crok, *Breaking the Hockey Stick,* Jan. 27, 2005.

[310] Antonio Regalado, *Global Warring: In Climate Debate, The Hockey Stick Leads to a Face-Off,* WALL STREET J. A1., Feb. 14, 2005.

confirm that the data was the same as used in MBH98. Mann replied that he did not have time to answer this inquiry, or any other request.[311]

McIntyre and McKitrick then published their paper in the journal Energy and Environment in October 2003, detailing the errors found in MBH98 and McIntyre's difficulty replicating that study.[312] Mann's response followed quickly and was harshly negative.[313] He accused McIntyre and McKitrick of making numerous errors and using the wrong data.[314] In particular, Mann said that he used a different data set than the one provided to McIntyre by Rutherford.[315]

On November 12, 2003, Osborn wrote to Briffa and Jones, forwarding a request from McIntyre to Mann seeking information that would substantiate Mann's criticism of the McIntyre and McKitrick article. McIntyre also took issue with some of Mann's public statements and questioned the removal of certain previously-available CRU data.[316] Osborn was obviously disturbed that Mann had provided incorrect data to McIntyre, which, in his view, "muddied" the waters:

> you will have seen Stephen McIntyre's request to us. We need to talk about it, though my initial feeling is that we should turn it down (with carefully worded/explained reason) as another interrim stage and prefer to make our input at the peer-review stage.
>
> In the meantime, here is an email (copied below) to Mike Mann from McIntyre, requesting data and programs (and making other criticisms). I do wish Mike had not rushed around sending out

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[311] Marcel Crok, *Breaking the Hockey Stick,* Jan. 27, 2005.

[312] Ross McKitrick and Steve McIntyre, *Corrections to the Mann et. al. (1998) Proxy Database and Northern Hemispheric Average Temperature Series,* 14 ENERGY & ENVM'T 751 (2003).

[313] Marcel Crok, *Breaking the Hockey Stick*, Jan. 27, 2005, *see also*, Antonio Regalado, *Global Warring: In Climate Debate The Hockey Stick Leads to a Face-Off,* Feb 14, 2005.

[314] *Id.*

[315] *Id.*

[316] CRU email 1068652882.txt (Nov. 12, 2003).

> preliminary and incorrect early responses - the waters are really muddied now. He would have done better to have taken things slowly and worked out a final response before publicising this stuff. Excel files, other files being created early or now deleted is really confusing things![317]

In his criticism of the Energy and Environment article, Mann divulged more information about his original work, including methods and data that were not described in the original article.[318] McIntyre and McKitrick decided to continue their investigation, but in order to do so they needed Mann's original source code. They asked, but at this point Mann had decided that McIntyre and McKitrick's efforts to replicate the hockey stick analysis amounted to "intimidation tactics" and he flatly refused their request.[319] In a February 14, 2005 Wall Street Journal article, Mann explained his denial: "[g]iving them the algorithm would be giving in to the intimidation tactics that these people are engaged in."[320]

The CRU material records leading scientists' reaction to McIntyre's requests for code. In an email dated February 6, 2004, Jones wrote to a number of colleagues about McIntyre's request to Mann. He responded to a colleague's suggestion to send un-runnable portions of code accompanied by an explanation of how the code was used:

> So now it seems that we're separating 'providing the code' from 'running the code'. I can't see the purpose of one without the other. Even if Mike complies I suspect there will need to be several sessions of interaction, which neither side will be very keen on. As I said before I know the code will involve lots of combinations (for different periods with different proxies). Also I would expect, knowing the nature of the PC-type regression approach, that there will be library routines. ***If the code is sent, there needs to be conditions. We don't want McIntyre (MM) to come out and say he can't get it to work after a few days. So, it is far some simple. I'm***

[317] *Id.*

[318] Antonio Regalado, *Global Warring: In Climate Debate, The Hockey Stick Leads to a Face-Off,* Feb. 14, 2005.

[319] *Id.*

[320] *Id.*

> *still against the code being given out. Mike has made the data available. That is all they should need.* The method is detailed in the original paper - in the online (methods) and also in several other papers Mike has written.[321]

In other words, these scientists discussed either providing McIntyre with information that could not be used or not providing any information at all. Either way, they decided to stonewall McIntyre's information request.

B. Other McIntyre Requests

Jones took the same position in response to McIntyre's requests for the data underlying a further proxy reconstruction prepared by Mann and Jones (2003), a proxy temperature reconstruction that is relied on in Chapter 6 of AR4. On February 9, 2004, Jones corresponded with Tas van Ommen, an Australian scientist who had received data requests from McIntyre.[322] In the underlying email, van Ommen forwarded his correspondence with McIntyre and authorized Jones to release the requested data:

> I am aware of McIntyre's controversial history and am trying to handle things in a non-inflammatory way. He seems not to be troubling me over my own delay, but has asked for data that was used in your Holocene paper of 1998. For this, I have referred him to you. I expect he wants to replicate your synthesis, and so he should use the identical data set, and I give you permission to pass on whatever it was I gave you for that work[323]

Despite his colleague's authorization, Jones (copying Mann) responded that he would "sit tight here and do nothing." Jones apprised van Ommen of his past dealings with McIntyre, explaining how he "hid" from data requests by citing obligations to third-parties.

> Thanks for the email. Steve McIntyre hasn't contacted me directly about Law Dome(yet), nor about any of the series used in the 1998 Holocene paper or the 2003 GRL one with Mike. I suspect (hope)

[321] CRU email 1076083097.txt (Feb. 6, 2004).

[322] CRU email 1076336623.txt (Feb. 9, 2004).

[323] *Id.*

> that he won't. I had some emails with him a few years ago when he wanted to get all the station temperature data we use here in CRU. At that time, I hid behind the fact that some of the data had been received from individuals and not directly from Met Services through the Global Telecommunications Service (GTS) or through GCOS…
> I'll just sit tight here and do nothing. Mike will likely do the same, but we'll expect another publication in the nearish future.[324]

Following van Ommen's referral of McIntyre to Jones, McIntyre followed up with Jones, requesting two sets of data: "Tas van Ommen has refered me to you for the version of his dataset that you used in Jones et al Holocene 1998 and I would appreicate a copy. I would also appreciate a copy of the Lenca series used in this study."[325] This request prompted an exchange between Jones and Mann, in which Mann stated that he would not provide information to McIntyre and encouraged Jones to do the same:

> *Personally, I wouldn't send him anything. I have no idea what he's up to, but you can be sure it falls into the "no good" category.* There are a few series from our '03 paper that he won't have--these include the latest Jacoby and D'Arrigo, which I digitized from their publication (they haven't made it publicly available) and the extended western North American series, which they wouldn't be able to reproduce without following exactly the procedure described in our '99 GRL paper to remove the estimated non-climatic component. ***I would not give them *anything*. I would not respond or even acknowledge receipt of their emails. There is no reason to give them any data, in my opinion, and I think we do so at our own peril!***[326]

Similarly, on May 7, 2004, Jones wrote to Ammann and van Ommen, attaching a proof of a paper to be published in Reviews of Geophysics. In part, Jones requested permission to make data available on the CRU website. In passing, he commented on McIntyre's requests for data and his and Mann's past stonewalling: "Many of us in the paleo field get requests from

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[324] *Id.*

[325] CRU email 1076359809.txt (Feb. 9, 2004).

[326] *Id.* (emphasis added).

skeptics (mainly a guy called Steve McIntyre in Canada) asking us for series. Mike and I are not sending anything, partly because we don't have some of the series he wants, also partly as we've got the data through contacts like you, but mostly because he'll distort and misuse them."[327]

Roughly a year later, on April 27, 2005, Jones wrote to Mann about a request from McIntyre for code, and attached the request and an excerpt from McIntyre's blog recounting McIntyre's efforts to obtain data related to key papers. Jones wrote:

> Presumably you've seen all this — the forwarded email from Tim. I got this email from McIntyre a few days ago. As far as I'm concerned he has the data — sent ages ago. *I'll tell him this, but that's all — no code.* If I can find it, it is likely to be hundreds of lines of uncommented fortran ! I recall the program did a lot more that just average the series. I know why he can't replicate the results early on — it is because there was a variance correction for fewer series.[328]

As can be seen, Jones and Mann and their colleagues were obviously hostile to McIntyre and other "skeptics" because of their disagreement with the conclusions those "skeptics" might draw, saying the "skeptics" would "misuse" and "distort" the data. But this attitude is not consistent with good science, as Jones' and Mann's colleagues and other scientists attested after the CRU material was released and the stonewalling became public (see section VI(G) below).

Research, particularly research of this immense importance, should be amenable to replication by anyone, regardless of whether they will draw different conclusions from the research than those producing the research would like. Mann's refusal to supply the underlying information is particularly surprising in light of Mann's receipt of federal funding for his research, meaning the federal taxpayer paid for the development of the information that Mann refused to divulge. The bona fides of McIntyre and McKitrick should have become evident after

[327] CRU email 1083962601.txt (May 7, 2004).

[328] CRU email 1114607213.txt (Apr. 27, 2005) (emphasis added).

their 2003 paper and follow-up papers were published in peer-reviewed scientific journals. Moreover, the independent Wegman Report subsequently confirmed the validity of their critiques of the hockey stick analysis and the methodological errors in that analysis that they brought to light, and the NRC report also confirmed that it was not confident of the central conclusions of the hockey stick paper.[329] In fact, McIntyre was appointed to be a reviewer of AR4. In hindsight, Mann's and Jones' characterizations of McIntyre appear petty and defensive.

Indeed, the Wegman Report specifically took issue with the response of Mann to McIntyre and McKitrick, to the willingness of the paleoclimate community in general to share underlying information, and indeed to Mann's attitude to Wegman's efforts to review his work. The report noted Mann's "confrontational tone" in responding to the Wegman committee's requests.[330] More fundamentally, the report noted:

> Sharing of research materials, data, and results is haphazard and often grudgingly done. We were especially struck by Dr. Mann's insistence that the code he developed was his intellectual property and that he could legally hold it personally without disclosing it to peers. *When code and data are not shared and methodology is not disclosed, peers do not have the ability to replicate the work and thus independent verification is impossible.*[331]

The report noted that Mann's withholding of data was especially objectionable because the data were developed using taxpayer money:

> We believe that federally funded research agencies should develop a more comprehensive and concise policy on disclosure. All of us writing this report have been federally funded. Our experience with funding agencies has been that they do not in general articulate clear guidelines to the investigators as to what must be disclosed. Federally funded work including code should be made available to other researchers upon reasonable request, especially if

[329] Wegman Report at 40.

[330] *Id.* at 7.

[331] *Id.* at 50 (emphasis added).

> the intellectual property has no commercial value. Some consideration should be granted to data collectors to have exclusive use of their data for one or two years, prior to publication. But data collected under federal support should be made publicly available. (As federal agencies such as NASA do routinely.)[332]

According to Wegman, data transparency in paleoclimate studies is also critical because of the "isolation" of this highly-networked community from independent statisticians and because of the hugely important public policy issues involved.

> As statisticians, we were struck by the isolation of communities such as the paleoclimate community that rely heavily on statistical methods, yet do not seem to be interacting with the mainstream statistical community. The public policy implications of this debate are financially staggering and yet apparently no independent statistical expertise was sought or used.[333]

As Wegman found, the isolation of this community caused it to rally around Mann rather than engage in serious analysis of whether Mann might be wrong:

> Generally speaking, the paleoclimatology community has not recognized the validity of the MM05 papers and has tended to dismiss their results as being developed by biased amateurs. The paleoclimatology community seems to be tightly coupled as indicated by our social network analysis, has rallied around the MBH98/99 position, and has issued extensive series of alternative assessments most of which appear to support the conclusions of MBH98/99.[334]

As a result, the report concluded that "[i]n this case, we judge that there was too much reliance on peer review, which was not necessarily independent."[335]

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[332] *Id.*

[333] *Id.*

[334] *Id.*

[335] *Id.*

C. Interested Persons Resort to FOIA

Despite the outcome of the Wegman Report, neither Mann nor anyone else in the paleoclimatology changed their practice of stonewalling requests for information. To the contrary, they accelerated their highly-contentious struggle against so-called "skeptics," and they started their own RealClimate website to counter McIntyre's ClimateAudit website.[336]

With the door shut on cooperating collegially with requests for information, those seeking to verify the results of paleoclimate studies were forced to resort to the U.S. and British FOIA to obtain the underlying data. These FOIA requests were filed by McIntyre and a number of others as well. In a CRU email from March 19, 2009, Jones candidly explained how it came to pass that CRU was now being asked to produce information pursuant to FOIA. The context for the email is a discussion with Santer as to a change in the policy at the journal of the Royal Meteorological Society ("RMS") in favor of data disclosure. Jones wrote to Paul Hardaker, Chief Executive Officer of the RMS:

> Several years ago I decided there was no point in responding to issues raised on blog sites. Ben has made the same decision as well. There are probably wider issues due to climate change becoming more main stream in the more popular media that the RMS might like to consider. I just think you should be aware of some of the background. CRU has had numerous FOI requests since the beginning of 2007. The Met Office, Reading, NCDC and GISS have had as well – many related to IPCC involvement. I know the world changes and the way we do things changes, but these requests and the sorts of simple mistakes, should not have an influence on the way things have been adequately dealt with for over a century.[337]

[336] The RealClimate website was started in December 2004, with Gavin Schmitdt of the Goddard Institute of Space Studies of the United States National Aeronautics and Space Administration, Mann and Amman among the founders. CRU email 1102687002.txt. (Dec. 10, 2004).

[337] CRU email 1237496573.txt (Mar. 19, 2009).

As a result, interested persons turned to FOIA laws in the United States and the United Kingdom to pursue key data. But the existence of legal obligations under FOIA did not persuade the CRU scientists or their counterparts in the United States to be forthcoming. To the contrary, their reaction to FOIA was to stonewall, to destroy data, to hide data, and to figure out any way possible not to respond.

In the CRU material, the troubling history of efforts to avoid FOIA requests began in 2005. It appears that a generic "brochure" explaining FOIA obligations was circulated to then current and evidently at least some former scientists at CRU. On January 21, 2005, Wigley, a former director of CRU and now at NCAR, wrote to Jones, asking in relevant part "I got a brochure on the FOI Act from UEA. Does this mean that, if someone asks for a computer program we have to give it out?? Can you check this for me (and Sarah)."[338]

Jones responded on the same day to Wigley suggesting ways to avoid disclosing information:

> On the FOI Act there is a little leaflet we have all been sent. It doesn't really clarify what we might have to do re programs or data. Like all things in Britain we will only find out when the first person or organization asks. *I wouldn't tell anybody about the FOI Act in Britain*. I don't think UEA really knows what's involved. *As you're no longer an employee I would use this argument if anything comes along.* I think it is supposed to mainly apply to issues of personal information — references for jobs etc.[339]

Wigley then responded to Jones expressing concern about releasing computer code, the type of information that Wegman had specifically said should be released in order to allow independent researchers to replicate research:

[338] CRU email 1106338806.txt (Jan. 21, 2005).

[339] *Id.* (emphasis added).

> Thanks for the quick reply. The leaflet appeared so general, but it was prepared by UEA so they may have simplified things. From their wording, computer code would be covered by the FOIA. My concern was if Sarah is/was still employed by UEA. I guess she could claim that she had only written one tenth of the code and release every tenth line.[340]

Closing the loop, Jones wrote to Wigley indicating that he had figured out a way to avoid disclosure:

> As for FOIA Sarah isn't technically employed by UEA and she will likely be paid by Manchester Metropolitan University. I wouldn't worry about the code. If FOIA does ever get used by anyone, there is also IPR to consider as well. ***Data is covered by all the agreements we sign with people, so I will be hiding behind them.*** I'll be passing any requests onto the person at UEA who has been given a post to deal with them.[341]

On February 2, 2005, the issue of FOIA compliance arose again in emails between Jones and Mann. This exchange was prompted by an email from Briffa to Jones, which attached both links and full copies of news stories and blog posts that reported on Mann's refusal to turn over the code underlying the MBH98 hockey stick. As can be seen, Jones and Mann at this point were fully engaged in concealing information as a means to frustrate the work of researchers who might criticize their work, in particular McIntyre and McKitrick. In this email, they were particularly concerned about the types of data that had been posted on ftp websites for purposes of sharing with their associates but which also might be accessed by the public, including those who didn't share these scientists' views. Jones wrote to Mann, in relevant part:

> Just sent loads of station data to Scott. Make sure he documents everything better this time ! ***And don't leave stuff lying around on ftp sites — you never know who is trawling them.*** The two MMs have been after the CRU station data for years**. *If they ever hear there is a Freedom of Information Act now in the UK, I think I'll***

[340] *Id.*

[341] *Id.* (emphasis added).

> ***delete the file rather than send to anyone.*** Does your similar act in the US force you to respond to enquiries within 20 days? — our does ! The UK works on precedents, so the first request will test it. We also have a data protection act, *which I will hide behind*. Tom Wigley has sent me a worried email when he heard about it — thought people could ask him for his model code. He has retired officially from UEA so he *can hide behind* that. IPR should be relevant here, but I can see me getting into an argument with someone at UEA who'll say we must adhere to it !…[342]

Mann replied:

> Yes, we've learned out lesson about FTP. We're going to be very careful in the future what gets put there. *Scott really screwed up big time when he established that directory so that Tim could access the data.* Yeah, there is a freedom of information act in the U.S., and the contrarians are going to try to use it for all its worth. But there are also intellectual property rights issues, so it isn't clear how these sorts of things will play out ultimately in the U.S.[343]

Later the same month, Jones raised FOIA in a post-script to a message to Mann, Hughes and Bradley, co-authors of the hockey stick paper. His note appears to be a warning to colleagues that FOIA might be invoked by competing researchers in order to gain access to data. Thus, he instructed his colleagues not to disclose the existence of British FOIA laws, "PS I'm getting hassled by a couple of people to release the CRU station temperature data. ***Don't any of you three tell anybody that the UK has a Freedom of Information Act*** **!**"[344]

As the timeline advanced, the acute hostility to McIntyre and to those associated with McIntyre's website, ClimateAudit, heightened even more. Just a few months before the July 2006 Wegman Report vindicated McIntyre and McKitrick's critique of the hockey stick, Mann sent two revealing emails. On April 26, 2006, Mann told Osborn:

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[342] CRU email 107454306.txt (Feb. 2, 2005) (emphasis added).

[343] *Id.* (emphasis added).

[344] CRU email 1109021312.txt (Feb. 21, 2005) (emphasis added).

> I'm saddened to hear that this bozo is bothering you too, in addition to NCAR, NSF, NAS, IPCC and everyone else. Rest assured that I won't ever respond to McIntyre should he ever contact me, but I will forward you any email he sends related to this. I assume Scott feels the same way...[345]

And on May 12, 2006, Mann wondered why Osborn was responding to McIntyre:

> hi tim. personally, I don't see why you should make any concessions for this moron.[346]

By 2007, Jones and his colleagues had begun to prevail on UEA officials to support their position of avoiding production of information to those who were connected with the ClimateAudit website. It would not appear that FOIA authorizes government agencies such as CRU to refuse to provide information on the basis that they do not agree with the views of the requesters, but that is evidently the position CRU took.

On June 19, 2007, Jones sent a message to Wei-Chyung Wang and Tom Karl about a critical thread on ClimateAudit. With regard to FOIA, he wrote:

> Nothing much else to say except:
>
> 1. *Think I've managed to persuade UEA to ignore all further FOIA requests if the people have anything to do with Climate Audit.*
>
> 2. Had an email from David Jones of BMRC, Melbourne. He said they are ignoring anybody who has dealings with CA, as there are threads on it about Australian sites.
>
> 3. CA is in dispute with IPCC (Susan Solomon and Martin Manning) about the availability of the responses to reviewer's at the various stages of the AR4 drafts. They are most interested here re Ch 6 on paleo.[347]

[345] CRU email 1146062963.txt (Apr. 26, 2006).

[346] CRU email 1147435800.txt (May 12, 2006).

[347] CRU email 1182255717.txt (Jun. 19, 2007) (emphasis added).

A day later, Jones wrote to Wang and Thomas Peterson of NOAA, stating his unwillingness to respond to information requests if submitted by someone affiliated with ClimateAudit:

> I won't be replying to either of the emails below [from Steve McIntyre and Douglas Keenan], nor to any of the accusations on the Climate Audit website. I've sent them on to someone here at UEA to see if we should be discussing anything with our legal staff. The second letter seems an attempt to be nice to me, and somehow split up the original author team. *I do now wish I'd never sent them the data after their FOIA request!* [348]

In the same email thread Peterson replied to Jones:

> Fascinating. Thanks for keeping me in the loop, Phil. I won't pass it on but I will keep it in the back of my mind when/if Russ asks about appropriate responses to CA requests. Russ' view is that you can never satisfy them so why bother to try?[349]

D. Refusals to Provide Information Related to the IPCC and Destruction of Records

Following publication of AR4 in 2007, CRU began receiving FOIA requests seeking information in CRU's files about the drafting of Chapter 6 on paleoclimatology in which CRU scientists had been directly and influentially involved. But these requests were denied on the ground that, as an international organization, the IPCC is not subject to England's FOIA laws. Peabody does not know whether this is a valid reason for refusing to disclose information in the files of an entity (UEA and its department CRU) that is clearly subject to England's FOIA. Nevertheless, the refusal to provide information about the process of writing the IPCC reports, a process in which CRU scientists were influential, contradicts EPA's view of the transparency of that process.[350]

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[348] CRU email 1182346299.txt (Jun. 20, 2007) (emphasis added).

