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MAKING EPA GREAT AGAIN

HEARING

BEFORE THE

COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY
HOUSE OF REPRESENTATIVES

ONE HUNDRED FIFTEENTH CONGRESS

FIRST SESSION

February 7, 2017

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MAKING EPA GREAT AGAIN

TUESDAY, FEBRUARY 7, 2017

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY,
Washington, D.C.

The Committee met, pursuant to other business, at 11:04 a.m., in Room 2318, Rayburn House Office Building, Hon. Lamar Smith [Chairman of the Committee] presiding.

LAMAR S. SMITH, Texas
CHAIRMAN

EDDIE BERNICE JOHNSON, Texas
RANKING MEMBER

Congress of the United States
House of Representatives
COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY
2321 RAYBURN HOUSE OFFICE BUILDING
WASHINGTON, DC 20515-6301
(202) 225-6371
www.science.house.gov

Full Committee

Making EPA Great Again

Tuesday, February 7, 2017
11:00 a.m. – 1:00 p.m.
2318 Rayburn House Office Building

Witness

The Honorable Jeffrey R. Holmstead, Partner, Bracewell & Giuliani

Dr. Kimberly White, Senior Director, Chemical Products and Technology, American Chemistry Council

The Honorable Rush Holt, CEO, American Association for the Advancement of Science

Dr. Richard Belzer, Independent Consultant

U.S. HOUSE OF REPRESENTATIVES
COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY

HEARING CHARTER

Tuesday, February 7, 2017

TO: Members, Committee on Science, Space and Technology

FROM: Majority Staff, Committee on Science, Space, and Technology

SUBJECT: Full Committee Hearing: "*Making EPA Great Again*"

The Committee on Science, Space and Technology will hold a hearing titled *Making EPA Great Again* on Tuesday, February 7, 2017, at 11:00 a.m. in Room 2318 of the Rayburn House Office Building.

Hearing Purpose:

The purpose of this hearing is to examine the Environmental Protection Agency's process for evaluating and using science during its regulatory decision making activities. Witnesses will discuss how EPA can pursue environmental protection and protect public health by relying on sound science.

Witness List

- **The Honorable Jeffrey Holmstead**, Partner, Bracewell LLP
- **Dr. Kimberly White**, Senior Director, Chemical Products and Technology, American Chemistry Council
- **The Honorable Rush Holt**, CEO, American Association for the Advancement of Science
- **Dr. Richard Belzer,** Independent Consultant

Staff Contact

For questions related to the hearing, please contact Majority Staff at 202-225-6371.

Chairman SMITH. The Committee on Science, Space, and Technology will come to order. Without objection, the Chair is authorized to declare recesses of the Committee at any time.

Welcome to today's hearing entitled "Making the EPA Great Again." I'll recognize myself for an opening statement and then the Ranking Member.

Today, we will examine how the Environmental Protection Agency evaluates and uses science in its regulatory decision-making process.

Sound science should be at the core of the EPA's mission. Legitimate science should underlie all actions at the Agency, from research to regulations, and be an integral part of justifying their actions. Unfortunately, over the last eight years, the EPA has pursued a political agenda, not a scientific one.

Time and again, we saw the EPA under the Obama Administration propose regulations that would have no significant impact on the environment. For example, the so-called Clean Power Plan, proposed by the EPA last June, set impossible targets for carbon emissions. Yet even EPA data shows that this regulation would only eliminate a miniscule amount of global carbon emissions and would reduce sea level rise by only 1/100 of an inch. In fact, the EPA has proposed some of the most expensive and expansive and ineffective regulations in history.

The rules proposed and finalized by the EPA placed heavy burdens on American families. Often, huge costs were shouldered by the taxpayer with little to show for it. And the EPA routinely relied on questionable science based on nonpublic information that could not be reproduced, a basic requirement of the scientific method. Americans deserve to see the science for themselves. If the EPA had nothing to hide, why didn't it make the scientific data it used for its regulations publicly available? What was the EPA hiding?

The Committee conducted oversight of EPA's use of suspect science to justify its claims. Our hearings culminated in legislation that required the EPA to make its data publicly available.

With the transition to a new Administration, there is now an opportunity to right the ship at the EPA and steer the Agency in the right direction. The EPA should be open and accountable to the American people and use legitimate science. Though ignored by the previous Administration, the EPA does have internal processes to ensure this accountability. The internal review process at the EPA should be restored and strengthened.

The Science Advisory Board provides critical feedback to the EPA on its proposals, but in recent years, SAB experts have become nothing more than rubberstamps who approve all of the EPA's regulations. The EPA routinely stacks this board with friendly scientists who receive millions of dollars in grants from the federal government. The conflict of interest here is clear.

Fortunately, the EPA can once again become an agency that is credible and respected. Simple changes, such as eliminating conflicts of interests, adding more balanced perspectives, and being more transparent can go a long way to restoring the Agency's credibility.

In recent years, the EPA has sought to regulate every facet of Americans' way of life. Instead, we should invest in research and

development and let technology lead the way. Far too often, the EPA has deliberately used its regulatory power to undercut American industries and advance a misguided political agenda that has minimal environmental benefit. The new Administration has the opportunity to let technology and innovation protect our environment without government mandates that impose costly and unnecessary regulations on the American people. The EPA should focus on environmental policies that can be justified and are based on good science.

Lastly, recent news stories report that another agency, NOAA, tried to deceive the American people by falsifying data to justify a partisan agenda. A senior scientist at NOAA has questioned the scientific integrity of a study written by Tom Karl while at NOAA that claimed that there was no stop in global warming from 1998 to 2013. This official has provided evidence that Karl "had his thumb on the scale" throughout the entire process. The Karl study was published in Science, the journal overseen by the American Association for the Advancement of Science.

In light of this new information, AAAS and Science should retract the Karl study. The Committee will continue our investigation of NOAA's refusal to provide the Committee with responsive documents on this subject.

It is clear that the Committee's investigation is justified. Although NOAA, AAAS, and others attempted to block the Committee's efforts, our goal remains to ensure that the scientific process funded by the American taxpayer is in fact open and honest.

[The prepared statement of Chairman Smith follows:]

COMMITTEE ON
SCIENCE, SPACE, & TECHNOLOGY
Lamar Smith, Chairman

For Immediate Release
February 07, 2017

Media Contact: Kristina Baum
(202) 225-6371

Statement of Chairman Lamar Smith (R-Texas)
Making EPA Great Again

Chairman Smith: Today we will examine how the Environmental Protection Agency evaluates and uses science in its regulatory decision making process.

Sound science should be at the core of the EPA's mission. Legitimate science should underlie all actions at the agency, from research to regulations, and be an integral part of justifying their actions.

Unfortunately, over the last eight years, the EPA has pursued a political agenda, not a scientific one.

Time and again, we saw the EPA under the Obama administration propose regulations that would have no significant impact on the environment.

For example, the so-called Clean Power Plan, proposed by the EPA last June, set impossible targets for carbon emissions.

Yet even EPA data shows that this regulation would only eliminate a miniscule amount of global carbon emissions and would reduce sea level rise by only 1/100th of an inch.

In fact, the EPA has proposed some of the most expensive and expansive and ineffective regulations in history.

The rules proposed and finalized by the EPA placed heavy burdens on American families. Often, huge costs were shouldered by the taxpayer with little to show for it. The previous EPA's regulations were all pain and no gain.

And the EPA routinely relied on questionable science based on nonpublic information that could not be reproduced, a basic requirement of the scientific method.

Americans deserve to see the science for themselves. If the EPA has nothing to hide, why not make the scientific data it uses for its regulations publically available? What was the EPA hiding?

This Committee conducted oversight of EPA's use of suspect science to justify its claims. Our hearings culminated in legislation that required the EPA to make its data publicly available.

This year we will pursue similar legislative remedies and hold the EPA accountable to the American people.

With the transition to a new administration, there is now an opportunity to right the ship at the EPA and steer the agency in the right direction.

The EPA should be open and accountable to the American people and use legit science.

Though ignored by the previous administration, the EPA does have internal processes to ensure this accountability. The internal review process at the EPA should be restored and strengthened.

The Science Advisory Board (SAB) provides critical feedback to the EPA on its proposals. But in recent years SAB experts have become nothing more than rubberstamps who approve all of the EPA's regulations.

The EPA routinely stacks this board with friendly scientists who receive millions of dollars in grants from the federal government. The conflict of interest here is clear.

Fortunately, the EPA can once again become an agency that is credible and respected. Simple changes, such as eliminating conflicts of interests, adding more balanced perspectives and being more transparent can go a long way to restoring the agency's credibility.

In recent years, the EPA has sought to regulate every facet of Americans' way of life. Instead, we should invest in research and development and let technology lead the way.

Far too often the EPA has deliberately used its regulatory power to undercut American industries and advance a misguided political agenda that has minimal environmental benefits.

The new administration has the opportunity to let technology and innovation protect our environment without government mandates that impose costly and unnecessary regulations on the American people.

The EPA should not pick winners and losers by regulating entire sectors of our economy. Instead, the EPA should focus on environmental policies that can be justified and are based on good science. Americans deserve nothing less.

Lastly, recent news stories report that another agency, NOAA, has deceived the American people by falsifying data to justify a partisan agenda.

A senior scientist at NOAA has questioned the scientific integrity of a study written by Tom Karl while at NOAA that claimed that there was no stop in global warming from 1998-2013.

This official has provided evidence that Karl "had his thumb on the scale" throughout the entire process. The Karl study was published in Science, the journal overseen by the American Association for the Advancement of Science (AAAS), an organization run by Rush Holt.

In light of this new information, it seems to me that AAAS and *Science* should retract the Karl study. The Committee will continue our investigation of NOAA's refusal to provide the Committee with responsive documents on this subject.

It is clear that the Committee's investigation into this matter was justified. While NOAA, AAAS, and others attempted to block the Committee's efforts – our goal remains to ensure that the scientific process funded by the American taxpayer is open and honest.

###

Chairman SMITH. That concludes my opening statement, and the Ranking Member, Ms. Johnson, is recognized for hers.

Ms. JOHNSON. Thank you very much, Mr. Chairman, and let me thank our witnesses for being here today. In particular, I'd like to thank our former colleague in the House, Dr. Rush Holt, a scientist, for being here to share his unique perspective.

I would also like to welcome to the Committee our new colleagues on both sides of the aisle. I stated at our organizational meeting this morning it is my hope that we will be able to find common ground together on important issues this Congress.

With that said, I'm disappointed but not really surprised our very first hearing of this Congress will be focused on attacking the Environmental Protection Agency, as was so often the theme of our hearings the last Congress.

I would also note that, of the witnesses invited by the majority to testify today, we have a lobbyist for the industry, a representative from an industry trade group, and a consultant for the industry. That is not a panel likely to produce an objective examination of EPA's activities.

The efforts by some to undermine how the EPA and other federal agencies use science threatens our economy, threatens public health, threatens the environment, threatens public confidence in our government. This is especially true when such efforts rely on biased, incomplete, and misleading information, alternative facts, if you will, in an attempt to advance a probably false narrative against EPA. Regulatory activity to protect public health and the environment should be supported by robust analyses of the best available scientific evidence. That is what EPA does. Policies geared toward preemptively limiting scientific input into the process undermine EPA's ability to take justifiable actions to protect the American public. Questioning the credibility of the scientific process, casting doubt on the scientific research used by EPA, or selectively limiting what resources—what sources of scientific information EPA may consider jeopardizes the effectiveness of the only government agency specifically tasked to protect human health and the environment.

Simply put, limiting the science EPA uses only serves to limit the actions EPA may take to protect public health and the environment. I hope that my colleagues will listen today with a critical ear and ask themselves whether they want to support policies that will harm future generations instead of empowering them, remove public health safeguards instead of strengthening them, and reverse the progress made over the last 40 years instead of working to find a constructive path forward.

Thank you, Mr. Chairman. I yield back.

[The prepared statement of Ms. Johnson follows:]

OPENING STATEMENT
Ranking Member Eddie Bernice Johnson (D-TX)

House Committee on Science, Space, & Technology
"Making EPA Great Again"
February 7, 2017

Thank you Mr. Chairman, and thank you to the witnesses for being here today. In particular, I want to thank my former House colleague, Dr. Rush Holt, for being here to share his unique perspective. I would also like to welcome to the Committee our new Colleagues from both sides of the aisle. As I stated at our organizational meeting this morning, it is my hope that we will be able to find common ground work together on important issues this Congress.

With that said, I am disappointed, but not really surprised, that our very first hearing this Congress will be focused on attacking the Environmental Protection Agency, as was so often the theme of our hearings last Congress. I would also note that, of the witnesses invited by the Majority to testify today, we have a lobbyist for industry, a representative from an industry trade group, and a consultant for industry. That is not a panel likely to produce an objective examination of EPA's activities.

The efforts by some to undermine how the EPA, and other federal agencies, use science threatens our economy, threatens public health, threatens the environment, and threatens public confidence in our government. This is especially true when such efforts rely on biased, incomplete, and misleading information—"alternative facts" if you will—in an attempt to advance a provably false narrative against the EPA.

Regulatory activity to protect public health and the environment should be supported by a robust analysis of the best available scientific evidence, and that is what EPA does. Policies geared towards preemptively limiting scientific input into this process undermine EPA's ability to take justifiable actions to protect the American public. Questioning the credibility of the scientific process, casting doubt on the scientific research used by EPA, or selectively limiting what sources of scientific information EPA may consider jeopardizes the effectiveness of the only government agency specifically tasked to protect human health and the environment.

Simply put, limiting the science EPA uses only serves to limit the actions EPA may take to protect public health and the environment.

I hope that my colleagues will listen today with a critical ear, and ask themselves whether they want to support policies that will harm future generations instead of empowering them, remove public health safeguards instead of strengthening them, and reverse the progress made over the last 40 years, instead of working to find a constructive path forward.

Thank you, I yield back.

Chairman SMITH. Okay. Thank you, Ms. Johnson.

And I'll proceed to introduce our witnesses today. Our first witness is the Hon. Jeffrey Holmstead, a partner at Bracewell. Mr. Holmstead is one of the country's leading air quality lawyers and heads the Environment Strategies Group at Bracewell. He previously served as the Assistant Administrator at the EPA for the Office of Air and Radiation. He also served on the White House staff as Associate Counsel to former President George H.W. Bush. Mr. Holmstead received his bachelor's degrees in economics and English from Brigham Young University and his law degree from Yale.

Our second witness today is Dr. Kimberly White, Senior Director in the Chemical Products and Technology Division of the American Chemistry Council. For the past five years, Dr. White has served as a scientific advisor to industry for the development and execution of research to assess chemical hazards. She also has worked to identify emerging issues and trends in science policy and risk evaluation. Dr. White received her bachelor's and master's degrees in biology and a Ph.D. in environment toxicology from Texas Southern University.

Our third witness is the Hon. Rush Holt, CEO of the American Association for the Advancement of Science. Dr. Holt has held positions as a teacher, scientist, administrator, and policymaker. From 1987 to 1998 he was Assistant Director of the Princeton Plasma Physics Laboratory, a Department of Energy national lab. Dr. Holt served for 16 years as a Member of the U.S. House of Representatives representing New Jersey's 12th Congressional District. In Congress, he was a member of the Natural Resources Committee and the Education and the Workforce Committee. Dr. Holt received his master's degree and Ph.D. in physics from New York University.

Our final witness today is Dr. Richard Belzer, independent consultant and former economist at the Office of Information and Regulatory Affairs. There, he was responsible for reviewing regulatory analyses prepared by the EPA, the Food and Drug Administration, and various components of the Departments of Agriculture, Commerce, Energy, and the Interior. Dr. Belzer has been an independent consultant in regulatory policy, economics, and risk analysis. In addition, he is the President of Regulatory Checkbook and the managing editor of NeutralSource.org. Dr. Belzer received his bachelor's and master's degrees in agricultural economics from the University of California at Davis and his master's and Ph.D. in public policy from Harvard University.

We welcome you all and appreciate your attendance today. And, Mr. Holmstead, we'll begin with your testimony.

TESTIMONY OF THE HONORABLE JEFFREY HOLMSTEAD, PARTNER, BRACEWELL LLP

Mr. HOLMSTEAD. Thank you very much. Good morning. As the Chairman mentioned, I am a partner at the law firm of Bracewell, LLP, but I do want to make it clear I am not appearing on behalf of any clients this morning. I am here to share my own views as a former EPA official and as someone in private practice, who has spent more than 25 years working with EPA on a range of issues.

I want to start by saying I do believe that EPA plays a vital role in protecting human and the environment, but I also know that there are opportunities to improve the way the Agency operates and makes decisions.

This morning, I would like to talk briefly about three reforms that would improve the way science is evaluated and used by the Agency. First, I would like to endorse this Committee's efforts to ensure that the scientific and technical information that is used to support regulatory decisions is publicly available in a manner that allows for independent analysis. This is where the science reform—the Secret Science Reform Act can make important and meaningful reforms.

The only legitimate concern that I have heard about this legislation is that in some cases data that is used to support rulemaking might include medical or other personal information about specific individuals and should not be released because of privacy concerns. I think this is a red herring. Certainly, no one believes that such information should be made public, but I cannot imagine the case in which personal information about any particular individual would be needed to support the types of regulatory actions taken by EPA. I do understand that documentation used in some studies does contain personal information, but names, addresses, and any other identifying information could and must be redacted before any such information is made public.

EPA would certainly incur cost to review data and ensure that personal information is redacted before it is made public, but when regulations impose billions of dollars on consumers and businesses, it is surely appropriate for the government to spend a tiny fraction of this amount to ensure that the scientific information used to support those regulations can be publicly available.