[349] CRU email 1182342470.txt (Jun. 20, 2007).

[350] EPA Administrator Memo to EPA Employees, January 23, 2009.

On May 9, 2008, Jones forwarded an unknown document to Mann, Bradley, and Amman that he told them they might want to delete. He said that "we" had figured out a way to avoid disclosing communications about Chapter 6 of AR4 and that they should "keep this quiet":

> *You can delete this attachment if you want.* Keep this quiet also, but this is the person who is putting in FOI requests for all emails Keith and Tim have written and received re Ch 6 of AR4. We think we've found a way around this. . .
>
> This message will self-destruct in 10 seconds![351]

On May 27, 2008, Osborn corresponded with Amman about whether Amman viewed emails related to AR4 as "confidential." One would not think that communications between scientists about the preparation of an IPCC report should be considered to be confidential, particularly where at least some of the scientists work for government agencies and others receive substantial government funding. Nevertheless, Osborn was clearly seeking an excuse for refusing to disclose information that was being sought. Briffa and Jones were copied on this exchange:

> Our university has received a request, under the UK Freedom of Information law, from someone called David Holland for emails or other documents that you may have sent to us that discuss any matters related to the IPCC assessment process. We are not sure what our university's response will be, nor have we even checked whether you sent us emails that relate to the IPCC assessment or that we retained any that you may have sent. However, it would be useful to know your opinion on this matter. In particular, we would like to know whether you consider any emails that you sent to us as confidential.[352]

On May 28, 2008, Amman responded,

> Oh MAN! Will this crap ever end??

[351] CRU email 1210341221.txt (May 9, 2008).

[352] CRU email 1211924186.txt (May 27, 2008).

> Well, I will have to properly answer in a couple days when I get a chance digging through emails. I don't recall from the top of my head any specifics about IPCC. I'm also sorry that you guys have to go through this BS. You all did an outstanding job and the IPCC report certainly reflects that science and literature in an accurate and balanced way.[353]

On May 30, 2008, Amman responded further that he considered his emails relating to AR4 to be confidential but that he did not label them as such:

> in response to your inquiry about my take on the confidentiality of my email communications with you, Keith or Phil, I have to say that the intent of these emails is to reply or communicate with the individuals on the distribution list, and they are not intended for general 'publication'. If I would consider my texts to potentially get wider dissemination then I would probably have written them in a different style. Having said that, as far as I can remember (and I haven't checked in the records, if they even still exist) I have never written an explicit statement on these messages that would label them strictly confidential.[354]

Osborn's May 30, 2008 response to this email indicates that Osborn was not looking for a good faith statement from Amman about whether the latter's emails about AR4 were confidential, but only a statement to that effect that might provide a basis to prevent disclosure of the emails:

> I don't think it is necessary for you to dig through any emails you may have sent us to determine your answer. Our question is a more general one, which is whether you generally consider emails that you sent us to have been sent in confidence. If you do, then we will use this as a reason to decline the request.[355]

Evidently, these scientists' concern about what they communicated with each other about preparation of AR4 was so serious that they undertook to delete the emails rather than have them subject to public scrutiny. Thus, on May 29, 2008, Jones issued a request to Mann under the

[353] *Id.*

[354] CRU email 1212156886.txt (May 30, 2008).

[355] CRU email 1212166714.txt (May 30, 2008).

subject line "IPCC & FOI." He asked that Mann delete his emails with Keith Briffa, and advised that he will make the same request to Gene Wahl and Caspar Amman:

> ***Can you delete any emails you may have had with Keith re AR4?*** Keith will do likewise. He's not in at the moment — minor family crisis. ***Can you also email Gene and get him to do the same?*** I don't have his new email address. We will be getting Caspar to do likewise.[356]

Later in the same thread, Mann responded to Jones that he would "contact Gene about this ASAP."[357]

On August 20, 2008, Jones indicated in an email to Gavin Schmidt and Mann that information pertaining to the development of AR4 would not be produced on the ground that the IPCC is not itself subject to FOIA:

> The skeptics will try to hang on to something, but I don't want to give them something clearly tangible. Keith/Tim still getting FOI requests as well as MOHC and Reading. All our FOI officers have been in discussions and are now using the same exceptions not to respond — advice they got from the Information Commissioner. As an aside and just between us, it seems that Brian Hoskins has withdrawn himself from the WG1 Lead nominations. It seems he doesn't want to have to deal with this hassle. *The FOI line we're all using is this. IPCC is exempt from any countries FOI – the skeptics have been told this. Even though we (MOHC, CRU/UEA) possibly hold relevant info the IPCC is not part our remit (mission statement, aims etc) therefore we don't have an obligation to pass it on.*[358]

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[356] CRU email 1212073451.txt (May 29, 2008) (emphasis added). At about the same time that Jones and Mann were discussing deleting emails, Jones asked Mann to nominate him for a Fellowship of the American Geophysical Union ("AGU"). Mann agreed and asked Jones to supply him with the number of times Jones' published papers had been cited in other published papers, which is a criterion the AGU looks at in awarding fellowships. Jones replied that his search revealed 62 citations but that he thought some of them were to another scientist of the same name, so Mann should "go with 52." Remarkably, Mann replied "OK—thanks. I'll just go with H [number of citations]=62. That is an impressive number and almost certainly higher than the vast majority of American Geophysical Union Fellows." CRU emails 1212924720.txt (Jun. 8, 2008) and 1213201481.txt (Jun. 11, 2008).

[357] CRU email 1212063122.txt (May 29, 2008).

[358] CRU email 1219239172.txt (emphasis added).

By this time, the wagons were fully circled at CRU as to information requests regarding the IPCC and, indeed, any information requests from anyone connected with ClimateAudit. In response to a Mann email on December 3, 2008, Jones relayed his success in convincing the officials at UEA responsible for responding to FOIA requests that requests from those associated with ClimateAudit could be refused based on their scientific and policy views, even though these officials had originally thought that FOIA required a response. Jones admitted that he had deleted "loads" of emails, just as he had asked Mann and others to do the previous May:

> When the FOI requests began here, the FOI person said we had to abide by the requests. It took a couple of half hour sessions — one at a screen, to convince them otherwise showing them what CA was all about. *Once they became aware of the types of people we were dealing with, everyone at UEA (in the registry and in the Environmental Sciences school — the head of school and a few others) became very supportive.* I've got to know the FOI person quite well and the Chief Librarian — who deals with appeals. The VC is also aware of what is going on — at least for one of the requests, but probably doesn't know the number we're dealing with. We are in double figures.
>
> One issue is that these requests aren't that widely known within the School. So I don't know who else at UEA may be getting them. CRU is moving up the ladder of requests at UEA though — we're way behind computing though. We're away of requests going to others in the UK — MOHC, Reading, DEFRA and Imperial College.
>
> So spelling out all the detail to the LLNL management should be the first thing you do. I hope that Dave is being supportive at PCMDI.
>
> The inadvertent email I sent last month has led to a Data Protection Act request sent by a certain Canadian, saying that the email maligned his scientific credibility with his peers! If he pays 10 pounds (which he hasn't yet) I am supposed to go through my emails and he can get anything I've written about him. ***About 2 months ago I deleted loads of emails, so have very little — if anything at all.*** This legislation is different from the FOI — it is supposed to be used to find put why you might have a poor credit rating !

> In response to FOI and EIR requests, we've put up some data - mainly paleo data. Each request generally leads to more — to explain what we've put up. Every time, so far, that hasn't led to anything being added — instead just statements saying read what is in the papers and what is on the web site! Tim Osborn sent one such response (via the FOI person) earlier this week. ***We've never sent programs, any codes and manuals.***[359]

A few days later on December 10, 2008, Jones responded to a question by Santer as to the number of FOI inquiries to CRU. Jones explained the strategy adopted by CRU for refusing to divulge information concerning the IPCC and, in the process, revealed that he had been told his deletion of emails was inappropriate:

> Haven't got a reply from the FOI person here at UEA. So I'm not entirely confident the numbers are correct. One way of checking would be to look on CA, but I'm not doing that. ***I did get an email from the FOI person here early yesterday to tell me I shouldn't be deleting emails – unless this was 'normal' deleting to keep emails manageable!***[360]

Again, Peabody expresses no opinion as to the validity under U.K. law of CRU's excuse to shield IPCC related communications. We submit that the excuse would not be a valid under U.S. law. Destruction of public records to avoid disclosure under FOIA unquestionably violates U.S. and U.K. law. The more important point is the veil of secrecy that the CRU scientists drew over their work with the IPCC, which does not comport with U.S. scientific norms. It illustrates that these scientists were acting as advocates, and it leads to the question, what were they hiding?

[359] CRU email 1228412429.txt (Dec. 3, 2008) (emphasis added).

[360] CRU email 1228922050.txt (Dec. 10, 2008) (emphasis added).

E. FOIA Requests Lead to Revelation that CRU Cannot Replicate Its Temperature Records and, In Fact, Has Destroyed Raw Temperature Data that It Used in Creating Those Records

CRU's obstruction of efforts to obtain information that could be used to replicate and critique the work of its scientists might imply that CRU had something to hide. That implication would seem to be confirmed by CRU's response to FOIA requests for information used to produce its temperature record. The response first involved obstruction, and then CRU made the admission that CRU cannot at this point determine exactly what data it used to produce that record and indeed destroyed some of that data.

One of the principal functions of the CRU is to maintain a record of global surface temperatures. As EPA indicates, the CRU temperature record is one of the main surface temperature records and EPA relied on it for its conclusion that temperatures have risen during the 20th century.[361] CRU's published temperature record, known as HadCRUT3, is not raw temperature data collected from temperature stations around the world. Instead, HadCRUT3 reflects adjustments to the raw data to account for various conditions that might affect the accuracy of the raw data.

Within the last several years researchers have sought information from CRU that might help them replicate and critique how CRU adjusts data. But this effort was stonewalled. We set forth below a lengthy email exchange between Willis Eschenbach and CRU in which Eschenbach asked CRU to provide the list of meteorological stations used in the preparation of the HadCRUT3 global temperature average and the raw data for those stations. After his initial,

[361] TSD at 28-29.

informal request was denied, Eschenbach began filing FOIA requests under British law.[362] CRU's response was to refer him to websites containing the general set of temperature records from which CRU selected specific records for use in producing HadCRUT3, but CRU refused to identify which specific station records that CRU actually chose for use.[363] As Eschenbach's emails show, without this information, determining exactly how the HadCRUT3 was produced cannot be determined.

On February 10, 2007, Eschenbach received this initial reply from Mr. Dave Palmer of CRU:

> Your request for information received on 28 September now been considered and I can report that the information requested is available on non-UEA websites as detailed below.
>
> The Global Historical Climatology Network (GHCN-Monthly) page within US National Climate Data Centre website provides one of the two US versions of the global dataset and includes raw station data. This site is at: http://www.ncdc. noaa.gov/ oa/climate/ ghcn-monthly/ index.php
>
> This page is where you can get one of the two US versions of the global dataset, and it appears that the raw station data can be obtained from this site.
>
> Datasets named ds564.0 and ds570.0 can be found at The Climate & Global Dynamics Division (CGD) page of the Earth and Sun Systems Laboratory (ESSL) at the National Center for Atmospheric Research (NCAR) site at: http://www.cgd. ucar.edu/ cas/tn404/
>
> Between them, these two datasets have the data which the UEA Climate Research Unit (CRU) uses to derive the HadCRUT3 analysis. The latter, NCAR site holds the raw station data (including temperature, but other variables as well). The GHCN would give their set of station data (with adjustments for all the numerous problems).

[362] The communications between Eschenbach and the CRU have been provided by Eschenbach and can be reviewed at Willis vs. The CRU: A History of (FOI) Evasion, *available at* http://omniclimate.wordpress.com/2009/11/24/ willis-vs-the-cru-a-history-of-foi-evasion/.

[363] *Id.*

> They both have a lot more data than the CRU have (in simple station number counts), but the extra are almost entirely within the USA. We have sent all our data to GHCN, so they do, in fact, possess all our data.
>
> In accordance with S. 17 of the Freedom of Information Act 2000 this letter acts as a Refusal Notice, and the reasons for exemption are as stated below
>
> Exemption Reason
>
> s. 21, Information accessible to applicant via other means Some information is publicly available on external websites[364]

Eschenbach shortly filed a second request that did not object to obtaining the data from third-party sources, but simply asked that CRU specifically identify the data it actually used so he could find it. He submitted a further email:

> Thank you for your reply (attached below). However, I fear that it is totally unresponsive. I had asked for a list of the sites actually used. *While it may (or may not) be true that "it appears that the raw station data can be obtained from [GHCN]", this is meaningless without an actual list of the sites that Dr. Jones and his team used.*
>
> The debate about changes in the climate is quite important. Dr. Jones' work is one of the most frequently cited statistics in the field. Dr. Jones has refused to provide a list of the sites used for his work, and as such, it cannot be replicated. Replication is central to science. I find Dr. Jones attitude quite difficult to understand, and I find your refusal to provide the data requested quite baffling.
>
> You are making the rather curious claim that because the data "appears" to be out on the web somewhere, there is no need for Dr. Jones to reveal which stations were actually used. The claim is even more baffling since you say that the original data used by CRU is available at the GHCN web site, and then follow that with the statement that some of the GHCN data originally came from CRU. Which is the case? Did CRU get the data from GHCN, or did GHCN get the data from CRU?
>
> Rather than immediately appealing this ruling (with the consequent negative publicity that would inevitably accrue to CRU from such an action), I am again requesting that you provide:

[364] *Id.*

> 1) A list of the actual sites used by Dr. Jones in the preparation of the HadCRUT3 dataset, and
>
> 2) A clear indication of where the data for each site is available. This is quite important, as there are significant differences between the versions of each site's data at e.g. GHCN and NCAR.
>
> I find it somewhat disquieting that an FOI request is necessary to force a scientist to reveal the data used in his publicly funded research … is this truly the standard that the CRU is promulgating?
>
> Thank you for your cooperation in this matter.[365]

On April 12, 2007, Eschenbach received his second reply, which again referred him to general temperature records without identifying which of these records was actually used. It also referred to some data that CRU had but which it believed that it was contractually prohibited from producing:

> In regards the "gridded network" stations, I have been informed that the Climate Research Unit's (CRU) monthly mean surface temperature dataset has been constructed principally from data available on the two websites identified in my letter of 12 March 2007. Our estimate is that more than 98% of the CRU data are on these sites.
>
> The remaining 2% of data that is not in the websites consists of data CRU has collected from National Met Services (NMSs) in many countries of the world. In gaining access to these NMS data, we have signed agreements with many NMSs not to pass on the raw station data, but the NMSs concerned are happy for us to use the data in our gridding, and these station data are included in our gridded products, which are available from the CRU web site. These NMS-supplied data may only form a very small percentage of the database, but we have to respect their wishes and therefore this information would be exempt from disclosure under FOIA pursuant to s.41. The World Meteorological Organization has a list of all NMSs.[366]

Eschenbach was left with no choice but to reiterate his request and rationale:

---

[365] *Id.* (emphasis added).

[366] *Id.*

While it is good to know that the data is available at those two web sites, that information is useless without a list of stations used by Jones et al. to prepare the HadCRUT3 dataset. As I said in my request, I am asking for:

1) A list of the actual sites used by Dr. Jones in the preparation of the HadCRUT3 dataset, and

2) A clear indication of where the data for each site is available. This is quite important, as there are significant differences between the versions of each site's data at e.g. GHCN and NCAR.

Without knowing the name and WMO number of each site and the location of the source data (NCAR, GHCN, or National Met Service), it is not possible to access the information. Thus, Exemption 21 does not apply – I still cannot access the data.

I don't understand why this is so hard. All I am asking for is a simple list of the sites and where each site's data is located. Pointing at two huge piles of data and saying, in effect, "The data is in there somewhere" does not help at all.

To clarify what I am requesting, I am only asking for a list of the stations used in HadCRUT3, a list that would look like this:

WMO# Name Source

58457 HangZhou NCAR

58659 WenZhou NCAR

59316 ShanTou GHCN

57516 ChongQing NMS

etc. for all of the stations used to prepare the HadCRUT3 temperature data.

That is the information requested, and it is not available "on non-UEA websites", or anywhere else that I have been able to find.

I appreciate all of your assistance in this matter, and I trust we can get it resolved satisfactorily.[367]

[367] *Id.* (emphasis added).

After receiving another communication in which CRU now took the position that it could not identify the locations of the requested information, Eschenbach wrote back:

> It appears we have gone full circle here, and ended up back where we started.
>
> I had originally asked for the raw station data used to produce the HadCRUT3 dataset to be posted up on the UEA website, or made available in some other form.
>
> You refused, saying that the information was available elsewhere on non-UEA websites, which is a valid reason for FOI refusals. I can report that the information requested is not available on non-UEA websites as detailed below.
>
> Your most recent letter (Further _information_ letter_final_ 070418_rev01. doc), however, says that you are unable to identify the locations of the requested information. Thus, the original reason for refusing to provide station data for HadCRUT3 was invalid.
>
> Therefore, since the information requested is not available on non-UEA websites, I wish to re-instate my original request, that the information itself be made available on your website or in some other form. I understand that a small amount of this data (about 2%, according to your letter) is not available due to privacy requests from the countries involved. In that case, a listing of which stations this applies to will suffice.
>
> The HadCRUT3 dataset is one of the fundamental datasets in the current climate discussion. As such, it is vitally important that it can be peer reviewed and examined to verify its accuracy. The only way this can be done is for the data to be made available to other researchers in the field.
>
> Once again, thank you for your assistance in all of this. It is truly not a difficult request, and is fully in line with both standard scientific practice and your "CODE OF PRACTICE FOR RESPONDING TO REQUESTS FOR INFORMATION UNDER THE FREEDOM OF INFORMATION ACT 2000". I am sure that we can bring this to a satisfactory resolution without involving appeals or unfavorable publicity.[368]

[368] *Id.*

CRU responded on April 27, 2007 confirming that it was unable to identify the temperature station records it used in the HadCRUT3 and that, even if could, it could not identify the raw data that was actually used from those stations. For the first time, CRU admitted that it may have had the original data but destroyed it:

> Further to your email of 14 April 2007 in which you re-stated your request to see
>
> "a list of stations used by Jones et al. to prepare the HadCRUT3 dataset" I am asking for: 1) A list of the actual sites used by Dr. Jones in the preparation of the HadCRUT3 dataset, and 2) A clear indication of where the data for each site is available. This is quite important, as there are significant differences between the versions of each site's data at e.g. GHCN and NCAR."
>
> In your note you also requested "the name and WMO number of each site and the location of the source data (NCAR, GHCN, or National Met Service)",
>
> I have contacted Dr. Jones and can update you on our efforts to resolve this matter.
>
> We cannot produce a simple list with this format and with the information you described in your note of 14 April. *Firstly, we do not have a list consisting solely of the sites we currently use. Our list is larger, as it includes data not used due to incomplete reference periods, for example. Additionally, even if we were able to create such a list we would not be able to link the sites with sources of data. The station database has evolved over time and the Climate Research Unit was not able to keep multiple versions of it as stations were added, amended and deleted. This was a consequence of a lack of data storage in the 1980s and early 1990s compared to what we have at our disposal currently. It is also likely that quite a few stations consist of a mixture of sources.*
>
> I have also been informed that, as the GHCN and NCAR are merely databases, the ultimate source of all data is the respective NMS in the country where the station is located. Even GHCN and NCAR can't say with precision where they got their data from as the data comes not only from each NMS, but also comes from scientists in each reporting country.

> *In short, we simply don't have what you are requesting.* The only true source would be the NMS for each reporting country. We can, however, send a list of all stations used, but without sources. This would include locations, names and lengths of record, although the latter are no guide as to the completeness of the series.
>
> This is, in effect, our final attempt to resolve this matter informally. If this response is not to your satisfaction, I will initiate the second stage of our internal complaint process and will advise you of progress and outcome as appropriate. For your information, the complaint process is within our Code of Practice and can be found at: http://www1. uea.ac.uk/ polopoly_ fs/1.2750! uea_manual_ draft_04b. pdf.[369]

Eschenbach decided to appeal, and received the following response asserting that CRU does not maintain records of the raw temperature data it uses nor can it identify such records in third-party sources other than to refer Eschenbach to the entire universe of temperature records from which CRU selected its data:

> Following David Palmer's letter of 27th April 2007 to you regarding your dissatisfaction with our response to your FOI request of 25th January 2007, I have undertaken a thorough review of the contents of our file and have spoken with both Mr. Palmer and Professor Jones.
>
> As a result of this investigation, I am satisfied that we have done all we can to fulfil your request and to provide you with the information you require where it is possible for us to do so. I confirm that we are able to make available on the Climatic Research Unit website a list of stations, including name, latitude, longitude, elevation and WMO number (where available).
>
> We are unable to provide a simple list of sources for these stations as we do not hold this information. Nor do we hold the raw (i.e. unadjusted) station data, as you describe it, at UEA. As stated in prior letters to you, raw station data are available on the NCAR and GHCN websites and gridded data are available on the Climatic Research Unit website. If these data are insufficient for your requirements, you will need to contact the NMS for the country in which the station is located to obtain the information you require.