Second, I believe it is important to reform the Science Advisory Board, which is generally known as the SAB, and other advisory groups that provide scientific and technical advice to EPA. Such groups are referred to as independent advisory committees, but the EPA Administrator appoints the members of these groups based on recommendations from EPA staff. Not surprisingly, EPA staffers tend to recommend people who share their views about the issues under consideration. There's no question that the members of the SAB and other advisory panels are well-qualified, but there are other scientists and researchers who are equally well-qualified but who do not get appointed because they may be more skeptical about EPA's views on certain important issues.

The SAB Reform Act would help to ensure that EPA decisions are influenced by experts from a variety of fields and backgrounds that are relevant to the issues under consideration. By focusing on disclosure rather than disqualification, the SAB Reform Act would allow for a wider range of viewpoints while ensuring that any possible conflicts, financial or otherwise, are publicly disclosed.

Third, I would ask this Committee to continue to look at the need to reform EPA's Integrated Risk Information System, which is generally known by the acronym of IRIS. The IRIS program evaluates specific chemicals to which the public may be exposed and then sets reference values that are used in a variety of EPA and state regulatory programs. Unfortunately, outside experts believe

that the IRIS program often overstates the actual risk posed by these specific chemicals.

EPA is inclined to be conservative in making both regulatory decisions and scientific conclusion. For example, if there are three well-conducted—I'm sorry. If there are five well-conducted studies finding that a substance poses very little risk and one that finds a higher risk, EPA will typically place much more weight on the one rather than the five. Some argue that this is appropriate and that EPA should always err on the side of being overly protective. But this is an issue for policymakers, not for scientists. When decisions are made based on overly conservative science, it can mislead the public and impose unnecessary regulatory burdens on society.

As this Committee considers how to improve the way that EPA deals with scientific and technical issues, I hope it will consider potential reforms to the IRIS program as well.

I thank you for giving me the chance to testify this morning, and I look forward to answering any questions that you may have.

[The prepared statement of Mr. Holmstead follows:]

"Making the Environmental Protection Agency Great Again"
Testimony Before the House Science, Space and Technology Committee
Tuesday, February 7, 2017 - 11:00 am
2318 Rayburn House Office Building

I. Introduction

Thank you Chairman, Ranking Member, and distinguished members of the Committee for inviting me to participate in today's hearing.

My name is Jeff Holmstead. I am a partner in the law firm Bracewell LLP and have been the head of the firm's Environmental Strategies Group (ESG) since 2006. For almost 25 years, my professional career has been focused on policy, regulatory, and legal issues arising under the Clean Air Act. From 1989 to 1993, I served in the White House Counsel's Office as Associate Counsel to President George H.W. Bush. In that capacity I was involved in many of the discussions and debates that led to the passage of the 1990 Amendments to the Clean Air Act – and was then deeply involved in the initial efforts to implement the 1990 Amendments. From 2001 to 2005, I was the Assistant Administrator of EPA for Air and Radiation and headed the EPA Office in charge of implementing the Clean Air Act.

When not in the federal government, I have been an attorney in private practice, representing a wide variety of clients on Clean Air Act and other environmental issues. Since I joined Bracewell LLP in 2006, I have worked primarily with companies and trade groups in the energy industry.

This hearing could not be more timely as the change in administration creates an opportune moment for refining the mission of EPA as it seeks to strike the right balance between the costs and benefits of environmental regulations. I have spent the last 25 years of my professional life working on EPA issues, and I can say with confidence that, if we focus on sound science and good regulatory design, we could have the environmental protection we all want at a much lower cost than we have today.

That is why I want to thank the Committee for hosting today's hearing, which I think will shed light on changes that can strengthen the work being done at EPA. To this end, I commend this Committee and its staff for considering the "Secret Science Reform Act" and the "EPA Science Advisory Board Reform Act." I hope that this hearing will push both bills a few steps closer to enactment.

II. The Secret Science Reform Act

No matter one's political views, it is hard to disagree that transparency is an important principle when it comes to the development of public policy and regulations. Transparency not only stays true to our collective democratic ideals but also helps to ensure that well-informed debate occurs before new policies are made or new regulations are promulgated. This is where the Secret Science Reform Act can make important and meaningful reforms.

How can EPA be sure that it is relying upon the best available science when the scientific and technical information used to support its actions cannot be identified and made available to the public? Only when such information is made public can other interested and qualified parties conduct independent analysis and seek to reproduce research results. Transparency not only breeds accountability but also a healthy respect for dialogue and honest debate.

I don't think anyone can object to the basic premise that scientific information used to support regulatory actions should be made public. Former President Obama's memorandum on scientific integrity stated that "there should be transparency in the preparation, identification, and use of scientific and technological information in policymaking."[1] Furthermore, a White House Office of Science and Technology Policy memo, also from the Obama Administration, explains that agencies should expand and promote access to scientific information by making it available online in open formats.[2] The Secret Science Reform Act would overcome bureaucratic hurdles that stand in the way of these principles.

As far as I know, the only legitimate concern that has been raised about this legislation is that, in some cases, data that has been used to support rulemaking might include medical or other personal information about specific individuals and should not be released because of privacy concerns. Certainly, no one believes that it is appropriate for anyone – much less a federal agency – to publicly release such information. But I cannot imagine a case in which personal information about any particular individual or individuals would be needed to support the types of regulatory actions and policies decisions made by EPA. I do understand that documentation used in some studies does contain personal information about some individuals. But names or other identifying information could certainly be redacted before any such information is made public.

Admittedly, EPA would incur costs to review certain data and ensure than personal information is redacted before it is made public. But when regulations impose billions of dollars on consumers and businesses, it is surely appropriate for the government to spend a tiny fraction of this amount to ensure that the scientific information used to support those regulations can be made public.

It is worth noting that EPA itself has recognized that "scientific research and analysis comprise the foundation of all major EPA policy decisions" and that "the Agency should maintain vigilance toward ensuring that scientific research and results are presented openly and with integrity, accuracy, timeliness, and the full public scrutiny demanded when developing sound, high-quality environmental science."[3] These are laudable goals, and the Secret Science Reform Act will ensure that EPA actually lives up to them when it relies on such information to support regulatory decisions.

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[1] See: https://www.gpo.gov/fdsys/pkg/FR-2009-03-11/pdf/E9-5443.pdf

[2] See: http://www.sciencemag.org/news/2010/12/white-house-releases-long-awaited-guidance-scientific-integrity

[3] See: http://ethics.iit.edu/ecodes/node/5537

III. Science Advisory Board Reform Act of 2015

The Science Advisory Board (SAB) Reform Act would also make sensible reforms and increase the likelihood that EPA's regulatory decisions will not only be based on the best data but will be informed by the best possible analysis and interpretation of that data. EPA's Science Advisory Board (SAB) and subsidiary groups like the Clean Air Science Advisory Committee (CASAC) often advise EPA on scientific issues that are important for the Agency's regulatory actions. Such groups are generally known as independent advisory committees, but the EPA Administrator appoints the members of these groups based on recommendations from EPA staff. Not surprisingly, EPA tends to choose people who share EPA's views about the importance of environmental issues. The members of the SAB and other subsidiary groups are well-qualified and have good credentials, but there are other scientists and researchers who are equally well qualified but do not get appointed because they are more skeptical about EPA's views on certain important issues.

The SAB Reform Act would help to ensure that EPA decisions are informed by experts from a variety of fields and backgrounds that are relevant to the issues under consideration. Throughout my professional career, I have seen how serious dialogue among thoughtful people with different perspectives can be used to inform both policy and science. By focusing on disclosure rather than disqualification, the SAB Reform Act would allow for a wider range of viewpoints while ensuring that any possible conflicts -- financial or otherwise – are publicly disclosed.

By requiring EPA to make public a list of nominees to the Board and accept public comments on the nominees, the Act comports with the maxims of transparency outlined above. And by instituting a requirement to balance scientific and technical points of view, the Act helps to ensure that the SAB provides the best advice and insights to EPA as it crafts regulations.

IV. EPA's Integrated Risk Information System (IRIS)

Another EPA program that I believe should receive scrutiny from the Committee is EPA's Integrated Risk Information System ("IRIS"). The IRIS program, which is located within EPA's National Center for Environmental Assessment ("NCEA"), endeavors to "develop impartial toxicity information independent of its use by EPA's program and regional offices to set national standards and clean up hazardous sites."[4] While the IRIS program as a whole is laudable, outside experts believe that it often overstates the actual risk posed by specific chemicals.

EPA is inclined to be "conservative" in making both regulatory decisions and scientific conclusions. For example, if there are 5 studies finding that a substance poses very little risk and one that finds a higher risk, EPA will typically place more weight on the one rather than the five. Some argue that this is appropriate, and the EPA should always err on the side of being overly protective, but this is a decision for policymakers, not for scientists. And when regulatory decisions are made based on overly conservative science, it can have serious effects on the regulated community, sometimes even threatening the viability of industrial facilities that provide important benefits to local workers and communities.

[4] See: https://www.epa.gov/iris/basic-information-about-integrated-risk-information-system

The IRIS Program sometimes relies on very small and statistically limited subgroups to reach conclusions regarding risk of specific chemicals. In some cases, it has even relied on outdated – and poor quality – Russian and Chinese data instead of domestic data relied upon by experts throughout various domestic industries, simply because the foreign studies found risks that the widely used U.S. studies did not. And then, in establishing reference values (or "safe" levels of exposure), the IRIS program relies on additional conservative assumptions.

As a result of these things, NCEA sometimes misleads the public and stokes unnecessary fears, causing serious real-world consequences for facilities that already are struggling to keep their doors open—and their jobs in the United States. The President and many other officials from both political policies have stressed the importance of keeping manufacturing capacity in the United States, but the IRIS program can, in some cases, imperil this important goal. Accordingly, as the Committee contemplates how best to craft transparent, fair, and predictable regulatory processes based upon sound scientific information, the IRIS program, in my view, should be an important part of that broader conversation.

V. Conclusion

Again, I very much appreciate the opportunity to appear before the Committee and hope my testimony will be helpful to you as you seek to shape the strategic direction at EPA. I commend the Committee for its work so far and I respectfully offer my input, as necessary, to you going forward. Thank you and I look forward to answering any questions that the Committee may have.

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Jeff Holmstead is a partner and Head of the Environmental Strategies Group at Bracewell LLP and has repeatedly been recognized by *Chambers USA* as one of the leading environmental lawyers in the country. He has worked on environmental and regulatory issues for more than 25 years – in both the federal government and the private sector. From 1989 to 1993, he worked on the White House staff of President George H.W. Bush, where he was deeply involved in the passage and then the early implementation of the 1990 Amendments to the Clean Air Act. In early 2001, he was nominated by President George W. Bush and confirmed by the Senate to be the Assistant Administrator of EPA for Air and Radiation – a position he held until late 2005.

When not in the government, he has been a lawyer in private practice advising and representing clients on a range of environmental issues. Since he joined Bracewell in 2006, most of his work has been with energy companies and trade associations, including utilities, upstream oil and gas companies, and petroleum refineries. He represents a number of clients dealing with the development, implementation, and enforcement of EPA regulations. Much of work over the last few years has been focused on EPA's efforts to regulate greenhouse gas emissions under the Clean Air Act.

Mr. Holmstead graduated from Yale Law School in 1987 and then served as a law clerk to Judge Douglas H. Ginsburg on the United States Court of Appeals for the DC Circuit. He graduated *summa cum laude* from Brigham Young University in 1984 with degrees in Economics and English. He and his wife are both from Colorado and are the proud parents of four children.

Chairman SMITH. Thank you, Mr. Holmstead.
Dr. White.

**TESTIMONY OF DR. KIMBERLY WHITE,
SENIOR DIRECTOR,
CHEMICAL PRODUCTS AND TECHNOLOGY,
AMERICAN CHEMISTRY COUNCIL**

Dr. WHITE. Good morning, Chairman Smith, and Members of the Committee. My name is Dr. Kimberly White, a scientist with the American Chemistry Council. And I appreciate this opportunity to testify regarding EPA's use of science in its regulatory decision-making process.

The business of chemistry is a critical component for manufacturing safe, high-quality products. ACC member companies and the public rely on science to spur innovation, advance product stewardship, and improve the assessments of chemical risk. Similarly, they expect high-quality science and objective assessment processes to underpin regulatory decisions by the federal government.

ACC has long maintained that EPA chemical assessments can and should reflect the most up-to-date and relevant science regarding potential impacts to human health and the environment from chemical exposures. Although EPA has made efforts to improve its scientific approach, the actual implementation has been slow and often lacking. This has been fundamentally due to the lack of a consistent science-based framework for conducting chemical evaluations. In 2016, Congress passed the Lautenberg Chemical Safety Act or the LCSA, which provides EPA a mandate for use of the best-available science and a weight-of-evidence approach in conducting risk evaluations for both new and existing chemicals. Implementing these new provisions under the LCSA will require significant changes to EPA's scientific evaluation procedures.

Unfortunately, as indicated in EPA's proposed framework rule for risk evaluation, EPA believes existing practices meet the standards of the LCSA. ACC does not support this belief and will continue fostering approaches to advance the technical quality and objectivity of scientific evaluations, particularly by promoting more transparency in both what science is being considered and how that information is interpreted.

My oral testimony today focuses on four areas to improve science evaluations at EPA, and my written testimony provides additional detail and some specific examples.

First, EPA should ensure that its chemical assessments address information needs of decision-makers and are fit for purpose. This includes clearly defining the scope of the evaluation, the methods to be used, and the utility of the evaluation for regulatory decisions. EPA is currently interpreting the LCSA as requiring the Agency to evaluate all uses of a chemical. However, the LCSA clearly indicates that EPA has discretion to select those conditions of use for its scope of their risk evaluation. A clear scoping step allows the Agency to determine if a screening-level assessment identifies risk sufficiently or if a more refined risk evaluation is needed. This approach also enables EPA to meet the stringent deadlines of the LCSA and focus resources on those conditions of use where unreasonable risk cannot be ruled out.

Secondly, scientific evaluations must utilize transparent and consistent criteria for selecting the most relevant, high-quality science and evaluating the evidence to draw conclusions. It is critical that EPA rely on studies of the highest quality, not simply those studies that produce the lowest points of departure or the highest exposure estimates.

Thirdly, EPA should employ a transparent weight-of-evidence framework that integrates evidence from human studies, animal research, and mechanistic data. The LCSA requires risk evaluations to integrate and assess available information on hazard and exposures for the conditions of use. The LCSA also requires EPA to make decisions using a weight-of-evidence approach.

Lastly, EPA must implement an effective peer-review process. Peer review should be independent and objective, allowing for robust engagement with stakeholders to provide a thorough review. It should also include a quality-assurance process that explicitly evaluates whether peer-review recommendations and public comments were completely and adequately addressed.

In conclusion, ensuring that up-to-date, high-quality science underlies EPA decision-making is critical to protecting human health and the environment. This can be achieved by consistent applications of processes throughout EPA to conduct risk evaluations using a weight-of-evidence process, as required by the LCSA.

Thank you for this opportunity to provide testimony, and I look forward to working with members of the Committee to ensure that high-quality science is the foundation for the Agency's decision-making.

[The prepared statement of Dr. White follows:]



Testimony of
Kimberly W. White, Ph.D.
Senior Director, Chemical Products and Technology Division
American Chemistry Council

Making EPA Great Again

House Committee on Science, Space, and Technology
February 7, 2017

Summary

The American Chemistry Council (ACC)[1] appreciates this opportunity to provide testimony on the U.S. Environmental Protection Agency's (EPA) process for evaluating and using science to support regulatory decision making.

The business of chemistry is a critical component for manufacturing safe, high quality products and ACC member companies rely on science to conduct the research necessary to discover new chemistries and identify new applications of existing chemistries. They also rely on science to develop new tools for assessing the potential hazards, exposures and risks of chemical substances. Similarly, they expect high quality, up to date science and relevant reliable assessment processes to underpin regulatory decisions by the Federal government. Reliance on the highest quality, best available science is critical to ensuring public trust. Without it, consumers are at a severe disservice and lose confidence in regulatory decision making, leading to product de-selection that is not supported by science, unwarranted public alarm and unnecessary costs. ACC supports actions to enhance the integration of the best available scientific knowledge and methods as the foundation for regulatory decision making across EPA. We also support advancing the technical quality and objectivity of EPA evaluations, particularly through enhancing transparency in both what science is being considered and how it is being interpreted and integrated.

Over the last 30 years, advances in scientific techniques and knowledge have improved our understanding of how chemicals interact with the human body and the environment. Research programs within industry, academia and government have expanded to investigate the underlying biological processes for chemical interactions and to improve the scientific basis of chemical policies and product stewardship efforts. It is simply not enough to have the science available for use. There must also be a transparent process and a willingness to enable integration of the science into EPA policies and practices. Current processes for evaluating scientific information and conducting chemical assessments at EPA are not always based on transparent, objective or consistent use of the best available science. In recent years, there has been a focus on EPA's Integrated Risk Information System (IRIS) program and addressing deficiencies identified by the National Academy of Sciences (NAS)[2]. These deficiencies are also evident in other EPA chemical assessment programs.

[1] The American Chemistry Council (ACC) represents the leading companies engaged in the business of chemistry. ACC members apply the science of chemistry to make innovative products and services that make people's lives better, healthier and safer. ACC is committed to improved environmental, health and safety performance through Responsible Care®, common sense advocacy designed to address major public policy issues, and health and environmental research and product testing. The business of chemistry is a $797 billion enterprise and a key element of the nation's economy. It is one of the nation's largest exporters, accounting for fourteen percent of all U.S. exports. Chemistry companies are among the largest investors in research and development.