[369] *Id.* (emphasis added).

> I hope you are able to accept this response. We have contacted the Information Commissioner's Office in relation to this matter and their advice is that if you are still dissatisfied with this response, you can, at this time, exercise your right of appeal to the Information Commissioner by contacting them at:
>
> Information Commissioner's Office
>
> Wycliffe House[370]

Eschenbach evidently decided to drop the matter at this point, and so the amazing revelation that CRU could not identify the specific raw temperature data that it had adjusted in producing HadCRUT3 – and had even destroyed at least some of the underlying data – was not publicized. But the revelation did become public at the end of last year. Roger Pielke Jr., a respected professor of environmental studies at the University of Colorado, requested the raw data from Jones. Jones responded:

> Since the 1980s, we have merged the data we have received into existing series or begun new ones, so it is impossible to say if all stations within a particular country or if all of an individual record should be freely available. *Data storage availability in the 1980s meant that we were not able to keep the multiple sources for some sites, only the station series after adjustment for homogeneity issues. We, therefore, do not hold the original raw data but only the value-added (i.e., quality controlled and homogenized) data.*[371]

The loss of the underlying data was reported on November 29, 2009 in the London Sunday Times, which aptly reported:

> Scientists at the University of East Anglia (UEA) have admitted throwing away much of the raw temperature data on which their predictions of global warming are based.

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[370] *Id.*

[371] Roger Pielke Jr., *We Lost the Original Data,* http://rogerpielkejr.blogspot.com/2009/08/we-lost-original-data.html (Aug. 12, 2009).

> It means that other academics are not able to check basic calculations said to show a long-term rise in temperature over the past 150 years.
>
> The UEA's Climatic Research Unit (CRU) was forced to reveal the loss following requests for the data under Freedom of Information legislation.
>
> The data were gathered from weather stations around the world and then adjusted to take account of variables in the way they were collected. The revised figures were kept, but the originals — stored on paper and magnetic tape — were dumped to save space when the CRU moved to a new building. . .
>
> Roger Pielke, professor of environmental studies at Colorado University, discovered data had been lost when he asked for original records. "The CRU is basically saying, 'Trust us'. So much for settling questions and resolving debates with science," he said.[372]

F. Initial Obstruction and then Compelled Disclosure of Underlying Data on the Asserted Tropical "Fingerprint"

As discussed above at section V(A)(2), one of the key pieces of evidence on which EPA relied to attribute climate change to anthropogenic GHG emissions is a paper by Santer et al, *Consistency of modeled and observed temperature trends in the tropical troposphere*, (Int J Climatology, 2008), that purports to show that the tropical troposphere is warming as predicted by climate models – thereby producing a "fingerprint" of anthropogenic warming. The paper responded to a paper by Douglass et al. that contested that theory as set forth in a paper by Karl et al. As also discussed above, Santer and others were involved in an inappropriate scheme to delay publication of the Douglass paper and expedite publication of the Santer et al. paper so that the two papers would be published together, with Santer getting the last word.

[372] Jonathan Leake, *Climate Change Data Dumped,* THE SUNDAY TIMES 5 (Nov. 3, 2009).

In addition to that scheme, once the Santer et al. paper was published, Santer attempted to stonewall efforts by McIntyre to obtain data that was used in the paper to show the existence of the tropical fingerprint. These attempts at stonewalling even included resistance to FOIA requests, even though Santer is employed by the Lawrence Livermore National Laboratory, an entity within the Department of Energy and therefore subject to the U.S. FOIA.

McIntyre started reasonably with a simple request to Santer for the modeled (or "derived") data that Santer had used in his paper. In attempting to show that, in fact, the tropical troposphere was warming as predicted by models, Santer et al. had made adjustments to raw data. McIntyre sought the data as adjusted. Obviously, the adjustments that Santer made to the raw data were critical to understand because, as seen, the point of the study was to show that, based on these adjustments, the temperatures in the tropical troposphere would indeed align with model predictions. Since the adjusted data had obviously been used in the study, they would have been readily available to Santer:

> Dear Dr Santer,
>
> Could you please provide me either with the monthly model data … used for the statistical analysis in Santer et al 2008, or a link to a URL. I understand that your version has been collated from the [Program for Climate Model Diagnosis and Intercomparison]; my interest is in a file of the data as you used it (I presume that the monthly data used for statistics is about 1–2 MB).
>
> Thank you for your attention,
>
> Steve McIntyre[373]

[373] CRU email 1225462391.txt (Oct. 31, 2008).

Typically, instead of simply providing the data, Santer decided to stonewall the request, and in doing so revealed that he had refused a similar request from Douglass, even though his paper rebutted a paper by Douglass. Santer told his colleagues:

> Dear folks,
>
> While on travel in Hawaii, I received a request from Steven McIntyre for all of the model data used in our IJC paper (see forwarded email). After some conversation with my PCMDI colleagues, I have decided not to respond to McIntyre's request. If McIntyre repeats his request, I will provide him with the same answer that I gave to David Douglass …[374]

Following a second request by McIntyre, Santer told him that he would not provide the modeled data used in the paper even though the data were obviously available to Santer and that McIntyre could try to replicate the data if he could:

> Dear Mr. McIntyre,
>
> I gather that your intent is to "audit" the findings of our recently-published paper in the International Journal of Climatology (IJoC). … You should have no problem in accessing exactly the same model and observational data sets that we employed. You will need to do a little work in order to calculate synthetic Microwave Sounding Unit (MSU) temperatures from climate model atmospheric temperature information. This should not pose any difficulties for you. Algorithms for calculating synthetic MSU temperatures have been published by ourselves and others in the peer-reviewed literature. You will also need to calculate spatially-averaged temperature changes from the gridded model and observational data. Again, that should not be too taxing.
>
> In summary, you have access to all the raw information that you require in order to determine whether the conclusions reached in our IJoC paper are sound or unsound. I see no reason why I should do your work for you, and provide you with derived quantities (zonal means, synthetic MSU temperatures, etc.) which you can easily compute yourself.

[374] *Id.*

> I am copying this email to all co-authors of the 2008 Santer and coworkers IJoC paper, as well as to Professor Glenn McGregor at IJoC. I gather that you have appointed yourself as an independent arbiter of the appropriate use of statistical tools in climate research…
>
> Please do not communicate with me in the future.[375]

Rather than trying to guess how Santer had produced the modeled data, McIntyre filed a FOIA request. McIntyre sought "any monthly time series of output from any of the 47 climate models sent by Santer and/or other coauthors of Santer et al.2008 to NOAA employees between 2006 and October 2008" and "any correspondence concerning these monthly time series between Santer and/or other coauthors of Santer et al.2008 and NOAA employees between 2006 and October 2008." McIntyre's appeal for data evidently enraged Santer, and he wrote to Tom Wigley, who had informed Santer about the FOIA request, that the FOIA request should be denied evidently for no better reason than the identity of the requester:

> Thanks for your email regarding Steven McIntyre's twin requests under the Freedom of Information (FOI) Act...
>
> My personal opinion is that both FOI requests (1) and (2) are intrusive and unreasonable. Steven McIntyre provides absolutely no scientific justification or explanation for such requests. I believe that McIntyre is pursuing a calculated strategy to divert my attention and focus away from research. As the recent experiences of Mike Mann and Phil Jones have shown, this request is the thin edge of wedge. It will be followed by further requests for computer programs, additional material and explanations, etc., etc....
>
> I believe that our community should no longer tolerate the behavior of Mr. McIntyre and his cronies. McIntyre has no interest in improving our scientific understanding of the nature and causes of climate change. He has no interest in rational scientific discourse. He deals in the currency of threats and intimidation. We should be able to conduct our scientific research without constant fear of an "audit" by Steven McIntyre; without having to weigh

[375] CRU email 1226337052.txt (Nov. 10, 2008).

> every word we write in every email we send to our scientific colleagues.
>
> In my opinion, Steven McIntyre is the self-appointed Joe McCarthy of climate science. I am unwilling to submit to this McCarthy-style investigation of my scientific research. *As you know, I have refused to send McIntyre the "derived" model data he requests, since all of the primary model data necessary to replicate our results are freely available to him. I will continue to refuse such data requests in the future. Nor will I provide McIntyre with computer programs, email correspondence, etc.* I feel very strongly about these issues. We should not be coerced by the scientific equivalent of a playground bully.[376]

Santer's hostility to McIntyre, however, is not a basis to refuse a FOIA request. Nor is it engaging in "McCarthy-style" tactics to request that the authors of an extremely important study produce the modeled data that formed the basis of the study, particularly when the lead author works for the federal government.

Wigley then responded to Santer and expressed support for the notion that FOIA should not be deemed to cover information, like the modeled data sets that McIntyre sought, even though such information would be readily available in files subject to FOIA:

> Joking aside, it seems as a matter of principle (albeit a principle yet to be set by the courts) that provision of primary data sources that are sufficient to reproduce the results of a scientific analysis is all that is necessary under FOI.
> It also seems that judgment of what correspondence is central to the analysis can only be made by the persons involved. As a participant in many of these inter-author communications, I do not recall any that would give information not already contained in the published paper. [377]

By now, however, in the fall of 2008, because the use of FOIA to induce these scientists to produce their data had now involved independent government officials, the group was no

[376] CRU email 1226451442.txt (Nov. 11, 2008).

[377] CRU email 1226456830.txt (Nov. 11, 2008).

longer able to cast a shroud over its research. Santer's response to Wigley indicated that he had been told that information in his files, including emails, were subject to production under FOIA no matter who makes the request and no matter what the interests are of the person making the request:

> I'd be very happy to discuss these issues with you tomorrow. I'm sorry that the tone of this letter is so formal, Tom. Unfortunately, after today's events, I must assume that any email I write to you may be subject to FOI requests, and could ultimately appear on McIntyre's "ClimateAudit" website.[378]

Indeed, Santer was forced to admit to his colleagues that his obstructionist attitude was causing embarrassment to the Department of Energy:

> Dear folks,
>
> There has been some additional fallout from the publication of our paper in the International Journal of Climatology. After reading Steven McIntyre's discussion of our paper on climateaudit.com (and reading about my failure to provide McIntyre with the data he requested), an official at DOEnergy headquarters has written to Cherry Murray at LLNL, claiming that my behavior is bringing LLNL's good name into disrepute. Cherry is the Principal Associate Director for Science and Technology at LLNL, and reports to LLNL's Director (George Miller).[379]

Santer was then forced to release the data that McIntyre asked for. On December 18, 2008, he wrote his colleagues:

> I just wanted to alert you to the fact that Steven McIntyre has now made a request to U.S. DOE Headquarters under the Freedom of Information Act (FOIA). … I was made aware of the FOIA request earlier this morning.
>
> McIntyre's request eventually reached the U.S. DOE National Nuclear Security Administration (NNSA), Livermore Site Office.

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[378] CRU email 1226451442.txt (Nov. 11, 2008).

[379] CRU email 1228249747.txt (Dec. 2, 2008).

> The requested records are to be provided to the "FOIA Point of Contact" (presumably at NNSA) by December 22, 2008. . . .
>
> Over the past several weeks, I've had a number of discussions about the "FOIA issue" with the PCMDI's Director (Dave Bader), with other LLNL colleagues, and with colleagues outside of the Lab. Based on these discussions, I have decided to "publish" all of the climate model surface temperature time series and synthetic MSU time series that we used in our International Journal of Climatology (IJoC) paper. This will involve putting these data sets through an internal "Review and Release" procedure, and then placing the data sets on the PCMDI's publicly-accessible website. The website will also provide information on how synthetic Microwave Sounding Unit (MSU) temperatures were calculated, anomaly definition, analysis periods, etc.
>
> After publication of the model data, we will inform the "FOIA Point of Contact" that the information requested by McIntyre is publicly available for bona fide scientific research. Unfortunately, we cannot guard against intentional or unintentional misuse of these data sets by McIntyre or others. . . .
>
> I hope that "publication" of the synthetic MSU temperatures resolves this matter to the satisfaction of the NNSA, DOE Headquarters, and LLNL.[380]

Ironically, it was then left to Wigley to tell Santer that having the data available is useful to the scientific method:

> This is a good idea. … ***To have these numbers on line would be of great benefit to the community. In other words, although prompted by McIntyre's request, you will actually be giving something to everyone***.[381]

Left unexplained in Wigley's email is why – if disclosing the modeled data "would be of great benefit to the scientific community – Santer could not have just released his data when McIntyre first requested it rather than attempting to stonewall it. In any event, the group's ability to stonewall information requests was now at an end.

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[380] CRU email 1229468467.txt (Dec. 16, 2008).

[381] *Id.*

G. Under Pressure, the Group Finally Realizes that Their Refusals to Disclose Data Are Contrary to Good Scientific Practice

In the fall of last year, as a result of McIntyre's requests for data, the authors of a paper published in Science were forced to admit that they had flipped a data set upside down. In a revealing email, Overpeck explained exactly why McIntyre should be provided with underlying information, in words that repudiate the almost decade-long effort to hide the data. Perhaps his candor resulted from the fact that he now realized his emails might be subject to public disclosure:

> D et al.—Please write all emails as though they will be made public.
>
> I would not rush and I would not respond to any of them until the best strategy is developed—don't want to waste anyone's time, including yours or Mc's. Since the recon in Science has an error, I think you do need to publish a correction in Science. … I don't think you have a choice here. . . .
>
> # 5 is tricky. Giving him the data would be good, but only if it is yours to give. You can't give him data that you got from others and are not allowed to share. ***But it would be nice if he could have access to all the data that we used—that's the way science is supposed to work.***[382]

Wigley was forced into a similar admission after McIntyre raised questions about data sets used in Briffa's proxy temperature reconstructions and Briffa, at least initially, declines to respond to McIntyre's requests for information. As Wigley wrote last fall:

> … Keith does seem to have got himself into a mess. …
> And the issue of with-holding data is still a hot potato, one that affects both you and Keith (and Mann). ***Yes, there are reasons—but many *good* scientists appear to be unsympathetic to these.*** The trouble here is that with-holding data looks like hiding something, and hiding means (in some eyes) that it is bogus science that is being hidden.[383]

[382] CRU email 1252164302.txt (Sept. 5, 2009) (emphasis added).

[383] CRU email 1254756944.txt (Oct. 5, 2009).

Indeed, "many good scientists" had compelling reasons to be concerned about the long effort to stonewall legitimate information requests – and so should EPA. As John Beddington, the British government chief scientific adviser, recently said, "I think, wherever possible, we should try to ensure there is openness and that source material is available for the whole scientific community."[384]

H. The Actions of these Scientists in Refusing to Disclose Data and Information Used in their Studies and Otherwise of Relevance to Public Discourse on Scientific Issues Flatly Contradicts EPA and the Administration's Commitment to Transparency and Openness

Immediately upon taking office, the Administrator promised that all science used by the Agency would be subject to rigorous standards of openness and transparency. In a January 23, 2009 memorandum to EPA employees, the Administrator stated that:

> EPA's actions must be transparent. In 1983, EPA Administrator Ruckelshaus promised that EPA would operate "in a fishbowl" and "will attempt to communicate with everyone from the environmentalists to those we regulate, and we will do so as openly as possible."
>
> I embrace this philosophy. Public trust in the Agency demands that we reach out to all stakeholders fairly and impartially, that we consider the views and data presented carefully and objectively, *and that we fully disclose the information that forms the bases for our decisions. I pledge that we will carry out the work of the Agency in public view so that the door is open to all interested parties and that there is no doubt why we are acting and how we arrived at our decisions.* [385]

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[384] As quoted in Ben Webster, *Britain's chief scientist John Beddington calls for engagement with climate skeptics,* THE TIMES, Jan. 27, 2010, *available at* http://www.theaustralian.com.au/news/britains-chief-scientist-john-beddington-calls-for-engagement-with-climate-sceptics/story-e6frg6xf-1225823874671.

[385] Memorandum from EPA Administrator to EPA employees (Jan. 23, 2009) *available at* http://www.epa.gov/Administrator/memotoemployees.html (emphasis added).

In response to a March 9, 2009 Presidential Memorandum for the Heads of Executive Departments, the Administrator reiterated that:

> The President's Memorandum stresses that "scientific information … developed and used by the Federal government should … ordinarily be made available to the public" and that, where permitted by law, "there should be transparency in the preparation, identification and use of scientific and technological information in policymaking." *Consistent with this principle and my commitment to transparency, I believe that the methodologies and guidelines that EPA uses for scientific analyses should be shared fully with the public.*[386]

The obfuscations and stonewalling described above clearly do not reflect the same commitment to openness and transparency. Early on, the paleoclimate community decided that it should not be required to divulge underlying data and information, particularly to those who might use the information to critique their findings. As time went on, they decided that McIntyre and anyone associated with him was essentially their enemy and that they would refuse to cooperate entirely with any effort to provide him with information, including under the U.S. and British FOIAs. Most troubling, this stonewalling even included destruction of information pertaining to the drafting of AR4.

The issue here is not just whether U.S. and British FOIA require disclosure in a certain situation. It is whether scientists, especially government-funded scientists involved in hugely important scientific research, are fully willing to make available for public scrutiny all of the information they use and produce, no matter the political, policy or scientific consequence. These scientists shared no such commitment. Accordingly, since the Administrator has directed

[386] Memorandum for the Heads of Executive Departments and Agencies**,** from President, Barack Obama on Scientific Integrity (Mar. 9, 2009) (emphasis added) *available at* http://www.whitehouse.gov/the_press_office/Memorandum-for-the-Heads-of-Executive-Departments-and-Agencies-3-9-09 (emphasis added).

that her agency will only rely on science to the extent that it has been produced in an open fashion, she cannot rely on the work of these scientists as reflected in the reports of the IPCC.

VII.

IMPROPER EDITORIAL AND PEER-REVIEW PRACTICES WERE USED IN DEVELOPING IPCC REPORTS, CONTRARY TO EPA INFORMATION QUALITY ACT REQUIREMENTS

"It related to several countries in this region and their water sources. We thought that if we can highlight it, it will impact policy-makers and politicians and encourage them to take some concrete action. It had importance for the region, so we thought we should put it in."

> Dr. Murari Lal, AR4 Coordinating Lead Author, Chapter 10 of WGII Report on Impacts, Adaptation and Vulnerability, referring to how misinformation about the pace of Himalayan glacier melt was included in the report, as reported in the Daily Mall, January 24, 2010

As a result of the CRU material and other recent revelations, numerous instances are known of improper editorial and peer-review processes used during preparation of the IPCC reports. These improper practices led to outright factual errors and to a biased and overstated view of the science. As Dr. Lal's candid statement above and the information cited in this section of our Petition makes clear, these practices were not mistakes and reflect an advocacy-driven agenda.

A. Rejection of Literature that Disagreed with Lead Authors' Views

IPCC lead authors evidently had no compunction in refusing to include in AR4 peer-reviewed literature that conflicted with their own scientific views. We described one example above, where AR4 Chapter 6 co-author Jansen refused to include references to two journal articles that disagreed with his own view that orbital oscillations caused early-Holocene warming.

Another example is AR4 Chapter 3 co-author Jones' treatment of a McKitrick and Michaels paper published in 2004 by Climate Research that presented an alternative theory for

recent warming.[387] Jones first tried to prevent the paper from being included in the chapter, and when that attempt failed because of publication of a second paper reaching a similar conclusion, he summarily dismissed both papers in the Chapter 3 text.

The McKitrick and Michaels paper was one of two papers to which Jones referred in a 2004 "HIGHLY CONFIDENTIAL" email to Mann, in which he stated "I can't see either of these papers being in the next IPCC report. Kevin and I will keep them out somehow - even if we have to redefine what the peer-review literature is !"[388] The Kevin that Jones is referring to is Kevin Trenberth. Jones and Trenberth were the two coordinating lead authors of Chapter 3 and so in a position to carry out Jones' undertaking.

Trenberth and Jones acted on their promise by not including the paper in the First Order Draft. Although McKitrick and Vincent Grey (another reviewer) objected to such exclusion, Trenberth and Jones flatly rejected their comments:

> References are plentiful. Those of value are cited Rejected. The locations of socioeconomic development happen to have coincided with maximum warming, not for the reason given by McKitrick and Michaels (2004) but because of the strengthening of the Arctic Oscillation and the greater sensitivity of land than ocean to greenhouse forcing owing to the smaller thermal capacity of land.[389]

This comment does not cite any peer-reviewed literature for its assertion, and so far as Peabody is aware, there has yet to be any such literature published that supports Trenberth and Jones' comment.

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[387] Ross McKitrick and Patrick Michaels, *A test of corrections for extraneous signals in gridded surface temperature data,* 26 CLIM. RES. 159 (2004), *available at* http://www.voguelph.ca/~mckitri/research/McKitrick-Michaels-CR.04.pdf.

[388] CRU email 1089318616.txt (Jul. 8, 2004).

[389] AR4 First Order Draft Review Comments. Chapter 3 at 7.

Trenberth and Jones were also successful in keeping Michaels and McKitrick (2004) out of the Second Order IPCC Draft. Once again, as IPCC peer reviewers, McKitrick and Grey objected, and once again, Trenberth and Jones wrote the same rejection comments as above. Trenberth and Jones did add at one point: "[r]ejected. McKitrick and Michaels (2004) is full of errors. There are many more papers in support of the statement than against it."[390] This is not logical, for science does not operate based on majority rule. Also, Jones and Trenberth failed again to cite any peer-reviewed literature to support their substantive basis for rejecting the paper. The fact that Jones had already stated that he would do whatever it takes to keep the paper out of AR4 – even though the paper was published in a peer-reviewed journal – and Jones' outright animoisity to those who disagree with him, as set forth in great detail above, calls into question the legitimacy of their asserted basis for rejection.

Although Jones and Trenberth successfully kept McKitrick and Michaels out of the First and Second Order draft of AR4, Jones and Trenberth could no longer sustain that position after an article by A. T. J. De Laat and A.N. Maurelis was published in 2006 reaching similar conclusions as McKitrick and Michaels.[391] Jones and Trenberth eventually did acknowledge the existence of both the McKitrick and Michaels 2004 paper and the De Laat and Maurelis 2006 paper in AR4. Chapter 3 of AR4, however, dismissed the two papers as follows:

> McKitrick and Michaels (2004) and De Laat and Maurellis (2006) attempted to demonstrate that geographical patterns of warming trends over land are strongly correlated with geographical patterns of industrial and socioeconomic development, implying that urbanization and related land surface changes have caused much of the observed warming. However, the locations of greatest socioeconomic development are also those that have been most

[390] AR4 Second Order Draft Review Comments. Chapter 3 at 21.

[391] A. T. J. DeLaat and A. N. Maurellis, *Evidence for Influence of Anthropogenic Surface Processes on Lower Tropospheric and Surface Temperature Trends,* 26 INT. J. CLIMATOL. 897 (2006).

> warmed by atmospheric circulation changes (Sections 3.2.2.7 and 3.6.4), which exhibit large-scale coherence. Hence, the correlation of warming with industrial and socioeconomic development ceases to be statistically significant. In addition, observed warming has been, and transient greenhouse-induced warming is expected to be, greater over land than over the oceans (Chapter 10), owing to the smaller thermal capacity of the land.[392]

This negative portrayal of both papers lacks any citation to peer-reviewed literature, and the discussion was not itself submitted to external IPCC peer reviewers. Also, McKitrick and Michaels had no previous opportunity to reply to Jones and Trenberth. Further, despite the IPCC claim, via Jones and Trenberth, that the results "cease to be statistically significant," no support is provided for this claim. It is thus apparent that Jones and Trenberth made their own rules when it came to the treatment of literature that did not reflect their own theories on climate change. Such literature was either excluded, or if that strategy did not work, the literature was summarily dismissed based on the coordinating lead authors' own views.[393]

B. IPCC Authors Acting as Reviewers

An IPCC chapter author should not peer review journal articles that he intends to include in his chapter because of the clear conflict of interest. The chapter author is supposed to be providing a neutral summary of the science, not advancing his own view of the science. If the chapter author reviews papers that are then included in his chapter, he could be seen as advancing his own agenda both in his review of the paper and its selection for inclusion in the IPCC report.

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[392] AR4 at § 3.2.2.1.

[393] McKitrick and Michaels later submitted what Jones calls the "expanded" version of the paper to the International Journal of Climatology in May 2004. Andrew Comrie, of the University of Arizona, was assigned to review that paper, but then Comrie asked Jones to review the paper which almost certainly assured the paper's demise. Jones told Mann that Comrie asked him to find two other reviewers and that all three reviewers flatly rejected the paper. CRU email 1092418712.txt (Aug. 13, 2004). Subsequently the paper received a formal rejection from the journal.

Jones realized that he should not act as a reviewer of papers that he intended to rely on as an IPCC author but did so anyway. In a July 2005 email, he revealed precisely why authors should not be reviewers:

> Now to your email. I have seen the latest Mears and Wentz paper (to Science), but am not reviewing it, thank goodness. I am reviewing a couple of papers on extremes, *so that I can refer to them in the chapter for AR4. Somewhat circular, but I kept to my usual standards...*
>
> IPCC, me and whoever will get accused of being political, whatever we do. As you know, I'm not political. If anything, I would like to see the climate change happen, so the science could be proved right, regardless of the consequences. This isn't being political, it is being selfish.[394]

In other words, Jones reviewed papers so that he could get them published and therefore could refer to them in AR4 Chapter 3. As Jones says, this is indeed "circular" and violates his supposed neutrality both as an AR4 author and as a reviewer of the paper. Given Jones' deep involvement in the abuses described throughout this Petition, Jones' undertaking to abide by his "usual standards" is not comforting.

Other examples exist of Jones acting as a reviewer for the purpose of creating science that could then be included in AR4 Chapter 3. In 2005, Stephen Schneider, editor of the journal Climatic Change, invited Jones to be a reviewer of a paper by Caspar Ammann and Eugene Wahl. This paper was vital to AR4 because it claimed to recreate Mann's hockey stick. Since the hockey stick was under attack, Jones was interested in supporting it for use in AR4 and therefore accepted the offer to do the review for the Ammann and Wahl paper.[395] (The extent to which Jones, Mann and others went to get this paper published in time for use in AR4 is

[394] CRU email 1120593115.txt (Jul. 5, 2005) (emphasis added).

[395] CRU email 1116017259.txt (May 13, 2005).

described in more detail below at section VII (D). A week later, Jones told Mann, Ammann's former teacher and mentor, that he liked the paper and that it will make preparation of IPCC AR4 chapter on Paleoclimate easier:

> Just reviewed Caspar's paper with Wahl for Climatic Change. Looks pretty good. Almost reproduced your series and shows where MM have gone wrong. Should keep them quiet for a while. Also they release all the data and the R software. Presume you know all about this. Should make Keith's life in Ch 6 easy![396]

One wonders just how unbiased Jones' review of this paper really was, particularly in light of Jones' admission that he applied less than stringent review standards to papers that he favored.[397].

C. Contributing Authors Not Identified

Mann and Crowley made significant contributions to the preparation of AR4 Chapter 6, but neither was included in the list of "contributing authors." Perhaps the reason they were not identified as authors is because they were IPCC reviewers, and authors obviously should not act simultaneously as reviewers.

In a 2004 email, Briffa, an AR4 Chapter 6 lead author, asked Overpeck, an AR4 Chapter 6 coordinating lead author, if he could involve Mann in the preparation of Chapter 6 by saying "Peck , I would still rather have Mike Mann in, so what is the story here - can I ask him?"[398] This was apparently approved, for later there is a response from Mann to Briffa in which Mann indicated he will assume both roles as a reviewer and a contributor, although he seemed to recognize the conflict:

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[396] CRU email 1116611126.txt (May 20, 2005).

[397] *See* below at section VIII (A)(2).

[398] CRU email 1097540855.txt (Oct. 11, 2004).

> Thanks again for your phone call, and the (informal) opportunity to help out where I can. I'm perfectly happy in that role (as an informal contributor and a formal reviewer, for example), if you and Peck, for example, are both comfortable with that.[399]

Mann then provided Briffa with a great deal of information to be used in AR4 from the RealClimate website that he and others maintain. That website attacks viewpoints contrary to his own:

> First, "RealClimate" should be helpful. It deals w/ the skeptic claims, etc. but using the Legitimate peer-reviewed research as a basis for the discussion. The "hockey stick" overview should be helpful:
>
> [1]http://www.realclimate.org/index.php?p=7
>
> as well as itemized esponses to the various contrarian propaganda/myths:
>
> [2]http://www.realclimate.org/index.php?p=11
>
> and the specific discrediting of the claims of McIntyre and McKitrick, based both on our response to their rejected Nature comment:
>
> [3]http://www.realclimate.org/index.php?p=8
>
> and the discussion of the analysis in the Rutherford et al (2004) paper in press in Journal of Climate, that independently discredits them:
>
> [4]http://www.realclimate.org/index.php?p=10
>
> In the following emails, I'll attach some other materials (submitted papers) that deal w/the McIntyre and Mckitrick matter, and the von Storch matter, Please let me know if there is anything we discussed that I forget to provide you. Will also draft an email to the small group (you, me, Scott, Caspar, Gene) about the prospective additional RegEM/Mann et al method model analyses….[400]

Mann was even given the opportunity to comment on and input the drafting of chapters outside the normal review process:

> don't know what the status of the whole chapter is - but I thought I would send this very first and rough draft to you anyway - I have to wait and see the whole thing and hear from Peck before doing

[399] CRU email 1102956436.txt (Dec. 13, 2004).

[400] *Id.*

> more… as I said - really appreciate your input , have a great Christmas and for f..ks sake keep the right priorities to the fore as the years progress [401]

Crowley also was an undisclosed contributing author of AR4. Crowley made a substantial contribution to the special MWP box in the AR4 Chapter 6 (Overpeck's "big hammer").[402] This box attempted to explain why the IPCC decided the MWP was not a significant global event. Crowley was asked to be formally listed as a co-author, but he expressed his discomfort with having his dual role as both a contributor and a reviewer disclosed:

> *I am not sure whether it is wise to add me to the CA [contributing author] list, just because the reviewer is supposed to be impartial and a CA loses that appearance of impartiality if he has now been included as a CA* - may want to check with Susan S. on this one to be sure – still happy to provide advice.[403]

Briffa, however, saw no problem for Crowley and said "[m]y own position on this is that you are an "unofficial" referee, who has (and still is) making a significant contribution - I see no conflict."[404] Crowley then admitted that he had been getting involved in political matters and should not be listed as a contributing author, regardless of his significant contribution:

> Hi all, there *is another reason why I should not be formally listed as an LA* - it is my understanding that IPCC contributors have to be a little careful about getting involved in political matters that could be used to impugn the integrity of the process - well I am starting to do just that, with the attached commen in Eos, plus some radio interviews where I have been somewhat pointed in my thoughts.

[401] CRU email 1103828684.txt (Dec. 23 2004).

[402] CRU email 1118866416.txt (Jun. 15, 2005) ("I have been fiddling with the best way to illustrate the stable nature of the medieval warm period"); *see also* CRU email 1121869083.txt (Jul. 20, 2005).

[403] CRU email 1121869083.txt, (Jul 20, 2005) (emphasis added).

[404] *Id.*

> I suppose its still ok to be a reviewer, but even then you might keep these comments in mind, tom.[405]

Ultimately, Crowley was not identified as a contributing author. But neither his role as a reviewer nor his political activities prevented him from, in fact, contributing.[406]

D. Manipulation of Publication Deadlines

The extent to which the IPCC went to ensure inclusion of papers supporting the views of the principal chapter authors is shown in the history of the Wahl and Ammann (2007) paper discussed above that attempted to rehabilitate the hockey stick. As stated, the paper was one of the key proxy temperature reconstructions relied on in Chapter 6 of AR4.

Wahl and Ammann produced two interrelated papers that were initially intended to be published as Wahl and Ammann in Climatic Change and as Ammann and Wahl in Geophysical Research Letters ("GRL"). For convenience, we refer to these papers as "WA" and "AW" The GRL paper contained much of the statistical analysis on which the Climatic Change paper depended. The interrelationship of the two papers is obvious from a review of the two papers as ultimately published.[407] Indeed, when the two papers where initially submitted, UCAR issued a press release highlighting that Ammann and Wahl's research would be published in two interrelated papers. According to the press release, "Their results appear in two new research

[405] CRU email 1122300990.txt (Jul. 25, 2005) (emphasis added).

[406] Zeroth Order Draft.

[407] Both papers were published in the same edition of Climatic Change in 2007 because they needed to cite to each other. *See* Caspar Ammann and Eugene Wahl, ***The Importance of the Geophysical Context in Statistical Evaluations of Climate Reconstruction Procedures***, 85 CLIM. CHANGE 71 (Aug. 31, 2007) (listed in the References section as published in "this volume"), and Eugene Wahl and Caspar Ammann, ***Robustness of the Mann, Bradley, Hughes Reconstruction of Northern Hemisphere Surface Temperatures: Examination of Criticisms Based on the Nature and Processing of Proxy Climate Evidence***, 85 CLIM. CHANGE 33 (Aug. 31, 2007).

papers submitted for review to the journals Geophysical Research Letters and Climatic Change."[408]

According to the IPCC, papers submitted for inclusion in AR4 had to meet the following dates:

- **May 10-13, 2005** – Literature to be cited will need to be published or available in draft form by this time (the date of the Second Lead Author meeting).
- **Dec 13-16, 2005** – Literature to be cited will need to be published or in press by this time (the date of the Second Lead Author meeting).
- **Late Feb 2006** – the TSU [Technical Support Unit] must hold final preprint copies of any unpublished papers that are cited in order that these can be made available to reviewers...if LA [Lead Authors] cannot assure us that a paper is in press and provide a preprint we will ask them to remove any reference to it.[409]

The WA and AW papers were submitted, respectively, to Climatic Change and GRL on May 10, 2005, at the deadline for when a paper had to be at least "available in draft." However, the papers appeared to miss the December 13-16 deadline for when a paper had to be "published or in press." According to UCAR, the publisher of Climatic Change, the WA paper was "Provisionally Accepted" by Climatic Change on December 12, 2005. The term "provisionally accepted" is not defined by UCAR, but it does not appear to meet the IPCC requirement that a paper has been "published or in press" at the time. A paper that is only provisionally accepted

[408] Press Release, UCAR, Media Advisory: The Hockey Stick Controversy New Analysis Reproduces Graph of Late 20th Century Temperature Rise, (May 11, 2005) *available at* http://www.ucar.edu/news/releases/2005/ammann.shtml.

[409] *See* Working Group I's original timetable, *available at* http://web.archive.org/web/20040708212706/http://ipcc-wg1.ucar.edu/wg1/wg1_timetable.pdf. *See also* Deadlines for literature cited in the Working Group I Fourth Assessment Report, *available at* http://web.archive.org/web/20060430074956/http://ipcc-wg1.ucar.edu/wg1/docs/PublicationDeadlines.pdf.

appears to be one that has not been fully accepted and therefore may be changed. Such a paper is not one that is either published or even in press.[410]

In fact, although the WA paper was "Provisionally Accepted" on December 12, 2005 by Climatic Change, the paper was manifestly not ready for press as required by that deadline. In the first place, the paper was not even published until August 2007, indicating that it had a long way to go before it was ready to be published. Moreover, the paper as published in 2007 cited works not even submitted for publication until 2006, including the AW paper.[411] Although the AW paper was originally submitted to GRL in 2005, that journal rejected it. They then rewrote it and submitted it to Climatic Change, but not until August 22, 2006. The AW paper as eventually published indicated that it was submitted on August 22, 2000, a date that must have been intended to mean August 22, 2006.

On February 28, 2006, the WA paper was accepted for publication by Climatic Change in a transparent attempt to make it seem as if the paper met the final IPCC deadline for providing final preprint copies of papers to the IPCC Technical Support Unit so the papers could be provided to reviewers.[412] But the AW paper on which the WA paper relied was still not even accepted (provisionally or otherwise) for publication at that point, so the WA paper could not at

[410] UCAR may have created the "provisionally accepted" category to ensure that the paper would at least arguably meet the second IPCC deadline. It certainly had an incentive to bend its own rules to favor the Wahl and Ammann paper given the network of interests that were involved. The Technical Support Unit for the IPCC Working Group 1 is administered by UCAR. *See* IPCC's Working Group I's website administered by UCAR, *available at* http://ipcc-wg1.ucar.edu/; *see also* IPCC's Executive Director Vacancy description ("The University Corporation for Atmospheric Research (UCAR) administers the TSU") *available at* http://www.ipcc.ch/pdf/vacancies/ipcc-wg-2-head-tsu-ad.pdf. One of UCAR's primary roles is to manage NCAR. Caspar Ammann works at NCAR, as does Tom Wigley, former head of CRU. *See* Caspar Ammann bio ("Caspar M. Ammann is a Scientist II in the Climate and Global Dynamics Division of NCAR studying past and present climate changes") *available at* http://www.cgd.ucar.edu/ccr/Ammann/Home.html.

[411] *See* References for Wahl and Ammann 2007, *available at* on page 35: http://www.cgd.ucar.edu/ccr/Ammann/millennium/refs/Wahl_ClimChange2007.pdf.

[412] The current version of Wahl and Ammann 2007 has an accepted date of March 1, 2006, so the IPCC was still using a soft deadline for the final publication milestone.

that point have been said to be complete. Certainly, any IPCC reviewer attempting to determine whether the WA paper supported draft IPCC text could not have done so, because the reviewer would not have been able to check the underlying statistical analysis contained in the not-yet-accepted AW paper.

The IPCC then changed its dates for submission of papers that could be included in AR4. After the Lead Author's meeting from June 25-30th, IPCC Working Group 1 sent a July 3, 2006 email to reviewers that stated:

> "To ensure clarity and transparency in determining how such material might be included in the final Working Group I report, the following guidelines will be used by Lead Authors in considering such suggestions. In preparing the final draft of the IPCC Working Group I report, Lead Authors may include scientific papers published in 2006 where, in their judgment, doing so would advance the goal of achieving a balance of scientific views in addressing reviewer comments…Reviewers are invited to submit copies of additional papers that are either in-press or published in 2006… All submissions must be received by the TSU not later than July 24, 2006 and incomplete submissions can not be accepted."[413]

Regardless of the fairness of this mid-stream change in submission dates, even this change should not have saved the WA paper. The AW paper was not accepted for publication as of July 24, 2006 and therefore was not available to reviewers to judge the accuracy of the conclusions of the WA paper. Indeed, neither paper was published until August 2007, more than a year after the July 24, 2006 deadline, and the AW paper was published a week before the WA paper so that the latter paper could cite it. Based on a CRU email from Wahl to Jones and

[413] *See* Guidelines for inclusion of recent scientific literature in the Working Group I Fourth Assessment Report, (emphasis added), *available at* http://web.archive.org/web/20070206012931/ipcc-g1.ucar.edu/wg1/docs/PublicationDeadlines_2006-07-01.pdf.

Ammann, the paper was still being edited as late as September 2007.[414] Even at that point, a portion of the two authors' statistical analysis was contained in Supplementary Information, which was not released until after the papers were published. [415]

In sum, AR4 cited to Wahl and Ammann's paper as support for the hockey stick, for the validity of using long-term proxy temperature reconstructions to compare past climate to current climate, and for the authors' view that 20th century warming was unprecedented. But the IPCC was able to do so only through the manipulation of its own deadlines for receipt of literature.

E. Reliance on Inappropriate Non-Peer Reviewed Secondary Sources Material From Advocacy Groups

The IPCC reports frequently relied on "studies" that were not peer reviewed, that were unscientific, and that were prepared by advocacy groups. We discuss several examples below, one of which recently forced the IPCC to issue an embarrassing retraction. We also note a number of other citations in the AR4 WGII and WGIII reports to the work of advocacy groups. The IPCC's failure to conduct due diligence is fundamentally at odds with IQA peer-review and data-quality standards to which EPA is subject for highly influential scientific information.

1. Himalayan Glaciers

The Working Group II Report of AR4 included a section describing the potential fate of glaciers in the Himalayan region of central Asia, for which the IPCC relied upon on a World Wildlife Fund ("WWF") study as authority. The IPCC evidently did not check the underlying

[414] CRU email 1189722851.txt (Sep. 13, 2007) (correspondence discussing the edits "[t]here were inevitably a few things that needed to be changed in the final version of the WA paper, such as the reference to the GRL paper that was not published (replaced by the AW paper here)... I imagine that MM will make the biggest issue about the very existence of the AW paper, and then the referencing of it in WA; but that was simply something we could not do without").