[2] National Academy of Sciences (NAS). NRC (National Research Council). Review of EPA's Integrated Risk Information System (IRIS) Process. Board on Environmental Studies and Toxicology. Division on Earth and Life Studies. Washington, DC: The National Academies Press, 2014. Available at http://www.nap.edu/catalog.php?record_id=18764.

ACC has consistently called upon EPA to improve the design and conduct of its chemical assessments. Recommended improvements have included adoption of consistent and transparent study evaluation methods to determine the quality and reliability of critical studies. We have also encouraged EPA to utilize an improved framework for integrating study results based on a weight of the scientific evidence approach that incorporates modern knowledge of mode of action to establish cause and effect. Furthermore, we have recommended that EPA improve its peer review and accountability practices for addressing both public comments and peer review recommendations. Although EPA has made efforts to identify practices for systematically reviewing the available science and to strengthen its peer review processes, the actual implementation of these practices has been slow and often lacking. This has been fundamentally due to the lack of a consistent science-based framework for conducting chemical evaluations within EPA.

Over the past several years Congress has worked to update and reform the Toxic Substances Control Act (TSCA) and in 2016 passed the Frank R. Lautenberg Chemical Safety for the 21st Century Act (LCSA). The LCSA establishes new requirements for the review of new and existing chemicals manufactured and used to make U.S. products; including requiring use of the best available science and a weight of evidence process to evaluate scientific information. EPA now has a mandate to apply high quality, reliable scientific information while evaluating new chemicals and prioritizing and evaluating the risks of existing chemicals. Implementing these new provisions will require significant changes to EPA's scientific evaluation procedures, particularly for existing chemicals. However, as we have recently seen in EPA's proposed framework rule for risk evaluation, EPA believes that existing practices meet the standards of the LCSA. ACC does not support this belief and we plan to continue to be a constructive partner to both Congress and EPA in identifying approaches that enhance the chemical assessment process. ACC's testimony today outlines four areas that can improve the evaluation of scientific information at EPA:

- Clear framework for conducting the chemical assessment;
- Application of consistent criteria for selecting and evaluating scientific data;
- Transparent and objective integration of scientific evidence; and
- Independent and robust peer review.

I. Clear Framework for Conducting Chemical Assessment

EPA and other federal government agencies conduct chemical assessments to inform risk management decisions. As such, EPA should ensure that the assessments it conducts will address the information needs of decision makers. EPA is tasked with evaluating new chemicals to be manufactured and used to make U.S. products and existing chemicals in the marketplace. As such, any assessment that EPA undertakes should be fit for purpose in order to effectively and efficiently utilize its limited resources. This can help ensure that chemical assessments are based on the best available information and are appropriately scaled and oriented to address relevant questions regarding risk. EPA should also make use of all available science evaluations, including primary scientific literature, grey literatures and reviews, conducted to inform the chemical assessment process and determine information needs. In this initial phase of chemical assessment, EPA can determine if a screening level

assessment will identify and assess risk sufficiently or if a more refined risk evaluation is needed.

EPA is currently interpreting the LCSA as requiring the Agency to evaluate *all* conditions of use of a chemical, regardless of how small, in the risk evaluation. This interpretation is unreasonable and inconsistent with other provisions in the LCSA which, clearly indicate that EPA has discretion to select the conditions of use that it will consider in the scope of a risk evaluation. There are significant questions about EPA's ability to meet the stringent evaluation deadlines of the LCSA if the Agency takes the position that all uses of a substance must be evaluated. A tiered approach, where EPA uses the scoping step to conduct a quantitative screening level analysis, will allow EPA to focus its limited resources on more robust refined risk evaluations for only those conditions of use where unreasonable risks cannot be ruled out.

In order to adequately and effectively evaluate the available science to make timely and science based decisions regarding potential risk from exposures, methods for conducting a chemical assessment must be clearly defined up front. The protocol, developed before the chemical evaluation begins, defines the methodologies that will be used in the assessment. It is made publicly available before the assessment begins and becomes a living document that can be commented upon and modified as needed. The protocol includes: a clear testable question/hypothesis, the planned search strategy (including criteria for inclusion and exclusion of studies), the criteria that will be used for study quality and risk of bias evaluations (including, for example, consideration of study design and confounders), the plan for integrating/synthesizing scientific evidence using a weight of evidence approach, the plan for quantifying and presenting findings, and the plan for peer review of the assessment. Section 6(b)(4)(B) of the LCSA requires EPA to establish, by rule, "a process to conduct risk evaluations." Incorporation of a protocol which includes these important risk evaluation elements is missing from EPA's proposed rule for risk evaluation. Without these elements it is not clear how EPA can meet the LCSA requirements that, for the first time in federal law, provide a statutory requirement mandating best available science and weight of the scientific evidence requirements to inform agency decision making.

In EPA's IRIS program there are similar concerns regarding scientific evaluation procedures. These concerns have been well articulated by the NAS. Assessment approaches also appear to be hampered by a lack of coordination among programs regarding the chemical assessments being undertaken and how those assessments can be utilized by other EPA programs. For instance, past assessments by EPA's IRIS program (e.g., n-butanol, trimethylbenzenes) did not seem to consider data developed by other EPA program offices in previous chemical assessments. It also has not been clear why the TSCA Work Plan chemicals program, within the Office of Pollution Prevention and Toxics (OPPT), at times evaluated the same chemicals that the IRIS program evaluated.

ACC recommends that EPA identify a consistent framework for conducting chemical assessments, including the methods to be utilized in the assessment and the utility of the assessment for regulatory decision making, prior to initiating the assessment These practices should be consistent with requirements outlined in section 26 of the LCSA which, requires

EPA to improve the quality, transparency and relevance of the scientific information, approaches, methods, protocols, and models that are used to evaluate chemical risks. EPA must additionally ensure that the information used is reasonable for and consistent with the intended use of the information.[3] When assessments are being conducted to inform significant rulemakings, EPA must make certain that these important standards are being met.

II. Application of Consistent Criteria for Selecting and Evaluating Scientific Data

Scientific evaluations must utilize transparent and consistent criteria for selecting the most relevant scientific information and evaluating the evidence to draw scientifically defensible conclusions to support decision making. In particular, a systematic approach can ensure that EPA is using clear procedures and protocols to develop reproducible and scientifically sound assessments. It is critical that EPA rely on the studies of the highest quality not simply those studies that produce the lowest points of departure or the highest exposure estimates. A lack of sufficient review of study information may lead to establishing unrealistic risk characterizations and exposure concentrations that are not relevant to actual human exposures. For example, in the Work Plan chemical draft risk assessment of 1-bromopropane, EPA did not provide information regarding the quality of the individual studies. Appendix M of the assessment identifies some quality considerations, but EPA did not provide any information regarding its own findings from its quality review of the individual studies. Additionally, no information was provided to describe how considerations were applied and what constitutes a study of "high quality" or "good quality."[4] Simply choosing the lowest value is not consistent with the best available science approach envisioned under the LCSA. As noted before, this new language will require that EPA make significant changes to its risk evaluation practices.

Given the lack of consistency in evaluating scientific information, EPA should develop, through an open and transparent process, (1) protocols that define the process for the acquisition of the scientific literature including study inclusion/exclusion criteria; and (2) a framework for evaluating studies for quality, reliability and relevance. Notably, the LCSA calls on the EPA risk evaluation process to comply with the best available science provision in Section 26(h), the weight of the scientific evidence provision in 26(i) and the transparency provision in 26(j).

III. Transparent and Objective Integration of Scientific Evidence

Considerable progress has been made over the years to improve understanding of the potential for risk from chemical exposures. In order for the Agency to reach scientifically robust conclusions, it must employ a transparent weight of evidence framework that

[3] Section 26(h)(1) states that the Administrator shall consider "the extent to which the scientific information, technical procedures, measures, methods, protocols, methodologies, or models employed to generate the information are reasonable for and consistent with the intended use of the information."

[4] See Comments of the American Chemistry Council on the TSCA Work Plan Chemical Draft Risk Assessment of 1-Bromopropane , Docket No. EPA-HQ-OPPT-2015-0084, May 9, 2016

integrates evidence from human epidemiological studies, laboratory animal research and mechanistic research. This includes evaluating the strengths and limitations of the human and animal data, understanding the biological significance of responses in animal models and of mechanistic research, and applying current scientific knowledge to extrapolate those findings to humans.