[415] The Supplementary Information has since been released to the public and is *available at* http://www.cgd.ucar.edu/ ccr/ammann/millennium/MBH-reevaluation.html.

source for the WWF study. That source now contests the WWF's conclusions. As a result, the IPCC has been forced to admit that its conclusions were incorrect as to recession of the Himalayan glaciers. According to a recent IPCC announcement:

> …It has, however, recently come to our attention that a paragraph in the 938 page Working Group II contribution to the underlying assessment[2] refers to poorly substantiated estimates of rate of recession and date for the disappearance of Himalayan glaciers. In drafting the paragraph in question, the clear and well-established standards of evidence, required by the IPCC procedures, were not applied properly.
>
> The Chair, Vice-Chairs, and Co-chairs of the IPCC regret the poor application of well-established IPCC procedures in this instance. This episode demonstrates that the quality of the assessment depends on absolute adherence to the IPCC standards, including thorough review of "the quality and validity of each source before incorporating results from the source into an IPCC Report". We reaffirm our strong commitment to ensuring this level of performance.[416]

Footnote 2 in the IPCC text above reads as follows:

> The text in question is the second paragraph in section 10.6.2 of the Working Group II contribution and a repeat of part of the paragraph in Box TS.6. of the Working Group II Technical Summary of the IPCC Fourth Assessment Report.

The paragraph in question (IPCC AR4 Working Group II Report Chapter 10.6.2, second paragraph, p. 493) describes the glaciers in the Himalayan region as rapidly receding and:

> …if the present rate continues, the likelihood of them disappearing by the year 2035 and perhaps sooner is very high if the Earth keeps warming at the current rate. Its total area will likely shrink from the present 500,000 to 100,000 km2 by the year 2035 (WWF, 2005).

Note the sole citation to the WWF study. Other portions of Chapter 10.6.2 describe how the Himalayan glaciers are important for the water resources and economies of hundreds of

[416] http://www.ipcc.ch/pdf/presentations/himalaya-statement-20january2010.pdf. (internal footnote deleted).

millions of people in the Southern Asia countries. The findings from Chapter 10.6.2 underlie conclusions in other sections of Chapter 10, including Chapter 10.4 and Chapter 10.2, which use them to justify that glacial lake outbursts and avalanches have increased (citing the same WWF, 2005 report) (IPCC AR4 WGII Chapter 10.2, p. 477):

> Glaciers in Asia are melting faster in recent years than before, as reported in Central Asia, Western Mongolia and North-West China, particularly the Zerafshan glacier, the Abramov glacier and the glaciers on the Tibetan Plateau (see Section 10.6.2) (Pu et al., 2004).As a result of rapid melting of glaciers, glacial runoff and frequency of glacial lake outbursts causing mudflows and avalanches have increased (Bhadra, 2002; WWF, 2005).

These findings also appear in the Technical Summary of IPCC AR4 Working Group II (p. 18) as:

> Glacier melt in the Himalayas is projected to increase flooding, and rock avalanches from destabilised slopes, and to affect water resources within the next two to three decades. This will be followed by decreased river flows as the glaciers recede. * N [10.2, 10.4]

And Technical Summary Box TS6, p. 59:

> • If current warming rates are maintained, Himalayan glaciers could decay at very rapid rates, shrinking from the present 500,000 km2 to 100,000 km2 by the 2030s. ** D [10.6.2]

All of these conclusions are based upon the findings in Chapter 10.6.2 which the IPCC has now admitted to be mistaken. As a recent letter in the journal Science stated after examining how the IPCC made the mistake as to the rate of recession of Himalayan glaciers, "[t]hese errors could have been avoided had the norms of scientific publication, including peer review and concentration upon peer-reviewed work, been respected."[417]

[417] Graham Cogley et al., *Tracking the Source of Glacier Misinformation.* 327 SCI. 522 (2010).

The EPA TSD relies on these erroneous IPCC results, repeating the IPCC findings in Table 16.1, p. 162:

> Glacier melt in the Himalayas is projected to increase flooding and rock avalanches from destabilized slopes and to affect water resources within the next two to three decades. This will be followed by decreased river flows as the glaciers recede.

Since the IPCC has admitted its error the TSD necessarily must also acknowledge its mistake. This series of events both undercuts EPA's conclusion that the IPCC review process was "rigorous" and shows how reliance on the IPCC led to errors in EPA's Endangerment Finding.[418]

2. African Agricultural Production

The EPA TSD Table 16.1 (p. 162) includes the following statement about African food supplies:

> • Agricultural production, including access to food, in many countries and regions is projected to be severely compromised by climate variability and change. The area suitable for agriculture, the length of growing seasons, and yield potential, particularly along the margins of semi-arid and arid areas, are expected to decrease. This would further adversely affect food security and exacerbate malnutrition in the continent. In some countries, yields from rain-fed agriculture could be reduced by up to 50% by 2020.

TSD Table 16.1 is a listing of "Examples of Key Regional Impacts as Identified by IPCC (2007b)" IPCC 2007b is the Working Group II Report of the IPCC AR4. IPCC AR4 WGII

[418] Even more recent reports indicate more errors in AR4 concerning the Himalyan glaciers. AR4 stated the total area of Himalyan glaciers "will likely shrink from the present 500,000 to 100,000 square kilometers by the year 2035." There are only 33,000 square kilometers of glaciers in the Himalayas. A table stated that between 1845 and 1965, the Pindari Glacier shrank by 2,840m — a rate of 135.2m a year. The actual rate is only 23.5m a year. The section stated Himalayan glaciers are "receding faster than in any other part of the world" when many glaciologists state they are melting at about the same rate. *See* Jeremy Page, UN climate change expert: there could be more errors in report, THE TIMES ONLINE, Jan. 23, 2010 *available at* http://www.timesonline.co.uk/tol/news/environment/article6999051.ece.

Chapter 9.4.2 (p. 448) on African Agriculture, contains the following text which underlies the IPCC finding repeated in the TSD:

> In other countries, additional risks that could be exacerbated by climate change include greater erosion, deficiencies in yields from rain-fed agriculture of up to 50% during the 2000-2020 period, and reductions in crop growth period (Agoumi, 2003).

The Agoumi 2003 reference is: Agoumi, A., 2003: Vulnerability of North African countries to climatic changes: adaptation and implementation strategies for climatic change. Developing Perspectives on Climate Change: Issues and Analysis from Developing Countries and Countries with Economies in Transition. IISD/Climate Change Knowledge Network, 14 pp. The Agoumi study was published by The International Institute for Sustainable Development (IISD), an organization with a stated political interest in climate change and policy – it was *not* a study in the peer-review scientific literature. According to the Agoumi report:

> The International Institute for Sustainable Development contributes to sustainable development by advancing policy recommendations on international trade and investment, economic policy, climate change, measurement and indicators, and natural resource management. By using Internet communications, we report on international negotiations and broker knowledge gained through collaborative projects with global partners, resulting in more rigorous research, capacity building in developing countries and better dialogue between North and South.

There is no new science in the Agoumi study, and, instead is an assessment of other studies on possible effects of climate change on agriculture in three African countries: Morocco, Tunisia, and Algeria. And the studies cited by Agoumi to support its findings on the future of African agriculture under climate change are themselves not peer-reviewed studies in the scientific literature, but are other UN reports and national communications listed in Agoumi (2003) as:

- Vulnerability studies on three North African countries (Algeria, Morocco and Tunisia) with respect to climatic changes, performed within the framework of the UNEP-GEF Project RAB94G31: 2000–2001.

- Initial national communications by three countries (Algeria, Morocco, and Tunisia), presented at COP-7 in October 2001. These communications are available at the Website of the United Nations Framework Convention on Climate Change:

- Democratic and Popular Republic of Algeria, 2001. Initial National Communication, Ministry of Landuse Planning and the Environment, Office of the Environment, National Project Alg/98/G31, March.

- Republic of Tunisia, 2001. Initial Tunisian Communication on the United Nations Framework Convention on Climate Change, Ministry of the Environment and Land-use Planning, October.

- Kingdom of Morocco, 2001. Initial National Communication on the United Nations Framework Convention on Climate Change, Ministry of Land-use Planning, Housing, and the Environment, October.

Thus, the EPA TSD based its findings on the IPCC WGII report, which based its findings on a report published by an organization with a declared political interest in climate change that based its findings from an assessment of other non-peer reviewed national studies. This is not the way that EPA science should be carried out.

3. Amazon Rain Forests

The IPCC's reliance on another WWF paper also led it to make inaccurate claims about the effect of climate change on the Amazon rain forests. Section 13.4.1 of the Working Group II report cited a WWF-sponsored paper,"Rowell and Moore, 2000," as the basis for the following highly-provocative claim:

> Up to 40% of the Amazonian forests could react drastically to even a slight reduction in precipitation; this means that the tropical vegetation, hydrology and climate system in South America could change very rapidly to another steady state, not necessarily producing gradual changes between the current and the future situation[.]

The underlying WWF paper[419] stated as follows:

> Up to 40% of the Brazilian forest is extremely sensitive to small reductions in the amount of rainfall. In the 1998 dry season, some 270,000 sq. km of forest became vulnerable to fire, due to completely depleted plant-available water stored in the upper five metres of soil. A further 360,000 sq. km of forest had only 250 mm of plant-available soil water left.

For support, the WWF paper, in turn, referred to a peer-reviewed article in Nature.[420] However, the Nature article does not contain the 40% figure, and it focused specifically on logging and fire in Brazilian Amazonia, rather than the general hazard of climate change to Amazon forests. At relevant part, the Nature article stated:

> ENSO-related drought can desiccate large areas of Amazonian forest, creating the potential for large-scale forest fires. Because of the severe drought of 1997 and 1998, we calculate that approximately 270,000 km^2 of Amazonian forest had completely depleted plant-available water stored in the upper five metres of soil by the end of the 1998 dry season. In addition, 360,000 km^2 of forest had less than 250mm of plant-available soil water left by this time[.][421]

As a result, the provenance of the IPCC's striking "40%" figure is uncertain. It is also unclear how the Nature article was transformed, via the WWF article, into support for the general risk that climate change poses to the Amazon rainforest. Neither the WWF advocacy group paper nor the Nature article provides a basis for the IPCC's statement.

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[419] Andy Rowell and Peter Moore, *Global Review of Forest Fires*. 14 (WWF/IUCN 2000).

[420] Daniel Nepsted et al., *Large-scale Impoverishment of Amazonian Forests by Logging and Fire*, 398 NATURE 505 (1999).

[421] *Id.*

4. Melting Mountain Ice

In Chapter 1 of the Working Group II report, "Assessment of observed changes and responses in natural and managed systems," the IPCC cited two sources for observations that purportedly substantiate diminished amounts of mountain ice due to climate change.[422] The first was an article in a popular climbing magazine written by an environmentalist and outdoorsman, Mark Bowen: Bowen, N., 2002: *Canary in a coalmine*. Climbing News, 208**,** 90-97, 138-139. The second was a paper authored by Dario-Andri Schworer, apparently written at the time that he was a candidate for the equivalent of a masters degree at the University of Berne: Schwörer, D.A., 1997: Bergführer und Klimaänderung: eine Untersuchung im Berninagebiet über mögliche Auswirkungen einer Klimaänderung auf den Bergführerberuf (Mountain guides and climate change: an inquiry into possible effects of climatic change on the mountain guide trade in the Bernina region, Switzerland). According to a media report, the latter article was based on interviews with approximately 80 Swiss mountain guides, who provided anecdotal observations.[423] To our knowledge, neither paper was subject to peer-review or independent confirmation by the IPCC.

[422] *See* Table 2.1, "Selected observed effects due to changes in the cryosphere produced by warming."

[423] Richard Gray and Rebecca Lefort, *UN climate change panel based claims on student dissertation and magazine article, Telegraph.co.uk* (Jan. 30, 2010), *available at* http://www.telegraph.co.uk/earth/environment/climatechange/7111525/UN-climate-change-panel-based-claims-on-student-dissertation-and-magazine-article.html (visited Feb. 1, 2010).

5. Amount of Netherlands Below Sea Level

Although AR4 stated that 55 percent of the Netherlands is below sea level, Dutch authorities state that only 26 percent is. Dutch Environment Ministry spokesmen Trimo Vallaart stated that the error had been pointed out to the IPCC several times but was not corrected. He further stated that the Netherlands is conducting a review of the IPCC report for further errors.[424]

6. Other Instances of Reliance on Advocacy Group Material

We have not conducted a complete search of how frequently advocacy group material was used as a source of information in AR4; however, groups such as the WWF and Greenpeace were allowed the make significant contributions. We are aware of at least the following examples of the Working Group II report citing material produced by the WWF:

- In Section 8.4.2.5, AR4 cited a WWF report for the statement that climate change is affecting mountain glaciers, causing rapid glacier retreat in the Himalayas, Greenland, the European Alps, the Andes Cordillera and East Africa (WWF, 2005: An overview of glaciers, glacier retreat, and subsequent impacts in Nepal, India and China. World Wildlife Fund Nepal Program, 79 pp.);

- In Section 10.2.4.2, AR4 cited a WWF report for the conclusion that rapid glacial melting and runoff are causing more frequent mudflows and landslides (WWF, 2005: An overview of glaciers, glacier retreat, and subsequent impacts in Nepal, India and China. WorldWildlife Fund, Nepal Program, 79 pp.);

- In Section 12.4.7.2, AR4 cited a WWF workshop project report to support the statement that climate change will produce significant impacts on selected marine fish and shellfish in the north-east Atlantic marine ecoregion (Baker, T., 2005: Vulnerability Assessment of the North-East Atlantic Shelf Marine Ecoregion to Climate Change, Workshop Project Report, WWF, Godalming, Surrey, 79 pp.);

- In Section 12.7, AR4 cited a WWF report for the global baseline "ecological footprint" against which to compare the footprint of persons living in various global regions (WWF, 2004: Living Planet Report 2004. WWF-World Wide Fund for Nature, Gland, Switzerland, 44 pp.);

[424] *See* http://www.breitbart.com/article.php?id=CNG.8d6e5773c60565dfc6e882b0a8dcbf18.4e1&show_article=1.

- In Section 13.2.3, AR4 cited a WWF report as support for statements about damage to the environment in Latin America caused by the growth of the tourism industry and harbor dredging (WWF, 2004: Deforestation threatens the cradle of reef diversity. WorldWide Fund for Nature, 2 December 2004.);

- In Section 13.2.4, AR4 cited the same WWF report for the statement that destruction of mangrove forests substantially reduces fish population on nearby reefs (WWF, 2004: Deforestation threatens the cradle of reef diversity. WorldWide Fund for Nature, 2 December 2004.).

It appears that the IPCC also relied upon materials produced by the advocacy group Greenpeace in both the Working Group II and Working Group III reports. We are aware of at least the following examples:

- In Section 6.4.2.6, the Working Group II report referred to a Greenpeace sponsored article for an opaque statement related to climate change, tourism and coral reefs: Hoegh-Guldberg, O., H. Hoegh-Guldberg, H. Cesar and A. Timmerman, 2000: Pacific in peril: biological, economic and social impacts of climate change on Pacific coral reefs. Greenpeace, 72 pp.

- In Section 3.1.2, the Working Group III report cited a Greenpeace publication regarding sustainable development pathways: Lazarus, M., L. Greber, J. Hall, C. Bartels, S. Bernow, E. Hansen, P. Raskin, and D. Von Hippel, 1993: Towards a fossil free energy future: the next energy transition. Stockholm Environment Institute, Boston Center, Boston. Greenpeace International, Amsterdam;

- In Section 4.3.3.2, the Working Group III report referenced a Greenpeace document for the nations or regions having the most investment in wind-energy: Wind Force 12, 2005: Global Wind Energy Council and Greenpeace;

- In Section 4.3.3.5, the Working Group III report cited a Greenpeace sponsored paper for estimates of potential global concentrated solar power: Aringhoff, R., C. Aubrey, G. Brakmann, and S. Teske, 2003: Solar thermal power 2020, Greenpeace International/European Solar Thermal Power Industry Association, Netherlands;

- In Section 4.3.3.5, the Working Group III report cited a Greenpeace publication for solar energy projects under construction: ESTIA, 2004: Exploiting the heat from the sun to combat climate change. European Solar Thermal Industry Association and Greenpeace, Solar Thermal Power 2020, UK.

- In Section 4.3.3.6, the Working Group III report cited two Greenpeace documents for estimates of current global installed peak solar energy storage capacity: http://www.greenpeace.org.ar/

cop10ing/SolarGeneration.pdf, and Greenpeace, 2006: Solar generation. K. McDonald (ed.), Greenpeace International, Amsterdam;

- In Section 4.4.3.3, the Working Group III report referred to a Greenpeace publication for estimates of potential future wind energy generation: Global wind energy outlook. Global Wind Energy Council, Bruxelles and Greenpeace, Amsterdam, September, 56 pp.;

Moreover, the Working Group II Report frequently used a citation method that did not candidly reveal an underlying affiliation with an environmental advocacy group. While "WWF" was used for the works noted above, in many instances, the IPCC report referred only to the name of the author when the work was actually sponsored by an interest group. For example, Chapter 4, "Ecosystems, their properties, goods and services" relies upon a source cited as "Hansen et al., 2003." By referring to the Chapter 4 index, one learns that this source is sponsored by WWF: Hansen, L.J., J.L. Biringer and J.R. Hoffmann, 2003: Buying Time: A User's Manual for Building Resistance and Resilience to Climate Change in Natural Systems. WWF Climate Change Program, Berlin, 246 pp. The "Overview" chapter of this WWF user's manual states as follows:

> A VAST ARRAY OF SCIENTIFIC literature now makes it abundantly clear that the climate is changing and ecosystems are being affected by these changes. Much as awareness has been raised about invasive species, environmental contaminants, altered hydrology, and habitat fragmentation, conservation practitioners must now address climate change. This manual aims to assist natural resource and protected area managers as they begin to consider how to respond to this growing threat.[425]

In the same chapter, a work cited as "Malcolm, et al., 2002a" refers to Malcolm, J.R., C. Liu, L. Miller, T. Allnut and L. Hansen, Eds., 2002a: Habitats at Risk: Global Warming and Species Loss in Globally Significant Terrestrial Ecosystems. WWF WorldWide Fund for Nature, Gland,

[425] *Id.* at 11 (*available at* http://assets.panda.org/downloads/0forewordoverview_8kop.pdf).

40 pp. In Chapter 13, "Latin America," a citation to "Rowell and Moore, 2000" refers to another WWF publication: Rowell, A. and P.F. Moore, 2000: Global Review of Forest Fires. WWF/IUCN, Gland, Switzerland, 66 pp. Similarly, in Chapter 14, "North America," the citation to "O'Neal, 2002" refers to a work sponsored by the advocacy group Defenders of Wildlife: O'Neal, K., 2002: Effects of Global Warming on Trout and Salmon in U.S. Streams. Defenders of Wildlife, Washington, District of Columbia, 46 pp. It is unclear whether these sponsored works provide independent, objective, and peer-reviewed science that should be the basis for AR4, or whether they advance the environmental agenda of their sponsor. It is also unclear whether these sources were independently verified by IPCC authors and contributors.

F. Fabrication of Information in Responding to Reviewer Comments in Order to Justify Information Contained in AR4 Report

An IPCC author appears to have abused the review process by fabricating the views of the author of an article referred to in the IPCC text. The subject matter concerned an attribution of observed increases in damage resulting from extreme weather events to anthropogenic climate change, and in particular a study by Roger Pielke, Jr., showing that increased U.S. economic losses from hurricanes was attributable to changes in wealth and population.

In the AR4 review process, a reviewer (Francis Zwiers, a researcher with the Canadian Centre for Climate Modelling and Analysis) made the following comment (as documented in the IPCC's Expert Review Comments to the Working Group II Second Order Draft[426]:

> I think this is inappropriate. It leads the reader into interpreting recent events in a particular way without providing supporting information. This suggestion, that the losses in 2004 and 2005 draw Pielke's results into question, needs to be supported with a reference or a solid in chapter assessment. What does Pielke think about this?

[426] http://ipcc-wg2.gov/AR4/SOD_COMMS/Ch01_SOD_Expert.pdf, p.121.

The "this" that the reviewer suggested was inappropriate is most likely this passage that survived the review process and appears in the final report (IPCC AR4 WG II Chapter 1.3.8.4, p. 110):

> A previous normalisation of losses, undertaken for U.S. hurricanes by Pielke and Landsea (1998) and U.S. floods (Pielke et al., 2002) included normalising the economic losses for changes in wealth and population so as to express losses in constant dollars. These previous national U.S. assessments, as well as those for normalised Cuban hurricane losses (Pielke et al., 2003), did not show any significant upward trend in losses over time, but this was before the remarkable hurricane losses of 2004 and 2005.

The author of the text that Zwiers questioned responded as follows:

> I believe Pielke agrees that adding 2004 and 2005 has the potential to change his earlier conclusions – at least about the absence of a trend in US Cat losses.[427]

According to Roger Pielke Jr., however, the IPCC author, in fact, never inquired about the matter and indeed misrepresented his views.[428] The IPCC comments were made in August, 2006 (according to the IPCC Response to Reviewers document). In March of 2006, Pielke Jr. gave the Roger Revelle Commemorative Lecture at the Smithsonian Museum of Natural History in Washington, DC, sponsored by the Ocean Studies Board of the U.S. National Academy of Sciences. In that lecture Pielke Jr. discussed his views on the impact of adding 2004 and 2005 to his earlier analysis. That lecture was subsequently published as an article in the journal Oceanography in June, 2006, two months before the IPCC made its speculative claims about his views, and thus in principle readily available to the IPCC (a more formal, fully peer-reviewed

[427] http://ipcc-wg2.gov/AR4/SOD_COMMS/Ch01_SOD_Expert.pdf, p.121.

[428] Roger Pielke Jr., *Systematic Misrepresentation of the Science of Disasters and Climate Change*, June 17, 2009, http://rogerpielkejr.blogspot.com/2009/06/systematic-misrepresentation-of-science.html.

study was published in 2008 by Pielke Jr.— Roger Pielke, Jr., et al. *Normalized Hurricane Damages in the United States: 1900-2005*. 9 Natural Hazards Review 29, 2008).

In the lecture and the Oceanography article, Pielke Jr. stated the following:

> The case of hurricane impacts in the United States is similarly instructive. Consider economic damage (adjusted for inflation) related to hurricane landfalls in the United States, 1900–2005, as shown in Figure 4. Although damage is growing in both frequency and intensity, this trend does not reflect increased frequency or strength of hurricanes. In fact, while hurricane frequencies have varied a great deal over the past 100+ years, they have not increased in recent decades in parallel with increasing damages. To the contrary, although damage increased during the 1970s and 1980s, hurricane activity was considerably lower than in previous decades.
>
> To explain the increase in damage, it is therefore necessary to consider factors other than variability or change in climate. Society has changed enormously during the past century and coastal development has taken pace at an incredible pace.
>
> Given the significance of societal change in trends of hurricane damage, one way to present a more accurate perspective on such trends is to consider how past storms would affect present society. We developed a methodology for ''normalizing'' past hurricane damage to present-day values (using wealth, population, and inflation). Figure 5 shows the historical losses of Figure 4 normalized to 2005 values.[429]

Clearly, Pielke Jr.'s findings, which included the hurricanes of 2004 and 2005 did *not* resemble anything like the IPCC's author's fabrication that "Pielke agrees that adding 2004 and 2005 has the potential to change his earlier conclusions – at least about the absence of a trend in US Cat losses." In fact, Pielke Jr. thought quite the opposite. Once again, EPA's faith in the robustness of the IPCC review process has been shown to be unjustified.

[429] Roger Pielke, Jr., *Disasters, Death, and Destruction, Making Sense of Recent Calamities*, 19 OCEANOGRAPHY, 5-6 (2006) *available at* http://www.tos.org/oceanography/issues/issue_archive/issue_pdfs/19_2/19.2_pielke.pdf.

G. Failure to Make Data Sources of Unpublished Material Relied on in Text Available to Reviewers

Despite their reliance on unpublished literature in AR4 text, IPCC authors not only refused to supply the unpublished studies to IPCC reviewers, they actively plotted ways of preventing IPCC reviewers from gaining access to those studies and to the data underlying those studies. There does not appear to be any justification for this practice other than to avoid reviewers' offering informed critiques to textual citations of the unpublished studies. In light of the recent revelations concerning the data sources actually used in unpublished papers cited in AR4 text, this obstruction of the work of reviewers was unfortunate.

On September 19, 2005, McIntyre asked IPCC WGI: "For the unpublished articles [referenced in the draft Intergovernmental Panel on Climate Change Working Group I Report,] could you also provide locations of download sites where the underlying data may be reviewed," a request that in hindsight seems prescient.[430] Instead of just providing him with the information, the authors immediately decided that McIntyre was acting improperly and set to work to prevent him from getting the information on the flimsy excuse that the IPCC is not responsible for the data sources used in studies on which the IPCC relies. Martin Manning emailed Overpeck and Jansen:

> Following the release of the first draft of the WG1-AR4 we have had a response from Steve McIntyre (a name that should ring a bell) regarding unpublished literature in our Chapter. He also asks about access to data sets but that is not an IPCC function so is easily dealt with.[431]

[430] CRU email 1127614205.txt (Sept. 19, 2005).

[431] *Id.*

Evidently thwarted at the IPCC, McIntyre followed up on one study by first asking the author of the study for the data, who refused, and then writing to Colin O'Dowd, Editor of Journal of Geophysical Research:

> Dear Dr O'Dowd,
>
> I am a reviewer for the IPCC Fourth Assessment Report …and am writing in respect to a submission to your journal by D'Arrigo et al…. This article was referenced in chapter 6 of the Draft IPCC 4AR and made available to IPCC reviewers. In the course of my review, I contacted the senior author, Dr. D'Arrigo, for the FTP location of the data used in this article or for alternative access to the data. Dr D'Arrigo categorically refused and I was referred to the journal editor if I desired recourse.
>
> Data Citation and Archiving
>
> I point out that AGU policies for data citation and data archiving… specifically require that authors provide data citation according to AGU standards and require that contributors archive data in permanent archives, such as the World Data Center for Paleoclimatology…
>
> **In cases, where the data has been archived, it has not been cited according to AGU policies. …**
>
> **In order that this submission comply with AGU policies on data archiving, I request that you require D'Arrigo et al. do (1) provide accurate data citations complying with AGU policies for all data sets presently archived at the WDCP; (2) archive all "grey" data used in the article**.[432]

The IPCC author's saw McIntyre's request as an "abuse" of his position as an IPCC reviewer, when in actuality he was trying to fulfill his function of providing independent and critical review to the IPCC authors, something they evidently didn't want. Rob Wilson forwarded the request to Tim Osborn and Keith Briffa, stating:

> please see the e-mail below from Steve McIntyre to the Editor of JGR. This seems a major abuse of his position as reviewer for the IPCC?[433]

[432] CRU email 1128000000.txt (Sept. 27, 2005) (emphasis added).

[433] *Id.*

Osborn then replied to Wilson and Briffa, along with Rosanne D'Arrigo, the author in question:

> Dear Rob and Rosanne,
> I strongly agree that this is an abuse of his position as an IPCC reviewer! The data archiving issues are a separate issue because I think there's no need for the data you used to be publicly available until the paper is actually published …
>
> I will take this issue up with the chapter lead authors and the WG1 technical support unit—unless you prefer that I didn't. Please let me know.[434]

The position here seems to be that the as-yet unpublished paper should be cited in AR4 text, but IPCC reviewers should not be allowed to review the underlying data on the ground that the paper has not yet been published. This type of attitude is more indicative of authors seeking a whitewash of their work as opposed to honest peer review.

[434] *Id.*

VIII.

THE CRU SCIENTISTS AND THEIR AMERICAN COUNTERPARTS INAPPROPRIATELY INTERFERED WITH THE NORMAL PROCESS BY WHICH PEER REVIEW LITERATURE IS DEVELOPED, FURTHER UNDERMINING THEIR OWN CREDIBILITY AND THAT OF THE INFORMATION ON WHICH EPA RELIED

"Who will rid me of this troublesome editor?"

Paraphrase of Dr. Michael Mann

"As for thinking that it is "Better that nothing appear, than something unacceptable to us" as though we are the gatekeepers of all that is acceptable in the world of paleoclimatology seems amazingly arrogant. Science moves forward whether we agree with individual articles or not."

Dr. Ed Cook

A. Manipulation of Peer-Reviewed Literature to Prevent Publication of Undesired Papers and to Favor Publication of Desired Papers

1. Efforts Against Disfavored Papers, Authors and Editors

As leading scientists in the climate field, several of the authors involved in AR4 were in a position to affect the types of papers that were published in the peer-reviewed literature. They were leading contributors to journals, served as peer-reviewers, and acted as journal editors. They abused their positions of influence, however, by manipulating the peer-review publication process to prevent publication of papers at odds with their own views and even to oust editors who had published such papers.

As those involved with this scheme knew, science generally develops through peer-review publication, and thus preventing publication of unwanted scientific theories and research in peer-review literature was the key to preventing those theories and research from gaining credibility. Mann is an editor of the leading journal Journal of Climate, and in a September 2009 email he told New York Times reporter Andy Revkin:

> Yep, what was written below is all me, but it was purely on background, please don't quote anything I said or attribute to me w/out checking specifically--thanks.
>
> Re, your point at the end—you've taken the words out of my mouth. Skepticism is essential for the functioning of science. It yields an erratic path towards eventual truth. But legitimate scientific skepticism is exercised through formal scientific circles, *in particular the peer review process. A necessary though not in general sufficient condition for taking a scientific criticism seriously is that it has passed through the legitimate scientific peer review process.* those such as McIntyre who operate almost entirely outside of this system are not to be trusted.[435]

The CRU material furnishes numerous instances of Mann and others using their influence in preventing publication of undesired papers. For example, Wigley wrote to a large number of colleagues that he had tried to get a paper rejected by Dr. Patrick Michaels of the University of Virginia even though other peer reviewers not connected with the Jones et al. group had accepted it in the normal course and the refereeing process had been more rigorous than usual:

> Danny Harvey and I refereed this and said it should be rejected. We questioned the editor (deFreitas again!) and he responded, saying…..
>
> The MS was reviewed initially by five referees. … The other three referees, all reputable atmospheric scientists, agreed it should be published subject to minor revision. Even then I used a sixth person to help me decide. I took his advice and that of the three other referees and sent the MS back for revision. It was later accepted for publication. The refereeing process was more rigorous than usual.
>
> On the surface this looks to be above board—although, as referees who advised rejection, it is clear that Danny and I should have been kept in the loop and seen how our criticisms were responded to.[436]

[435] CRU email 1254259645.txt (Sep. 29, 2009) (emphasis added).

[436] CRU email 1051156418.txt (Apr. 23, 2003).

Similarly, Briffa contacted his friend Ed Cook of Lamont-Doherty Earth Observatory at Columbia University, who was acting as a reviewer of a paper he did not like, telling him that "confidentially" he needed a "hard and if required extensive case for rejecting" a piece.[437] In making his request, Briffa apparently inappropriately disclosed to Cook the identity and recommendation of the other reviewer when he said that he needed evidence for rejecting a work "to support Dave Stahle's" rejection.[438]

Cook replied the same day to Briffa to point out a review for the Journal of Agricultural, Biological, and Environmental Sciences of a paper which, if not rejected, could "really do some damage."[439] Cook went on to say that it is an "ugly" paper to review because it is "rather mathematical" and it "won't be easy to dismiss out of hand as the math appears to be correct theoretically."[440] Since the paper responded to points made in a Briffa paper, Cook enlisted Briffa's help in responding to it, rather than providing his own independent analysis:

> Now something to ask from you. Actually somewhat important too. I got a paper to review (submitted to the Journal of Agricultural, Biological, and Environmental Sciences), written by a Korean guy and someone from Berkeley, that claims that the method of reconstruction that we use in dendroclimatology (reverse regression) is wrong, biased, lousy, horrible, etc. … If published as is, this paper could really do some damage. It is also an ugly paper to review because it is rather mathematical, with a lot of Box-Jenkins stuff in it. It won't be easy to dismiss out of hand as the math appears to be correct theoretically, but it suffers from the classic problem of pointing out theoretical deficiencies, without showing that their improved inverse regression method is actually better in a practical sense. So they do lots of monte carlo stuff that shows the superiority of their method and the deficiencies of our way of doing things, but NEVER actually show how their

[437] CRU email 1054748574.txt (Jun. 4, 2003).

[438] *Id.*

[439] CRU email 1054756929.txt (Jun. 4, 2003).

[440] *Id.*

> method would change the Tornetrask reconstruction from what you produced. Your assistance here is greatly appreciated. Otherwise, I will let Tornetrask sink into the melting permafrost of northern Sweden (just kidding of course).[441]

Jones participated in this network of reviewers extensively, and he wrote to Mann about two papers that he hoped to block at two other prominent journals, Geophysical Research Letters ("GRL") and the Journal of Geophysical Research ("JGR"). Jones wrote that the "[r]ecently rejected two papers (one for JGR and for GRL) from people saying CRU has it wrong over Siberia. *Went to town in both reviews, hopefully successfully.* If either appears I will be very surprised, but you never know with GRL."[442]

Likely the most aggressive intrusion Jones, Mann and their allies made in the peer-review process followed publication of a paper by Willie Soon and Sallie Baliunas in the Journal of Climate Research that presented evidence that the MWP was at least as warm as today. Following publication of the article, Jones wrote Mann in 2003 that he was going to inform the Journal that he would boycott them if they did not fire "this troublesome editor." As head of the CRU and a leading scientist, Jones' boycott could have teeth. Jones' email said:

> I will be emailing the journal to tell them I'm having nothing more to do with ***it until they rid themselves of this troublesome editor***. A CRU person is on the editorial board, but papers get dealt with by the editor assigned by Hans von Storch…It results from this journal having a number of editors. The responsible one for this is a well-known skeptic in N[ew] Z[ealand]. He has let a few papers through by Michaels and Gray in the past. I've had words with Hans von Storch about this, but got nowhere. Another thing to discuss in Nice ![443]

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[441] *Id.*

[442] CRU email 1080742144.txt (Mar. 31, 2004) (emphasis added).

[443] CRU email 1047388489.txt (Mar. 11, 2003) (emphasis added). Dr. Michaels and Dr. Gray have collectively published numerous papers questioning the science underlying the IPCC reports.

Mann obviously agreed with Jones' bullying and responded that they should encourage others to boycott the journal and to intercede with the editorial board. He responded back:

> This was the danger of always criticizing the skeptics for not publishing in the "peer-reviewed literature". Obviously, they found a solution to that--take over a journal! So what do we do about this? *I think we have to stop considering "Climate Research" as a legitimate peer-reviewed journal. Perhaps we should encourage our colleagues in the climate research community to no longer submit to, or cite papers in, this journal. We would also need to consider what we tell or request of our more reasonable colleagues who currently sit on the editorial board...* What do others think?[444]

Tom Wigley went even further by urging a full-scale assault on the editorial board:

> "PS Re CR, I do not know the best way to handle the specifics of the editoring. Hans von Storch is partly to blame -- he encourages the publication of crap science 'in order to stimulate debate'. *One approach is to go direct to the publishers and point out the fact that their journal is perceived as being a medium for disseminating misinformation under the guise of refereed work.* ***I use the word 'perceived' here, since whether it is true or not is not what the publishers care about -- it is how the journal is seen by the community that counts.***
>
> I think we could get a large group of highly credentialed scientists to sign such a letter -- 50+ people.
>
> Note that I am copying this view only to Mike Hulme and Phil Jones. Mike's idea to get editorial board members to resign will probably not work -- ***must get rid of von Storch too,*** otherwise holes will eventually fill up with people like Legates, Balling, Lindzen, Michaels, Singer, etc. I have heard that the publishers are not happy with von Storch, so the above approach might remove that hurdle too."[445]

In July 2003, the Director of Climate Research, Otto Kinne, investigated the complaints about the editing and refereeing process, and wrote a defense:

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[444] *Id.*

[445] CRU email 1051190249.txt (Apr. 24, 2003) (emphasis added).

> Dear colleagues, In my [20 June 2003] email to you I stated, among other things, that I would ask C[limate] R[esearch] editor Chris de Freitas to present to me copies of the reviewers' evaluations for the 2 Soon et al. papers. I have received and studied the material requested. Conclusions: 1) The reviewers consulted (4 for each ms) by the editor presented detailed, critical and helpful evaluations. 2) The editor properly analyzed the evaluations and requested appropriate revisions. 3) The authors revised their manuscripts accordingly. Summary: Chris de Freitas has done a good and correct job as editor. [446]

This answer did not satisfy Mann. Mann wrote to Jones, Wigley, and Hulme that Kinne was "disingenuous" and said "I think that the community should, as previously suggested in this eventuality, terminate its involvement with this journal at all levels--reviewing, editing, and submitting, and leave it to wither way into oblivion and disrepute."[447]

Wigley wondered whether going public with their boycott would be illegal: "I agree that Kinne seems like he could be a deFreitas clone. However, what would be our legal position if we were to openly and extensively tell people to avoid the journal?"[448] Santer responded that he was simply going to give up on Climate Research and publish in another journal. He said

> Based on Kinne's editorial, I see little hope for more enlightened editorial decision making at Climate Research. Tom, Richard Smith and I will eventually publish a rebuttal to the Douglass et al. paper. We'll publish this rebuttal in JGR - not in Climate Research.[449]

In response to these protests, wholesale changes were made in the editorial staff at Climate Research.

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[446] CRU email 1057941657.txt (Jul. 11, 2003).

[447] *Id.*

[448] *Id.*

[449] *Id.*

Similar attempts to prevent publication of undesired papers also occurred at Geophysical Research Letters ("GRL"). Mann et al. plotted how to deal with an editor that approved publication of a paper by the hated McIntyre and McKitrick. Mann's first gambit was evidently to attempt to insert himself into the review process, but Steve Mackwell, Editor in Chief of Geophysical Research Letters, resisted:

> Dear Prof. Mann
>
> In your recent email to Chris Reason, you laid out your concerns that I presume were the reason for your phone call to me last week. I have reviewed the manuscript by McIntyre, as well as the reviews. The editor in this case was Prof. James Saiers. He did note initially that the manuscript did challenge published work, and so felt the need for an extensive and thorough review. For that reason, he requested reviews from 3 knowledgeable scientists. All three reviews recommended publication.
>
> While I do agree that this manuscript does challenge (somewhat aggresively) some of your past work, I do not feel that it takes a particularly harsh tone. On the other hand, I can understand your reaction. As this manuscript was not written as a Comment, but rather as a full-up scientific manuscript, you would not in general be asked to look it over. And I am satisfied by the credentials of the reviewers. Thus, I do not feel that we have sufficient reason to interfere in the timely publication of this work.[450]

Mann was dissatisfied with this response and told his colleagues:

> Dear All,
>
> Just a heads up. Apparently, the contrarians now have an "in" with GRL. This guy Saiers has a prior connection w/ the University of Virginia Dept. of Environmental Sciences that causes me some unease.
>
> I think we now know how the various Douglass et al papers with Michaels and Singer, the Soon et al paper, and now this one have gotten published in GRL.[451]

Wigley then suggested that a campaign be organized to oust the offending editor:

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[450] CRU email 1106322460.txt (Jan. 20, 2005).

[451] *Id.*

> This is truly awful. GRL has gone downhill rapidly in recent years. I think the decline began before Saiers. I have had some unhelpful dealings with him recently with regard to a paper Sarah and and I have on glaciers—it was well received by the referees, and so is in the publication pipeline. However, I got the impression that Saiers was trying to keep it from being published.
>
> Proving bad behavior here is very difficult. If you think that Saiers is in the greenhouse skeptics camp, then, if we can find documentary evidence of this, we could go through official AGU channels to get him ousted. Even this would be difficult.[452]

Mann agreed, suggesting that Mackwell should be included, even though he admitted that he didn't know Mackwell and so his sole basis for suggesting that Mackwell might have to be replaced was Mackwell's resistance to Mann's attempt to intrude himself in the journal's internal editorial process:

> Yeah, basically this is just a heads up to people that something might be up here. What a shame that would be. It's one thing to lose "Climate Research." We can't afford to lose GRL. I think it would be useful if people begin to record their experiences w/ both Saiers and potentially Mackwell (I don't know him—he would seem to be complicit w/ what is going on here).
> If there is a clear body of evidence that something is amiss, it could be taken through the proper channels. I don't that the entire AGU hierarchy has yet been compromised![453]

Mann continued in the same vein in a further email that the group may have to boycott GRL completely:

> I'm not sure that GRL can be seen as an honest broker in these debates anymore, and it is probably best to do an end run around GRL now where possible. They have published far too many deeply flawed contrarian papers in the past year or so. There is no possible excuse for them publishing all 3 Douglass papers and the Soon et al. paper. These were all pure crap.

[452] *Id.*

[453] *Id.*

> There appears to be a more fundamental problem w/ GRL now, unfortunately…[454]

Further emailing continued over the course of the year concerning the GRL "problem." Wigley wrote to his colleagues in June that:

> As you know, we suspect that there has been an abuse of the scientific review process at the journal editor level. The method is to choose reviewers who are sympathetic to the anti-greenhouse view. Recent papers in GRL (including the M&M paper) ***have clearly not been reviewed by appropriate people. We have a strong suspicion that this is the case, but, of course, no proof because we do not know *who* the reviewers of these papers have been.*** Perhaps now is the time to make this a direct accusation and request (or demand) that this information be made available. In order to properly defend the good science it is essential that the reasons for bad science appearing in the literature be investigated. … I note that the instigators of all this are Canadians and that the science has no national boundaries.[455]

Mann later expressed satisfaction that the problem had been solved: "[t]he GRL leak may have been plugged up now w/ new editorial leadership there"[456] as if the publication of papers that do not agree with Mann's views is a "leak" that needs to be "plugged."

Review of a 2009 McIntyre and McKitrick paper involved a different tactic for manipulating the peer-review process. On January 29, 2009, Jones spoke of telling the editor of IJC, Glenn McGregor, to select reviewers of a paper submitted by McIntyre and McKitrick who he knows will be critical. Jones wrote to Ben Santer:

> I've just seen that M+M have submitted a paper to IJC on your H2 statistic - using more years, up to 2007… Anyway you'll likely get this for review, or poor Francis will. Best if both Francis and Myles did this. If I get an email from Glenn I'll suggest this.[457]

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[454] *Id.*

[455] CRU email 1119957715.txt (Jun. 25, 2005).