EPA's 2005 Guidelines for Carcinogen Risk Assessment[5] emphasize the importance of weighing all of the evidence—including both studies that provide evidence of an effect as well as those that provide no evidence of an effect—in reaching conclusions about the potential for a chemical to be carcinogenic to humans. The weighing of the evidence includes addressing not only the likelihood of human carcinogenicity, but also the conditions under which such effects might occur. Weighing the scientific evidence entails clearly explaining the kinds of evidence available (e.g., epidemiology, toxicology, mechanistic) and how that evidence fits together in drawing conclusions regarding human relevance and dose-response. Section 6(b)(4)(F)(i) of the LCSA requires risk evaluations to integrate and assess available information on hazards and exposures for the conditions of use of the chemical substance. Additionally, Section 26(i) of the LCSA requires EPA to make decisions using a weight of scientific evidence approach. The Congressional Record clearly describes how a weight of the scientific evidence approach requires the consideration of the strengths, limitations and relevance of each study.[6]

Unfortunately, it has been unclear how the EPA programs apply weight of evidence approaches or how the programs incorporate mode of action information when evaluating the science to reach decisions. There also appears a lack of acknowledgement in some EPA programs regarding science that supports a threshold for safe exposures to a chemical. In 2011, the NAS[7] reviewed the draft formaldehyde IRIS assessment and concluded that EPA had not sufficiently documented methods to identify or evaluate relevant scientific studies; and had not adequately integrated the lines of evidence from the available animal, human, and mechanistic data. The NAS report also called the draft formaldehyde assessment subjective and potentially problematic given the inconsistencies in the available scientific data. The NAS also noted areas where EPA's approaches may not be scientifically justified. For example, the NAS review noted that with regard to the biologically based dose-response (BBDR) model manipulations made by the EPA "...some of the manipulations are extreme, may not be scientifically justified, and should not have been used as the basis of rejection of the use of the BBDR model in its assessment. Model manipulations that yield results that are implausible or inconsistent with available data should be rejected as a basis for judging the utility of the model."

[5] EPA 2005. Guidelines for Carcinogen Risk Assessment. Available at http://www.epa.gov/sites/production/files/2013-09/documents/cancer_guidelines_final_3-25-05.pdf.
[6] See Senate Congressional Record, June 7, 2016 at page S3518, available at: https://www.congress.gov/crec/2016/06/07/CREC-2016-06-07-pt1-PgS3511.pdf.
[7] National Academy of Sciences (NAS). National Research Council (NRC). Review of the Environmental Protection Agency's Draft IRIS Assessment of Formaldehyde. Washington, DC: The National Academies Press, 2011.

In addition to identifying scientific concerns with the formaldehyde IRIS assessment the NAS also identified recurrent problems with EPA's process for evaluating chemicals more broadly. While the EPA is working to address these NAS recommendations, after more than 5 years, the IRIS program is still falling short and has not yet released a final assessment that is fully consistent with these important NAS recommendations. In addition to the IRIS program, and more recently, in a 2016 Work Plan chemical review of 1-bromopropane, EPA had multiple studies for identified hazards, such as reproductive and developmental toxicity, and carcinogenicity. EPA also had multiple exposure studies to consider. However, the Agency failed to apply a weight of evidence approach.

When there are multiple studies available, the only scientifically defensible approach is to weigh the studies by considering study characteristics and determining which studies are of higher quality and should be given greater weight in the assessment. Failure to employ a weight of evidence approach is a critical deficiency that seriously limits any conclusions that can be drawn. To ensure clarity and consistency in the scientific evaluation process, EPA should (1) develop a clear weight of evidence framework to identify and evaluate each stream of evidence, including strengths, limitations, and relevance of each study; and (2) integrate evidence based upon strengths, limitations, and relevance. This approach should be implemented in all programs and codified in EPA's risk evaluation framework regulations under the LCSA.

IV. Independent and Robust Peer Review

Peer review is essential in the evaluation of scientific information to ensure the development of scientifically defensible assessments. It also allows for the review of the underlying assumptions, methodology, criteria, and conclusions reached in the evaluation. EPA currently has several mechanisms to conduct peer review of scientific information including the Science Advisory Board, Science Advisory Panel, NAS contracted review and ad hoc peer review. As outlined in EPA's 2015 Peer Review Handbook,[8] "the success and usefulness of any peer review depends on the quality of the draft work product submitted for peer review, the care given to the statement of the issues or "charge," the match between the peer review draft product and the form of peer review, the match between the peer review draft product and the scientific/technical expertise of the reviewers, and Agency use of peer review comments in the final product."

Unfortunately, peer review processes and approaches are inconsistently applied throughout the Agency, including the selection of peer review panel members and the consideration given to public and peer review comments. During some EPA peer review meetings, the peer reviewers have appeared to be overly deferential to EPA and reluctant to be seen as criticizing EPA staff. We have also seen situations where peer reviewers have suggested discounting a study solely based on the funding source, without any considerations being given to the quality of the study. Also, EPA staff often comment throughout peer review meetings, essentially participating as peers, while industry experts are typically excluded

[8] EPA Peer Review Handbook 4th Edition, 2015. Available at https://www.epa.gov/sites/production/files/2016-03/documents/epa_peer_review_handbook_4th_edition.pdf

from the dialogue. This practice undermines the integrity of the reviewers' role as independent and external to the assessment itself.

A critical element of peer review is also the consideration of public comments. The public plays an important role in the review process by helping identify key scientific information and potential concerns with the assessment being evaluated. Currently, there is no robust consideration of public comments in the peer review process. Reviewers on the EPA Science Advisory Board (SAB) are not given clear advice regarding what it means to "consider" public comments. In fact we have seen SAB chairs ignore public input because they are not required to address it. When this has occurred, SAB staff have not clarified to the peer reviewers that they can and should respond to public input.

In 2013, EPA's IRIS program announced a revised process that included an explicit response to comments step. However, 2016 IRIS assessments of trimethylbenzenes and ammonia and the 2017 ethylene oxide assessment contained no response to public comments in the final documents and only addressed peer review comments. This is a clear departure from EPA's commitment in step 5 of its IRIS process which states that the Agency "Develops a disposition of peer review and public comments and provides these as an appendix to the IRIS assessment."[9] Compounding concerns, the SAB committee reviewing the trimethylbezene assessment also did not respond to public comments, essentially creating a black hole where public comments are provided to the Agency but no clear responses are provided. Peer review should be independent and objective allowing for robust engagement with stakeholders to provide a thorough review. It should also include a quality assurance process that explicitly evaluates whether the peer review recommendations and public comments were completely and adequately addressed.

Conclusion

The incorporation of up to date scientific information, approaches and methods to ensure that EPA decision making is firmly based on high quality science is critical to protecting human health and the environment. This can be achieved by transparent, objective and consistent application of evaluation processes throughout EPA to evaluate and integrate scientific information utilizing a weight of scientific evidence process as required under the LCSA. Further, a robust and independent peer review process must be employed. ACC looks forward to working with members of the Committee to enhance the approaches to ensure that high quality science is the foundation to regulatory decision making regarding potential chemical hazards and risks.

[9] EPA IRIS Process Flow Chart. Available at https://www.epa.gov/sites/production/files/2014-03/documents/iris_process_flow_chart.pdf.

Kimberly W. White is a Senior Director in the American Chemistry Council's Chemical Products and Technology Division where she works in support of scientific research and chemical assessments that are firmly based on up-to-date scientific knowledge, meet the highest standards of scientific inquiry and are evaluated in accordance with the most relevant scientific approaches. For the past 5 years, Dr. White has served as a scientific advisor to industry partners for the development and execution of scientific research to inform chemical hazard assessments. She has also worked to identify emerging issues and trends in science policy and risk evaluation. Dr. White has presented at scientific symposia; collaborated to organize multi-stakeholder workshops to improve the conduct of chemical assessments; and managed scientific research programs. Additionally, Dr. White has coauthored publications on weight of evidence frameworks, problem formulation in chemical assessment and understanding potency information associated with human exposures. She has also been the lead representative in discussions with regulatory and chemical assessment agencies. In her most recent past position, Dr. White served as a Scientific Advisor with the American Petroleum Institute where she managed toxicology research, regulatory response, and product stewardship efforts for the oil and natural gas industry. She has also held positions as an Environmental Manager for Boar's Head Provisions Co., Inc. and as an Environmental Scientist for Resource Management Concepts, where she managed environmental compliance and sustainability efforts. Dr. White possesses B.S. and M.S. degrees in biology and a Ph. D in Environmental Toxicology.

Chairman SMITH. Thank you, Dr. White.
And, Dr. Holt.

TESTIMONY OF THE HONORABLE RUSH HOLT, CEO, AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

Dr. HOLT. Good morning, Chairman Smith and Ranking Member Johnson and esteemed Members of the Committee. Thanks for the opportunity to testify today on behalf of the American Association for the Advancement of Science, the AAAS. AAAS is the largest general science membership society, publisher of the Science family of journals, and our mission is simple: to advance science, engineering, and innovation throughout the world for the benefit of all people.

The hearing today, as I understand it, is general and not about specific bills, so I will talk in general about the use of good science as a basis for policy and regulation.

I'm pleased to note from the title of today's hearing that the Committee acknowledges that the EPA has been great. The success of EPA is really because the environment regulations of past decades have been based—such as the Clean Air Act—have been based on science. And that sets this EPA apart from a number of other regulatory agencies. It has worked.