[456] CRU email 1132094873.txt (Nov. 15, 2005).

[457] CRU email 1233249393.txt (Jan. 29, 2009).

It is unethical to try to thwart the review process in this manner by keeping a paper from publication by selecting editors biased against the works they are reviewing.

Santer responded to Jones the same day indicating that he had been provided with an advance copy of the paper, which is also inappropriate since Santer had not been selected as a reviewer. Santer agreed with Jones' suggestion that favorable reviewers should be selected in order to prevent publication of the paper: "Dear Phil. Yeah, I had already seen the stuff from McIntyre… It would be great if Francis and Myles got McIntyre's paper for review."[458]

2. Efforts for Favored Papers

In contrast to their strong-arm tactics against disfavored papers, Jones, Mann, Wigley, Santer, and Trenberth bent the rules to ensure publication of papers that supported their position regardless of quality. As an Editor for the Journal of Climate, Mann recommended Jones to review a paper by Santer and Wigley.[459] Jones and Santer were PhD students of Wigley, and Jones had been part of an email distribution list in which Santer and Wigley discussed the preparation of the paper.[460] Thus, Jones would hardly be considered an impartial reviewer. Ultimately, Jones gave the paper a positive review,[461] although he was clearly dissatisfied with it when he said "[a]lso just sent back comments to Mike Mann on the paper by Tom and you factoring out ENSO and Volcanoes. Felt like writing red ink all over it, but sent back a short publish subject to minor revision to Mike." [462]

[458] CRU email 1233245601.txt (Jan. 29, 2009).

[459] CRU email 972415204.txt (Oct. 24, 2000).

[460] CRU email 969652057.txt (Sep. 22, 2000).

[461] CRU email 972415204.txt (Oct. 24, 2000).

[462] CRU email 972499087.txt (Oct. 25, 2000).

Another way the peer-review process was tainted in favor of desired papers is by expediting publication where necessary to get an immediate response to an undesirable paper. We saw above how this tactic was used to allow Santer to get an expedited response to the Douglass et al. paper concerning an asserted anthropogenic warming "fingerprint" in the tropical troposphere. The same tactic was used following publication in JGR of a paper by John McLean, Chris de Freitas (the editor of Climate Research who supervised the publication of the disliked Soon and Baliunas paper), and Robert Carter. The paper showed that much of the warming signal in the southern hemisphere has been driven by El Nino Southern Oscillation ("ENSO") rather than carbon dioxide.

Mann emailed Trenberth about the paper this past July, and Trenberth replied that they needed to bypass the editor who allowed publication of the paper in order to obtain special treatment in getting a reply published immediately. Mann fully agreed and laid out his strategy:

> We probably need to take this directly to the chief editor at JGR, asking that this not be handled by the editor who presided over the original paper, as this would represent a conflict of interest. if we are told that is not possible, then we would at least want the chief editor himself to closely monitor the handling of the paper. I too am happy to sign of at this point[463]

Trenberth replied that he agreed that their request for favorable treatment in responding to the paper meant that they were criticizing the editor who had approved that paper, and he said that they should seek to avoid that editor completely:

> You have a go from me. By all means clean up. I think you should argue that it should be expedited for the reasons of interest by the press. Key question is who was the editor who handled the original, because this is an implicit criticism of that person. May need to point this out and ensure that someone else handles it.[464]

[463] CRU email 1249007192.txt (Jul. 30, 2009).

[464] *Id.*

The response paper[465] was submitted by Grant Foster to JGR Atmosphere, and Jones was a co-author. In Foster's submission letter, he asked the journal to take supervision of his comment away from the person who supervised the original publication:

> Please consider the attached manuscript for publication in the Journal of Geophysical Research (Atmospheres). We consider that the errors in the analysis of McLean et al are so serious that the publication of a Comment to correct the public record is amply justified. In view of the high profile of the issue, we would prefer if one of the senior editors could take charge of the editorial process.[466]

James Salinger, another lead author for AR4, responded to Foster:

> Should we not also inquire about their time line for publishing the comment, and on the basis that is so serious, and the implications of their flawed findings ask it to be expedited.
>
> Perhaps
> We also note that the paper is now being used as the basis of campaigns against climate change policy and, should you decide to go ahead and publish our comment, expedite its acceptance.[467]

The effort to get an expedited response to the McLean et al. paper included a direct violation of the journal's policy that reviewers recommended by the authors could not be their close associates. Jones plotted as follows:

> Agree with Kevin that Tom Karl has too much to do. Tom Wigley is semi retired and like Mike Wallace may not be responsive to requests from the JGR.
>
> We have Ben Santer in common! Dave Thompson is a good suggestion. I'd go for one of Tom Peterson or Dave Easterling. To get a spread, I'd go with 3 US, One Australian, and one in Europe. So I suggest Neville Nicholls and David Parker.

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[465] Grant Foster et al., *Comment on" Influence of the Southern Oscillation on tropospheric temperature", by J.D. McLean et al.*, J. GEOPHYS. RES. submitted (2009).

[466] CRU email 1249326482.txt (Aug. 3, 2009).

[467] *Id.*

> ***All of them know the sorts of things to say***—about our comment and the awful original, without any prompting.[468]

Ultimately, Foster suggested five reviewers to the journal, including Wigley and Santer, who were obviously closely connected to Jones, as well as Wallace, Thompson and Easterling per Jones' recommendation on the basis that he knew that they would support the paper.[469] This is a direct abuse of the peer review process in violation of journalistic norms. Trenberth then tried to influence publication by giving a copy of the manuscript to Michael McPhaden, president of the American Geophysical Union, which publishes JGR:

> About time. Incidentally i gave a copy to Mike McPhaden and discussed it with him last week when we were together at the OceanObs'09 conference. Mike is President of AGU.[470]

B. Intimidation/bad-mouthing to influence scientific development

Mann and Wigley in particular resorted to admonishment of those they disagreed with. For instance, Mexican climate researcher Jorge Sánchez-Sesma said that he met Mann at a conference. At first Mann was "very kind," but when Mann found out Sánchez-Sesma's work ran counter to some of Mann's conclusions, Mann "changed his attitude."[471]

Wigley goes so far as to suggest that efforts be made to ruin the careers of fellow scientists:

> Jim Titus mentioned to me that in the legal profession here people are disbarred for behavior like that of De Freitas (and even John Christy -- although this is a more subtle case). We cannot do that of course, but *we can alert the community of honest scientists to such behavior and formally discredit these people*.[472]

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[468] CRU email 1249503274.txt (Aug. 5, 2009). (emphasis added.

[469] CRU email 1249503274.txt (Aug. 5, 2008) (emphasis added).

[470] CRU email 1254163518.txt (Sep 28, 2009).

[471] CRU email 1079384474.txt (Mar. 15, 2004).

[472] CRU email 1061298033.txt (Aug. 19, 2003) (emphasis added).

Dr. Christy is a well-respected and multiple-award-winning professor of atmospheric science, director of the Earth System Science Center at the University of Alabama in Huntsville, and a Fellow of the American Meteorological Society. Dr. de Freitas is an Associate Professor in the School of Geography, Geology and Environmental Science at the University of Auckland in New Zealand. He has served as Deputy Dean of Science, Head of Science and Technology, and for four years as Pro Vice Chancellor at that university. He is Vice President of the Meteorological Society of New Zealand and is a founding member of the Australia-New Zealand Climate Forumm, and serves on the Executive Board of the International Society of Biometeorology from 1999-2001. Plainly, the credentials of scientists who disagree with him do not stop Wigley from engaging in character assassination.

Mann continued his battle against McIntyre and McKitrick in the press, telling New York Times reporter Andy Revkin:

> The McIntyre and McKitrick paper is pure scientific fraud. I think you'll find this reinforced by just about any legitimate scientist in our field you discuss this with. ... To recap, I hope you don't mention MM at all. It really doesn't deserve any additional publicity.[473]

The politics of climate research was enough to make at least one scientist contemplate leaving the field completely. After constant haranguing by Mann et al., Ed Cook, a scientist that battled Mann over the MWP, wrote:

> I never wanted to get involved in this quixotic game of producing the next great NH temperature reconstruction ***because of the professional politics and sensitivities involved.*** … This all reinforces my determination to leave this NH/global temperature reconstruction junk behind me once I get this paper submitted. It's

[473] CRU email 1107899057.txt (Feb. 8, 2005).

> not worth the aggravation. However, the paper is something that I need to do for Jan. And I still think it is a good paper.[474]

Mann's intimidation affected the way results were presented in journal articles. Briffa wrote in a review submitted to Science magazine:

> You will see that I think the work is genuinely interesting and potentially of wide significance. The bottom line is that you should publish this but the way the authors have chosen to present their results smacks of a lack of clarity of thought (and a lot of fudging!). *I believe that they are more concerned with trying to temper their ideas so as not to "offend" Mann et al*.[475]

That was not the only time that Briffa discussed the ramifications of angering Mann. In a October 2003 email, Briffa told some less established paleoclimate researchers that their results may start a "minor explosion" because they differed from Mann's, but "that is what the science needs."[476] The up-and-coming researchers wrote back to the well-established Briffa and asked if he would be a co-author on the paper, among other reasons because of the potential for this "minor explosion" which he no doubt could better shield them from. Briffa never became a co-author. Although the paper was published, it repeats a pattern of scholarly intimidation that results from fear of reprisal from Mann.

C. Excluding scientists with undesired opinions from conferences.

The campaign against disagreeing scientists was not limited to publications and intimidation. Diverging opinions were also left out of conferences and professional events. Mann tried to have names removed from a conference invitee list on the ground that they did not agree with the "consensus" viewpoint. He said "the last two on the list (w/ question marks) would be unwise choices because they are likely to cause conflict than to contribute to concensus

[474] CRU email 1000154718.txt (Sep. 10, 2001) (emphasis added).

[475] CRU email 1014240346.txt (Feb. 20, 2002) (emphasis added).

[476] CRU email 1066075033.txt (Oct. 13, 2003).

and progress."[477] Phil Jones concurred by saying "I agree with Mike that the last two names on the list should be removed."[478] Mann later elaborated on exactly why he did not want to invite Zorita, in words that more aptly describe Mann's own conduct:

> I'm afraid I don't agree on Zorita. He has engaged in some very nasty, and in my opinion unprofessional email exchanges with some close colleagues of mine who have established some fundamental undisclosed errors in work he co-published with von Storch. Given this, I don't believe he can be involved in constructive dialogue of the sort we're looking for at this workshop. There are some similarly problematic issues w/ Cubasch, who like von Storch, who has engaged in inflammatory and ad hominem public commentary. There is no room for that on any side of the debate.[479]

Mann's tactics gave pause to even some on his side of the issue. Bradley, a co-author of the original hockey stick paper, disassociated himself from comments made by Mann to the editor of Science magazine after it published a paper Mann did not like. Mann thought it was better for information that questions his position to not be published at all. Bradley responded:

> As for thinking that it is "Better that nothing appear, than something unnacceptable to us".....as though we are the gatekeepers of all that is acceptable in the world of paleoclimatology seems amazingly arrogant. Science moves forward whether we agree with individual articles or not.[480]

D. Conclusions as to Manipulation of Peer-Reviewed Science

In a recent letter to the editor of The Wall Street Journal, Mann wrote:

> Society relies upon the integrity of the scientific literature to inform sound policy. It is thus a serious offense to compromise the peer-review system in such a way as to allow anyone—including

[477] CRU email 1124994521.txt (Aug. 25, 2005).

[478] *Id.*

[479] CRU email 1125067952.txt (Aug. 26, 2005).

[480] CRU email 0924532891.txt (Apr. 19, 1999).

> proponents of climate change science—to promote unsubstantiated claims and distortions.[481]

Yet Mann and his collaborators engaged in a decade-long campaign of preventing dissenting scientific voices from being heard – a campaign that involved manipulation of both the IPCC publication and peer-review publication rules, threats of boycotts unless "troublesome" editors were fired, and threats to ruin the careers of those who did not support the "consensus" viewpoints. In the end, Mann and his colleagues became what Mann's former collaborator Bradley believed was "amazingly arrogant:" "the gatekeepers of all that is acceptable in the world of paleoclimatology."

[481] Michael Mann, *Science Journals Must Be Unpolluted by Politics*, WALL STREET J. A12, Dec. 31, 2009.

IX.

THE ABUSES REVEALED IN THE CRU MATERIAL REQUIRE THAT EPA RECONSIDER ITS ENDANGERMENT FINDING AND CONDUCT FULL EVIDENTIARY HEARINGS ON THE SCIENCE

A. Unless EPA Reconsiders the Endangerment Finding in Light of the CRU Material, the Finding Will Be Arbitrary, Capricious, and Otherwise Not in Accordance with Law

1. Even Without Considering the CRU Material, EPA Appears to Have Abrogated Its Statutory Obligation to Exercise Its Own Judgment

We have seen above that EPA did not conduct its own independent examination of climate change science and instead relied almost entirely on the "assessment literature." As EPA stated in the TSD, in language that is repeated in the Endangerment Finding and otherwise in the TS and Response to Comments document, it "relies most heavily" on this "assessment literature."[482] In particular, as seen, on the critical attribution issue, EPA relied primarily on the work of the IPCC, an international body not subject to U.S. data quality standards.

Section 202(a), however, requires that the Agency exercise its own judgment, not merely summarize judgments reached by others who are not subject to U.S. standards. EPA seems to believe that it met its obligation to exercise judgment by reviewing the procedures with which the "assessment literature" was prepared – at least those procedures as they exist on paper – to ensure that such literature conforms to the Agency's standards for quality and transparency and represents what EPA believes is a "consensus" view. But even if EPA is correct that the "assessment literature" represents a "consensus" view, and even if that literature had been prepared according to U.S. data quality standards (which it wasn't), the statute requires more of the Agency. It requires a searching inquiry by EPA of the basis on which the asserted "consensus" exists so that the Agency can exercise judgment as to whether the Agency *itself*

[482] TSD at 4.

agrees with the "consensus" view. Indeed, as Ed Cook, one of the CRU scientists, informed his colleagues who were urging on him the "consensus" view, "consensus science can impede progress as much as promote understanding."[483] Section 202(a) requires EPA to exercise *its* judgment, not to report the judgment of others.

EPA's failure to exercise its own judgment is reflected in the procedures it used to ensure the validity of the Endangerment Finding, procedures that were far less than the Agency would have used if it were truly rendering its own opinion on a matter of such importance. For example, the Agency did not select independent peer reviewers to review its Endangerment Finding and instead deliberately selected reviewers who had been involved with preparing the "assessment literature." As EPA said, "[g]iven our approach to the scientific literature the purpose of the federal expert review was to ensure that the TSD accurately summarized the conclusions and associated uncertainties from the assessment reports."[484] Similarly, EPA did not docket the data and studies relied on by the "assessment literature," or the data relied on by those studies, assuming (wrongly) that such information could be tracked down by the public. EPA did not independently investigate and confirm the quality and transparency of the information cited in the "assessment literature," relying instead on its review of the written procedures of the organizations preparing that literature to conclude that those organizations had ensured quality and transparency. And EPA even determined the length of the comment period based in substantial part on the public's prior ability to comment directly on the "assessment literature."

EPA's decision to proceed in this fashion strikes right at the heart of the notice and comment rulemaking that EPA undertook in support of the Endangerment Finding. The

[483] CRU email 988831541.txt (May 2, 2001).

[484] Resp. to Comm. Vol. 1 at 59.

fundamental purpose of the comment process, after all, is to ensure that a "genuine interchange" is carried on between the agency and the public. *Conn. Light & Power v. NRC,* 673 F.2d 525, 530-31 (D.C. Cir. 1982). As the courts have said, "[t]he touchstone of our inquiry is thus the agency's openmindedness ...." *Advocates for Highway and Auto Safety v. Federal Hwy. Admin.,* 28 F.3d 1288, 1292 (D.C. Cir. 1994) (citing *Air Transport Ass'n of America v. Dep't of Transportation,* 900 F.2d 369, 379 (D.C. Cir. 1990). Here, there could be no genuine interchange between an openminded EPA and the public on what EPA's scientific judgment should be because the Agency did not intend to exercise any such judgment.

2. The Revelations in the CRU Material Require EPA to Reconsider the Endangerment Finding

Even assuming *arguendo* that EPA could, in theory, have relied primarily on the "assessment literature," the CRU material shows that such reliance was misplaced. Although EPA believed that its review of the written procedures of the organizations that prepared the "assessment literature" was sufficient to ensure that data and information used by in that literature met EPA's standards for quality and transparency, the CRU material demonstrates that the IPCC process was not transparent and rigorous and therefore does not meet the standards to which EPA is subject. Having relied so heavily on the "assessment literature," EPA must now reconsider that reliance in light of the flaws revealed in the CRU material. As the matter stands, EPA has effectively ceded its responsibilities to an international body whose conclusions were reached following a flawed scientific process that cannot validly form the basis for EPA regulation.

As described at great length above, the individuals implicated by the CRU material were the leading scientists involved with preparation of the IPCC chapters on which EPA relied in the Endangerment Finding on the critical issue of whether climate change is caused by

anthropogenic GHG emissions. The abuses revealed call into question both the substance of the IPCC chapters that EPA relied on and the process used to prepare those chapters. The abuses thus go to the core of the Endangerment Finding and therefore are plainly of "central relevance" to that finding within the meaning of CAA § 307(d)(7)(B).

For instance, the coordinating lead authors and lead authors of those chapters appear to have suppressed information and studies that conflicted with their desire to tell a story of unprecedented 20th century climate change. They manipulated publication deadlines to favor inclusion in their chapters of studies supporting their views, they allowed those who supported their views to act as contributing authors without disclosing that fact, and they inappropriately acted both as authors and reviewers, again without proper disclosure, and they relied on unpublished secondary source material prepared by advocacy organizations. The CRU material shows that these chapters cannot be described as the product of an open process and a neutral summary of the science. Even worse, the peer-review pressure was so conflicted and agenda-driven that basic factual errors are beginning to emerge and the conclusions are now being disputed in the scientific literature.

The abuses, furthermore, included manipulating the development of peer-reviewed literature to influence the types of studies that would be used both in the IPCC process and in the course of public debate. These scientists used their influence in the climate science journal community to prevent publication of papers that conflicted with their views, and they even actively discussed organizing boycotts and took other action to get "troublesome" journal editors – those who allowed publication of articles that they didn't like – replaced. They used inappropriate means to get papers published supporting their views, as by applying lenient peer-review standards, by selecting peer-reviewers who were close associates of the author in

violation of the standards of the affected journal, and by expediting publication of responses to articles that they did not like. And they routinely denigrated the work of scientists with whom they did not agree, calling them "moron[s]," "idiots," and perpetrators of "fraud."[485]

Of at least equal concern, they absolutely stonewalled efforts to obtain underlying data that could be used to replicate and critique their work, even when the requests were submitted under FOIA and even to the point of destroying information. Their view seemed to be that information would not be provided to skeptics on the ground that, as one of them admitted, "Why should I make the data available to you, when your aim is to try and find something wrong with it?" And, in a similar vein, they shrouded the IPCC process in secrecy by refusing to divulge, and even destroying, information pertaining to preparation of the IPCC reports, in violation of U.K. freedom of information laws.

These actions fall far short of the standards to which EPA is subject in preparing its Endangerment Finding. They reflect a fundamentally arbitrary approach to science that is not open to dissenting views. EPA, of course, cannot rely, particularly to the extent it did here, on information that was prepared in such an arbitrary fashion.

Moreover, the actions of these influential scientists show a lack of transparency directly at odds with EPA's obligations under the IQA and under the standards for transparency that EPA has set for itself. As EPA's IQA Guidelines provide:

> For disseminated influential and supporting data, EPA intends to ensure reproducibility according to commonly accepted scientific, financial, or statistical standards. ***It is important that analytic results for influential information have a higher degree of transparency*** regarding (1) the source of the data used, (2) the

[485] CRU email 1147435800.txt (May 12, 2006); CRU email 1092167224.txt (Aug. 10, 2004); and CRU email 1197325034.txt (Dec. 12, 2007).

> various assumptions employed, (3) the analytic methods applied, and (4) the statistical assumptions employed.[486]

The Administrator specifically committed to high levels of transparency immediately upon taking office, both in her January 23, 2009 memorandum to EPA employees ("we [will] fully disclose the information that forms the basis for our decisions) and in her March 9, 2009 memorandum regarding scientific integrity reiterating her "commitment to transparency." That commitment and EPA's responsibility for transparent science set forth in its IQA guidelines cannot be squared with the actions revealed in the CRU material.

In sum, the Endangerment Finding stated that "EPA has no reason to believe that the assessment reports do not represent the best source of material to determine the state of science and the consensus view of the world's scientific experts on the issues central to making an endangerment decision with respect to greenhouse gases."[487] Given the CRU material, however, it now does.