I want to state from the outset that I don't want my presence here to be construed as advocating for a specific environment legislation. I want to talk about science and the process by which science is conducted. It must be recognized as the most reliable pathway to knowledge and the best basis for making public policy and regulations. Science is not a political construct or a belief system. It provides testable, fundamental knowledge of the world and how things work. It's a set of principles dedicated to discovery and the use of evidence to continually test those discoveries.

And although science gets a great deal of credit for advancing our understanding of the world, I think it is less understood for its foundational quality, humility in the face of evidence, and over time, when one's cherished beliefs and partisan ideologies and wishful thinking have turned out to be wanting, the scientific evidence is most likely to remain. We need more reverence for evidence in our policymaking. Without reverence for evidence and by extension evidence-based policymaking, our country's future is compromised.

Science is not static. That's why the process of science can converge on reliable knowledge. Science does not deal in cut-and-dried facts, ever-immutable. Sometimes, we'll see the science push aside an understanding for a better, more verifiable understanding. That's the job of scientists through the scientific process, not the job of politicians second-guessing the scientific process.

However attractive any of us may find our own belief at any time, one's odds of success are better if one goes with the scientifically established thinking. Scientific progress depends on openness, transparency, the free flow of ideas and people. These are principles that have helped the United States attract and benefit

from science talent, from the Apollo program and exploring the far reaches of the universe, to advancing biomedical research for curing disease, to harnessing science to build a thriving high-tech sector. The United States has been a leader in science, in education, and in innovation. And the principles are the same principles that have allowed EPA to base their regulations on science.

Furthermore, scientists, whether industry, academia, or government, must have confidence that they can conduct their work in an atmosphere free of intimidation and undue influence. Policymakers should never dictate the conclusions of a scientific study, and they should base policy on a review of relevant research and provisions of relevant studies. In other words, the integrity of the process must be upheld.

During the Bush and Obama Administrations, federal agencies worked to develop and implement scientific integrity and access to date policies. This bipartisan recognition of strengthening the scientific integrity in federal agencies lays a good foundation that should not be weakened. In other words, I'm here to say don't try to reform the scientific process. It has served us well and will serve us well.

I thank you again for the invitation to testify, and I look forward to working with you in this Congress.

[The prepared statement of Dr. Holt follows:]

Written Testimony
Before the
Committee on Science, Space and Technology
by
Rush Holt, Ph.D.
Chief Executive Officer
American Association for the Advancement of Science
Executive Publisher, *Science*
February 7, 2017

Good Morning Chairman Smith, Ranking Member Johnson, and fellow colleagues of this esteemed committee. Thank you for the opportunity to testify before you today on behalf of the American Association for the Advancement of Science, or AAAS. AAAS is the largest general scientific society and publisher of the *Science* family of journals. Our mission is simple: to advance science, engineering, and innovation throughout the world for the benefit of all people.

I want to state from the outset that I do not want my presence here to be construed as advocating for a specific environmental regulation. Science, and the process by which science is conducted, must be recognized as the most reliable pathway to knowledge and the best basis for making public policy and regulations. To quote a recent editorial in our journal *Science*, "science is not a political construct or a belief system. It provides testable, fundamental knowledge of the world and how things work." It is a set of principles dedicated to discovery and use of evidence to continually test these discoveries. Though science gets a great deal of credit for advancing our understanding of the world, it is less understood for its foundational quality: humility in the face of evidence. Overtime, when one's cherished beliefs, partisan ideologics, and wishful thinking have turned out to be wanting, the scientific evidence is likely to remain.

We need more reverence for evidence in our policy making. Without respect for evidence, and by extension evidence-based policymaking, our country's future, and indeed all of humanity's future, becomes dangerously compromised.

Good regulations depend on scientific progress. Science is not static, that is why the process of science converges on reliable knowledge. Science does not deal with cut-and-dried facts, ever immutable. Sometimes we will see the science push aside an understanding for a better, more verifiable understanding. That is the job of scientists -- through the scientific process. However attractive you may find your own belief at any time, your odds of success are better if you can go with scientifically established thinking.

Scientific progress depends on openness, transparency, and the free flow of ideas and people. These are the principles that have helped the United States attract and richly benefit from scientific talent. From the Apollo Program and exploring the far reaches of the universe, advancing biomedical research for curing diseases, to harnessing science to build a thriving high-tech sector, the United States has been a leader in science, education, and innovation. In order to remain the world leader, the U.S. must continue to foster this free exchange of ideas and talent.

Furthermore, scientists -- whether in industry, academia, or the government -- must have confidence that they can conduct their work in an atmosphere free of intimidation or undue influence. Policymakers should never dictate the conclusions of a scientific study, and they should base policy on a review of relevant research and the provisions of relevant statutes. In other words, the integrity of the process must be upheld. During the Bush and Obama Administration federal agencies worked to develop and implement scientific integrity and access

to data policies. This bipartisan recognition of strengthening scientific integrity in federal agencies lays a crucial foundation that should not be weakened.

Moreover, regulations and agency actions should be informed by the best available science and a rigorous scientific process. Undermining the integrity of the scientific process, or the ability of federal agencies to utilize rigorous science in establishing policies, could have long-term consequences ranging from a depletion of intellectual capital, to negative health outcomes for Americans and the world. It is with this in mind that we urge caution in setting laws that would make science a combat zone. Legislation removing concepts like reproducibility and independent analysis from the hands of scientists and into a legislative chamber or a court room would truly have a chilling effect on the scientific process and reduce the benefits that science could bring to society. Seeking to influence the scientific process has no place in how a government or other entity should conduct science.

In recent decades, opinion and ideological assertions have crowded out scientifically validated evidence on some issues. If policymakers and citizens do not recognize the value that science plays in modern society, and the enormous opportunity for scientific evidence to help make better public decisions, research and innovation will not thrive.

Thank you again for the invitation to testify today. I look forward to working with you in the weeks and months ahead.

Biography of Rush D. Holt, Chief Executive Officer

Rush D. Holt, Ph.D., became the 18th chief executive officer of the American Association for the Advancement of Science (AAAS) and executive publisher of the Science family of journals in February 2015. In this role, Holt leads the world's largest multi-disciplinary scientific and engineering society.

Over his career, Dr. Holt has held positions as a teacher, scientist, administrator, and policymaker. From 1987 to 1998, Holt was assistant director of the Princeton Plasma Physics Laboratory (PPPL), a Department of Energy national lab, which is the largest research facility of Princeton University and one of the largest alternative energy research facilities in the country. At PPPL, Holt helped establish the lab's nationally renowned science education program. From 1980 to 1988, Holt served on the faculty of Swarthmore College, where he taught courses in physics and public policy. In 1982, he took leave from Swarthmore to serve as an AAAS/American Physical Society Science and Technology Policy Fellow on Capitol Hill. The Fellowships program, dating to 1973, places outstanding scientists and engineers in executive, legislative, and Congressional branch assignments for one or two years; by early 2015, the program had served nearly 3,000 alumni working worldwide in the policy, academic, industry, and nonprofit realms. Holt has said that his AAAS S&T Policy Fellowship was "life changing," and served as a springboard to his role in Congress. He also served as an arms control expert at the U.S. State Department, where he monitored the nuclear programs of countries such as Iraq, Iran, North Korea, and the former Soviet Union. In 1981, Holt was issued a patent for an improved solar-pond technology for harnessing energy from sunlight.

Before coming to AAAS, Holt served for 16 years as a member of the U.S. House of Representatives, representing New Jersey's 12th Congressional District. In Congress, Holt served as a senior member of the Committee on Natural Resources and the Committee on Education and the Workforce. On Capitol Hill, Holt established a long track record of advocacy for federal investment in research and development, science education, and innovation. He served on the National Commission on the Teaching of Mathematics and Science (known as the Glenn Commission), founded the Congressional Research and Development Caucus, and served as a co-chair of the Biomedical Research Caucus. Holt served eight years on the Permanent Select Committee on Intelligence and, from 2007 to 2010, chaired the Select Intelligence Oversight Panel, which worked to strengthen legislative oversight of the intelligence community. His legislative work earned him numerous accolades, including being named one of Scientific American magazine's "50 National Visionaries Contributing to a Brighter Technological Future" and a "Champion of Science" by the Science Coalition. He has also received awards from the American Chemical Society, the American Association of University Professors, the National Association of Graduate-Professional Students, the American Institute for Medical and Biological Engineering, the Council of Scientific Society Presidents, the American Geophysical Union, and the Biotechnology Industry Organization. Holt is also a past recipient of two of AAAS' highest honors: the William D. Carey Lectureship Award (2005) and the Philip Hauge Abelson Award (2010).

From December 2014 to February 2015, Holt was appointed a Director's Visiting Scholar at the Institute for Advanced Study in Princeton, New Jersey.

Holt is a Phi Beta Kappa graduate of Carleton College in Northfield, Minnesota, and he holds M.A. and Ph.D. degrees in physics from New York University. He is an elected fellow of AAAS, the American Physical Society, and Sigma Xi, and he holds honorary degrees from Monmouth University, Rider University, University of Toledo, and Thomas Edison State College. He is married to Margaret Lancefield, a physician, and they have three children and seven grandchildren.