3. The Broad Discretion Afforded to EPA by the Statute Does Not Justify EPA's Endangerment Finding

EPA takes the position that it has broad discretion in making an endangerment finding given the "precautionary and preventive" nature of the endangerment language in section 202(a). It argues that it does not have to show a "significant risk of harm" nor that harm is actually occurring or is probable. According to EPA, so long as the magnitude of risk is high, as the Agency believes to be the case with GHG endangerment, EPA could make an endangerment finding even if the risk of the endangerment occurring is relatively low.[488]

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[486] Guidelines for Ensuring and Maximizing the Quality, Objectivity, Utility and Integrity of Information Disseminated by the Environmental Protection Agency (Oct. 2002) at 21 (emphasis added).

[487] Endangerment Finding, 74 Fed. Reg. at 66,511.

[488] *See* general discussion at Endangerment Finding, 74 Fed. Reg. at 66,506-09.

The proper legal standard for the Endangerment Finding was discussed extensively in comments, and we will not reiterate that discussion here. Nevertheless, it is important to highlight that, whatever discretion EPA may have in making an Endangerment Finding, it must nevertheless justify and defend the Endangerment Finding that it actually made. *See Motor Vehicle Mfrs. Ass'n of the United States, Inc. v. State Farm Mutual Auto Ins. Co.*, 463 U.S. 29, 43 (1983) (agency must articulate a "rational connection between the facts found and the choice made"). Here, EPA did not assess the danger as low risk/high magnitude. It found instead both a high risk and high magnitude of harm. Thus, based on the "assessment literature," EPA found that "[t]he scientific evidence is compelling that elevated concentrations of heat-trapping greenhouse gases are the root cause of recently observed climate change."[489] Citing the IPCC, EPA stated that "[m]ost of the observed increase in global average temperatures since the mid-20th century is very likely due to the observed increase in anthropogenic GHG concentrations," with "very likely" defined as 90-99% probability.[490] In other words, in EPA's view, there is not just a risk of climate change danger caused by anthropogenic GHGs, that danger is already occurring.

EPA must now defend its high risk/high harm conclusion, even if *arguendo* it had discretion to make a lower finding of endangerment. As the United States Court of Appeals for the D.C. Circuit recently said in invalidating EPA's Clean Air Interstate Rule, the Agency must defend the choice it made, even if had discretion to make a different choice. *North Carolina v. EPA*, 531 F.3d 896, 901 (D.C. Cir., 2008).

[489] *Id.* at 66,518.

[490] TSD at 48, 7.

This distinction between the Endangerment Finding that EPA might be authorized to make and the Endangerment Finding it actually made here is crucial in light of the CRU material. EPA may be tempted to argue that, even with the CRU material, it may still find endangerment given the very permissive (in EPA's view) statutory standard. But the fact that EPA might still be able to make an Endangerment Finding *of some kind* (a fact that Peabody does not concede) does not justify the Endangerment Finding that EPA actually made.

Indeed, this distinction becomes even more crucial given EPA's view of how its Endangerment Finding fits into the regulatory framework of section 202(a). EPA maintains that, at the Endangerment Finding phase of the regulatory process, it does not have to find that control measures that would be triggered by the finding would actually prevent a substantial part of the danger it found.[491] It also maintains that it does not have to assess health and welfare benefits from the processes that produce GHG emissions, benefits that may be reduced by EPA regulatory attempts to eliminate or reduce the endangerment.[492] In fact, the Agency argues that all questions as to the interrelationship of endangerment and regulatory consequences are not relevant at the endangerment phase and do not become relevant until the regulatory phase.[493]

If EPA's view of the statutory structure is correct (which we do not concede), then it becomes even more important that EPA correctly identify and justify, at the endangerment phase, the specific type of endangerment that EPA believes is occurring and will occur. Obviously, the regulation that EPA ultimately proposes must be guided by the nature and extent of the endangerment that EPA has found. A high risk/high magnitude endangerment finding might justify one level of regulation, while a different finding might justify a different level.

[491] Endangerment Finding, 74 Fed. Reg. at 66,507-08.

[492] *Id.* at 66,515-16.

[493] *Id.*

Thus, the question that EPA must answer at the endangerment phase is not just "endangerment, yes or no?," but specifically what type of endangerment, assuming endangerment is found at all. The regulatory consequences of EPA's answer could be enormous.

In sum, the revelations in the CRU material mean that EPA must reconsider its Endangerment Finding no matter what level of legal discretion the Agency has.

B. Because of the Taint Created by the CRU Material, EPA Should Utilize Formal Rulemaking in Reconsidering Its Endangerment Finding

An on-the-record proceeding is necessary to rectify the substantial flaws in the process that EPA has employed, flaws that stem from the abuses infecting the studies on which the Endangerment Finding is principally based. In light of EPA's wholesale adoption of those studies, it is also the only way to ensure that EPA fulfills its obligation to engage in transparent decision-making that provides interested parties with a meaningful opportunity to be heard.

EPA has already rejected the request of one commenter for evidentiary hearings. The CRU material now requires the Agency to reconsider that denial.

1. EPA Has the Authority to Convene an Evidentiary Hearing

EPA has the authority to provide the additional procedural protections requested here.[494] "Agencies are always free to provide procedural safeguards greater than those required by statute or by the Constitution," and such "additional procedures . . . provide significant benefits to the public and to the agency by permitting more thorough consideration of the issue before the

[494] The Clean Air Act does not expressly require an on-the-record proceeding, *United States v. Florida East Coast Railway Co.*, 410 U.S. 224 (1973), but it also does not preclude one. The Agency has the authority to adopt an on-the-record process of its own volition, in accord with its own promises of transparency and scientific integrity. Section 307(d)(1), which states that the "provisions of section 553 through 557 and section 706 of Title 5 shall not, except as expressly provided in this subsection, apply to actions to which this subsection applies," cannot be interpreted to mean that EPA is *prohibited* from voluntarily applying such procedures, but only that EPA does not have a statutory obligation to use such procedures. It does not limit EPA's discretion in deciding whether to do so.

agency."[495] Employing more formal procedures would enable EPA to avoid the pitfalls of "entirely fail[ing] to consider an important aspect of the problem, offer[ing] an explanation for its decision that runs counter to the evidence before the agency, or is so implausible that it could not be ascribed to a difference in view or the product of expertise" each of which would be grounds for reversal of an agency decision.[496]

There is no reason that EPA cannot do an on-the-record proceeding here. EPA is fully equipped to do such a proceeding, and does so in a number of contexts under a wide variety of statutes. *See* EPA Consolidated Rules of Practice, 40 C.F.R. § 22.3(a) (defining "hearing" as "an evidentiary hearing on the record").[497]

2. An Evidentiary Hearing is Warranted under Applicable Legal Standards

Case law and other authoritative guidance make clear that an evidentiary hearing on this Petition is warranted. In *Vermont Yankee Nuclear Power Corp. v. Natural Resources Defense Council, Inc.*, 435 U.S. 519, 524, 543 (1978), the Supreme Court noted that, although courts should not routinely dictate agency decisional processes, "extremely compelling circumstances" "would …justify a court in overturning agency action because of a failure to employ procedures beyond those required by statute."[498]

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[495] Richard Pierce, et. al, ADMINISTRATIVE LAW 353 (5th ed. 2009).

[496] *MVMA v. State Farm Auto Ins.*, 463 U.S. 29, 63 (1983).

[497] *See* http://www.epa.gov/oalj/index.htm (describing role of EPA administrative law judges); http://www.epa.gov/oalj/statutes.htm (listing statutes administered by EPA administrative law judges, which include the Clean Air Act).

[498] *See also People v. United States*, 666 F.2d 1066, 1083 (7th Cir. 1981) (finding extremely compelling circumstances and that, as a result, "the parties should have been afforded the right of cross-examination with regard to the supplementary evidence"); *Nat'l Wildlife Federation v. Marsh*, 721 F.2d 767, 784-86 (11th Cir. 1983) (requiring an agency to follow a procedure that was not required by statute, but that the agency had followed in other cases).

The Supreme Court later made clear in *Motor Vehicles Manufacturers Association,* 463 US. at 50, that its decision in *Vermont Yankee* was no "talisman under which any agency decision is by definition unimpeachable." Instead, courts must be "assured" that the agency's process "as a whole and in each of its major parts provides a degree of public awareness, understanding, and participation commensurate with the complexity and intrusiveness of the resulting negotiations."[499]

The question of whether to apply an "on the record" process on reconsideration has a ready analogy to the principle that agencies normally possess discretion to select their procedural mode *i.e.*, to decide whether to express their delegated powers by way of rulemaking or adjudication.[500]

This principle from *Bell Aerospace and Chenery II*, however, is not a license to agencies to make an unfettered or arbitrary choice between rulemaking and adjudication. "[T]here may be situations where the Board's reliance on adjudication would amount to an abuse of discretion or a violation of the Act . . . ."[501] What an agency's ability to use its informed discretion to proceed way of formal rulemaking means, taken together with the *Chenery* principle, is that agencies must select the right procedural tool to use for the precise administrative problem at hand. Agencies have some discretion in choosing the right tool, but not absolute discretion.

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[499] *Weyerhaeuser Co. v. Costle*, 590 F.2d 1011, 1024 n.11 (D.C. Cir. 1978); *see also Chicago, Milwaukee, St. Paul & Pac. R.R. Co. v. United States*, 585 F.2d 254, 263 n.15 (1978) (holding summary procedures without adequate notice was arbitrary and capricious and, though leaving the precise procedures to be used to the agency's discretion, "requir[ing] that there be an appropriate exercise of that discretion")

[500] *See, e.g., NLRB v. Bell Aerospace Co.*, 416 U.S. 267, 294 (1974) ("The views expressed in *Chenery II* and *Wyman-Gordon* make plain that the Board is not precluded from announcing new principles in an adjudicative proceeding and that the choice between rulemaking and adjudication lies in the first instance within the Board's discretion."); *SEC v. Chenery Corp.*, 332 U.S. 194, 203 (1947) ("[T]he choice made between proceeding by general rule or by individual, ad hoc litigation is one that lies primarily in the informed discretion of the administrative agency.").

[501] *Bell Aerospace*, 416 U.S. at 294.

The Administrative Conference of the United States ("ACUS")[502] has issued authoritative guidance on a variety of regulatory matters, and many of its recommendations have been adopted by agencies. It has recommended that agencies provide on-the-record proceedings when: (1) scientific or technical issues are "complex;" (2) the problem posed is so "open-ended" that the agency would benefit from diverse views; and (3) the costs errors may pose are "significant."[503]

a. The Scientific and Technical Issues are Highly Complex

The ACUS criteria are readily satisfied here. First, it cannot seriously be doubted that "the scientific, technical or other data relevant" to the Endangerment Finding are complex. The range of scientific issues that must be considered in assessing carbon dioxide (as well as the mix of six gases the Endangerment Finding identifies), their role in climate change, and the impacts therefrom is extraordinarily broad. Questions of attribution pose issues of daunting complexity, as only a few examples will suffice to demonstrate: What was the duration, magnitude and geographic scope of the Medieval Warm Period? Does it not call into question the notion that late-20th century warming was unprecedented, and primarily attributable to anthropogenic causes? What conclusions can responsibly be drawn from tree ring data? What significance attaches to the absence of a fingerprint in the vertical column of the tropical troposphere that was predicted by every climate model, but not borne out by actual observed data? To what extent do the activities revealed by the CRU emails – manipulation or concealment of raw data, refusal to produce it upon request, orchestration of the timing of publications, steering the peer review

[502] ACUS is a bipartisan independent agency and advisory committee. Administrative Conference Act, Pub. L. No. 88-499, 78 Stat. 615 (1964). It closed in 1995 when its funding was eliminated, but was reauthorized in 2004 and awaits funding. *See* Final Listing of Recommendations and Statements Regarding Administrative Practice & Procedures 60 Fed. Reg. 56,312 at 56,312-56,316 (Nov. 8, 1995); Pub. L. No. 108-401, 118 Stat. 2255. Its principal role was to develop recommendations for improving federal agency procedures for administering programs, including recommendations for "regulatory activities and other Federal responsibilities may be carried out expeditiously in the public interest." Administrative Conference Act § 2(e).

[503] 1 C.F.R. § 305.76-3(1) (1993).

process to achieve foreordained conclusions (and in some instances converting "peer" review into a de facto "self" review) – require wholesale reassessment of conclusions underlying the Endangerment Finding? In considering any one or all of these issues, the Agency must necessarily confront substantial questions concerning the quality of the relevant data, the integrity of the scientific processes by which it was analyzed, and the reliability of the conclusions drawn from those analyses.

b. The Open-Ended, Long-Term Nature of Climate Change Underscores the Value of Evidentiary Proceedings Airing Diverse Viewpoints

The Endangerment Finding also easily satisfies the second criterion: the problem at issue is so open-ended that EPA would profit from receiving diverse public views. Climate change is a long-term issue; no step taken today will resolve it tomorrow, in a year, or in ten years. EPA acknowledged as much in the proposed Endangerment Finding itself. There, EPA stated that it "took the approach that the timeframe under consideration should be consistent with the timeframe over which greenhouse gases may influence the climate (i.e., observed effects and projected effects over the next several decades and indeed at least for the remainder of this century)**.**"[504] In light of EPA's complete reliance on a scientific literature survey, the significant deficiencies in that literature demonstrated in the foregoing sections of this petition, and a peer review process that is in fact a self-review process, EPA would greatly benefit from receiving additional scientific input, and, in particular, would benefit from receiving it through an on-the-record proceeding.

[504] Proposed Endangerment and Cause or Contribute Findings for Greenhouse Gases Under Section 202(a) of the Clean Air Act. 74 Fed. Reg. at 18,889.

c. The Costs Of Contemplated Regulation Are Significant

The costs of the Endangerment Finding are likely to be significant, readily satisfying the third ACUS criterion. The Endangerment Finding will inevitably trigger the Administrator's need to promulgate numerous regulations on nearly every sector of the economy, including many heretofore not subject to regulation. This is confirmed by comments from the Secretaries of Agriculture, Commerce, Transportation and Energy Departments that accompanied the ANPR. Those comments, (along with similar comments from the Council of Economic Advisors, the U.S. Small Business Administration, and OMB) condemned the effort to issue an Endangerment Finding because it was based upon incorrect basic assumptions about the costs and benefits of regulation in this area, and the fact that the regulatory proposals will "harm" this country's competitiveness.[505]

3. An Evidentiary Hearing Will Ensure Appropriate Scrutiny of the Scientific Evidence Underlying The Endangerment Finding

The procedural path that EPA followed in adopting the Endangerment Finding has yielded an administrative record that is wholly inadequate. Especially given the inherent gravity of the issues addressed in the Endangerment Finding, the certain prospect of judicial review demands the creation of a record with scientific integrity. That review requires a court "to engage in a substantial inquiry ... [a] probing, thorough, in depth-review."[506] An adequate record of an agency's decisionmaking is necessary to determine "whether the decision was based on a consideration of relevant factors and whether there has been a clear error of judgment."[507]

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[505] Letter to Administrator Susan E. Dudley, 73 Fed. Reg. 44,359-61.

[506] *Citizens to Preserve Overton Park v. Volpe*, 401 U.S. 402, 415 (1971); *see also Ackerman v. United States*, 324 F. Supp. 2d 1, 5 (D.D.C. 2004) (applying Overton Park); *Gonzalez v. Department of State*, 135 F. Supp. 2d 193, 196 (D.D.C. 2001).

[507] *Overton Park*, 401 U.S. at 416.

Convening an on-the-record proceeding, rather than merely relying on paper filings as EPA has chosen, would better ensure that results reached by EPA reflect scientific truths. By analogy, when the judiciary is called upon to ascertain factual truths through consideration of empirical scientific data, it uses adversarial testing of evidence and not paper filings. On-the-record processes, such as those proposed here, better ensure both transparency and scientific integrity. Advocacy on both sides of a question in real time permits a responsive exchange of information that cuts to the heart of a matter and tests the veracity of views being advanced. Moreover, "cross-examination has always been considered a most effective way to ascertain truth."[508]

Courts have recognized the "critical role" the on-the-record process can play in agency decision-making by "clarify[ing] the issues and positions being considered at the agency level."[509]

There can be no question that the science here would benefit from rigorous testing through cross-examination. Adversarial procedures such as cross-examination have uncovered doubts, weaknesses, and contradictions on scientific issues in other proceedings before the Agency.[510] As shown above, there are numerous scientific assertions embodied in the Endangerment Finding and TSD that are controverted. Only an adversarial process conducted on

[508] *Watkins v. Sowders*, 449 U.S. 341, 348 (1981); see also *Kentucky v. Stincer*, 482 U.S. 730, 736 (1987) ("The opportunity for cross-examination . . . is critical for ensuring the integrity of the fact-finding process.").

[509] *U.S. Lines,Inc. v. FMC*, 584 F.2d 519, 542 (D.C. Cir. 1978); *People v. United States*, 666 F.2d 1066 (7th Cir. 1981) ("The history of the proceedings before the record was supplemented shows that the opportunity for cross-examination was critical in achieving an accurate determination of the facts.").

[510] *See* http://www. regulations.gov/fdmspublic/component/main?main=DocumentDetail&o=090000648 024979b (last visited June 20, 2009); Alliance Hr'g Presentation, EPA-HQ-OAR-2006-0173-0412.4 (May 30, 2007), *available at* http://wwww.regulations.gov/fdmspublic/ContentViewer?objectId=090000648024979b& disposition=attachment&contentType=ppt8 (last visited June 20, 2009).

the record would enable a true testing of the data at issue and reduce the risk of errors, and assure the public of a decision with scientific integrity.

In its TSD, for example, EPA elected to rely on secondary sources for which it has had no real scientific testing of any kind. It has used a rulemaking process that effectively permits no replies and no rebuttals. This process has thus admitted of no responsive thrust and parry about those secondary sources, or the propriety of their methods and use of the data. Yet, there is significant reason to question those sources and data in a number of respects. As discussed at length in earlier sections of this petition, the CRU materials place substantial doubt on the integrity of much of the analysis comprising the studies, especially the IPCC report, on which EPA has chosen to rely. By conducting this process as it has chosen, EPA has not simply opened the door to errors, it has effectively invited them. Given the number and magnitude of the consequences that flow from the Endangerment Finding as adopted, those errors will have serious consequences for the country.

4. EPA May Not Make a Public Health Endangerment Finding on the State of the Record as It Now Exists

During the comment period, numerous commenters maintained that EPA may not make an endangerment finding as to indirect health impacts that are really welfare impacts. Commenters argued that EPA could not find that GHGs endanger public health, since the mechanism by which the asserted impacts to health occur is through climate change, which is a welfare impact.[511]

[511] *See* discussion at Endangerment Finding, 74 Fed. Reg. at 66,526-29.

EPA rejected that view and stated that the CAA provides it with discretion to make the health-based endangerment finding that it made here.[512] EPA's discussion of this issue, however, lacks an essential element. EPA fails to come to grips with the fact that CO_2 is a benign, naturally-occurring gas that is necessary for life on earth. This is what distinguishes it from the traditional air pollutants that EPA regulates, where the public health danger is through direct inhalation or exposure. Unlike traditional pollutants, which tend to endanger public health or welfare almost by definition, CO_2 by its nature is beneficial to life, even at enhanced atmospheric levels. As shown in Peabody's comments on the Endangerment Finding proposal, a large volume of scientific literature shows that enhanced levels of CO_2 produce a wide variety of positive impacts for plant life, including agricultural crops. Since perhaps the most critical public health and welfare issue in the world is the need to grow more food without disturbing ecologically sensitive lands, enhanced levels of CO_2 produce unquestioned health and welfare benefits.

Of course, too much of even a benign substance can create a danger, but that doesn't mean that such substance is a danger to the public health or welfare, either directly or indirectly. For instance, one can choke to death on too much water, but that does not make water a pollutant that endangers the public health.

Moreover, EPA's argument that enhanced levels of CO_2 in the atmosphere indirectly endanger the public health requires proof of a long chain of, at best, uncertain cause and effect relationships. For instance, to prove its contention that GHGs endanger public health from deaths from heat waves, EPA must demonstrate how GHGs interact in the atmosphere to create

[512] *Id.*

warming, it must show the extent of that warming in particular locales, it must show that such warming will be sufficient to cause increases in mortality that would not otherwise have occurred, and it must show that such mortality increases are not offset by reductions in cold-related deaths. EPA itself recognizes the "uncertainties" in the evidence in this regard.[513]

Yet despite these uncertainties, EPA relied on IPCC reports that were not prepared in accordance with U.S. data quality standards and therefore do not meet U.S. standards of reliability. Thus, EPA's attempt to transform a benign naturally-occurring substance into a dangerous air pollutant is based on evidence that it should never have used in the first place.

[513] *Id.* at 66,526.

X.

CONCLUSION

For all of the foregoing reasons, EPA must reconsider its Endangerment Finding. EPA should issue public notice of reconsideration and take comment on the effect on the Endangerment Finding of the CRU material and other information set forth in this document. EPA should also convene full evidentiary hearings as a part of reconsideration.

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Respectfully submitted.

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