Chairman SMITH. Thank you, Dr. Holt.
And, Dr. Belzer.

**TESTIMONY OF DR. RICHARD BELZER,
INDEPENDENT CONSULTANT**

Dr. BELZER. Thank you, Chairman Smith, Ranking Member Johnson, Members of the Committee. Thank you again for the opportunity to testify today. My testimony is informed by 30-plus years of experience with environment science and economics, and I'm not testifying on behalf of any past or present client. Most of my recent consulting work has involved intellectual property, and I don't think that's related at all to EPA.

In 1983, then-Administrator Bill Ruckelshaus wrote an article published in Science that "Risk assessment at EPA must be based only on scientific evidence and scientific consensus. Nothing will erode public confidence faster than suspicion that policy considerations have been allowed to influence the assessment of risk."

But EPA risk assessments are chock full of policy considerations, so it should be no surprise that public confidence in the EPA has eroded, as Ruckelshaus predicted. You need not and should not take my word for it. In 2004, the EPA's science advisor published a report on the Agency's risk-assessment practices. The science advisor defended these practices as follows, "EPA seeks to adequately protect public health and environmental health by ensuring that risk is not likely to be underestimated."

Now, under the Ruckelshaus principle, when asked to measure a mouse, EPA is not supposed to give dimensions closer to that of an elephant because elephants are riskier, which they are. The EPA's science advisor justifies mistaking mice for elephants because EPA is "a health and environmental protective agency." That's a non sequitur.

The Central Intelligence Agency is a national security agency. Would that justify exaggerating the risk that Iraq possessed weapons of mass destruction? No. But it is just as improper for EPA to exaggerate the human health risks of contaminants in drinking water, chemicals in commerce, or pollutants in the atmosphere.

When any agency exaggerates risk, it undermines responsible regulatory decision making, and it does so three ways. First, it scares the public, which cannot discriminate between large and small risks if agencies exaggerate. Second, it undermines the accurate estimation of benefits from regulation. Exaggerated risk estimates lead to exaggerated benefit estimates. Third, it usurps the authority of the head of the Agency, who is charged by Congress with making oftentimes hard choices. He can't do that with unreliable information. And if he figures out that he's being sandbagged by his own staff and decides to ignore what he's being told, he will be accused of ignoring science.

In my written testimony, I explain why EPA's safety assessments are not scientific, and I use EPA's definition of the reference dose, a copy of which is distributed to you, but here's the definition. A reference dose is an estimate with uncertainties spanning perhaps an order of magnitude of a daily oral exposure to the human population, including sensitive subgroups, that is likely to be without an appreciable risk of a deleterious effect during a lifetime.

Now, an order of magnitude we understand. That's a factor of 10. But what is perhaps an order of magnitude? Does it mean less than 10, more than 10, a lot more than 10? Could it be 10? It could be 1,000. It could be 10,000. We don't know. What is an appreciable risk of a deleterious effect? How bad must an effect be to qualify as deleterious? What is an appreciable risk of experiencing such a thing? Now, these are not scientific terms. These are policy terms? Whose personal opinions inform these choices? This is a definition only a lawyer could love and quite possibly only a lawyer could justify.

There's a great deal of interest in transparency, and I've highlighted the reference dose definition because almost every issue of transparency arises downstream of obscure definitions like this. Even if every other transparency issue were solved, the most fundamental opacities in EPA risk and safety assessments would remain.

Now, probably the most effective thing Congress can do to improve the quality of EPA science or any agency's science is to require that they adhere to the principles and procedures set forth in OMB's Information Quality Guidelines. These guidelines have been available—been out for 15 years, but there's very little to show for it because agencies do not comply, and they don't comply because no one has standing in federal court to compel them to comply.

I'm happy to look forward to any questions you might have and expand upon this at your leisure.

[The prepared statement of Dr. Belzer follows:]

Testimony before the U.S. House of Representatives

Committee on Science, Space, and Technology

Hearing:

The Environmental Protection Agency's

Process for Evaluating and Using Science During Its Regulatory Decision Making Activities

Richard B. Belzer, Ph.D.

February 7, 2017

Chairman Smith, Ranking Member Johnson, and Members of the Committee, thank you for the opportunity to testify today concerning the Environmental Protection Agency's use of science for regulatory decision-making. My testimony in informed by 30 years of experience with environmental science and economics that began in earnest during my doctoral research at Harvard University.

I. Background

After completing my dissertation, in 1988 I joined the Office of Information and Regulatory Affairs at the Office of Management and Budget as a staff economist. I served five years under the administrations of Presidents Reagan and George H.W. Bush and five years under the administration of President Clinton. My job was to review Regulatory Impact Analyses prepared by Federal agencies in support of regulations expected to have annual costs exceeding $100 million. Many of the RIAs I reviewed concerned regulations with estimated costs of many billions of dollars. I reviewed RIAs from several agencies including the Food and Drug Administration, the U.S. Departments of Agriculture, Commerce, Interior and Labor, but mostly the Environmental Protection Agency. Because of my dissertation work on the potential use of deposit-refund systems for managing hazardous waste, within EPA I focused on major rules developed by the Office of Solid Waste and Emergency Response and the Office of Water.

The principles I followed during my reviews were the same under all three administrations: provide OMB officials and White House staff the most objective estimates possible of benefits, costs and other effects. My job was strictly analytical. Both Executive Order 12291, signed by President Reagan, and Executive Order 12866, signed by President Clinton, clearly stated a preference that the net social benefits of federal regulation be maximized. But this objective is infeasible if decision-makers lack unbiased estimates of benefits and costs.

I left OMB in 1998, and after a stint as a visiting professor, in 2001 I opened a private consulting practice. My testimony today is not on behalf of any client, past or present.

II. Fundamental Characteristics of EPA Risk Assessment

A. EPA risk assessments are, by design, not objective

I learned during my doctoral research that EPA risk assessments did not objectively characterize risk. Rather, they were described as "conservative."[1] This term is misleading because it does not make clear what it is being "conserved." EPA risk assessments are neither "conservative" nor liberal," but they are intended to approximate something close to the worst case. I have reviewed some risk assessments in which risk estimates were either practically or theoretically impossible.

You need not take my word for it. In 2004, the EPA Science Advisor published a report on its risk assessment practices. At the time, EPA faced a chorus of criticism alleging that the Agency grossly exaggerated risks. EPA defended its practices by stating as follows:

> EPA risk assessments tend towards protecting public and environmental health by preferring an approach that does not underestimate risk in the face of uncertainty and variability. In other words, EPA seeks to adequately protect public and environmental health by *ensuring that risk is not likely to be underestimated.*[2]

In plain English, this means that whenever there is scientific uncertainty, EPA errs on the side of *overstating* human health risk. Further, when characterizing health risk in a population, EPA looks for individuals who faces the highest potential risk and uses those persons to describe the population.

These are not sensible practices. If we were characterizing the risk to Americans posed by peanuts, we would not say that the risk of death from anaphylactic shock from peanut ingestion is 50%, even though it is conceivable that there is someone for whom this is true. Similarly, if we were concerned about obesity in the United States, we would not say that Americans weigh 1,036 pounds – the reputed weight of the heaviest person in the United

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[1] Another descriptor EPA uses for its risk assessments is "protective," but that term also begs the question what is being protected. Precautionary efforts to protect the public from risk in one area necessarily exposes them to risk in another.

[2] U.S. Environmental Protection Agency Office of the Science Advisor (2004), p. 11 (emphasis in original).

States.[3] When thinking about the health risk posed by PM2.5, we do not assume that *everyone* is elderly, infirm, or suffers from chronic obstructive pulmonary disease.

We know not to assume the worst when we make routine decisions in almost every avenue of life. For some reason, however, we do not practice common sense in environmental health policy. And it is EPA policy not to use common sense. Quoting again from the 2004 report of the EPA Science Advisor (p. 13):

> [S]ince EPA is a health and environmental protective agency, EPA's policy is that risk assessments should not knowingly underestimate or grossly overestimate risks. This policy position prompts risk assessments to take a more "protective" stance given the underlying uncertainty with the risk estimates generated.

In plain English, this means EPA will strive for the highest estimate of risk that does not bring upon the Agency unbearable ridicule. You simply cannot rely on EPA risk assessment to give you an unvarnished perspective. When given an EPA risk assessment, all you know is risk can't be any worse.

These practices undermine responsible regulatory decision-making at least three ways.

First, they needlessly and irresponsibly scare the public about the hazards of life. Exaggerating risk is an excellent tactic for gaining the most attention from Congress, the White House, the press and the public, and for increasing one's budget and delegated legislative authority to regulate.

Second, they undermine the responsible estimation of benefits from regulation. If I'm given a worst-case risk assessment, I cannot use it to estimate public health benefits. I need, at a minimum, a *central tendency* estimate, like an average or median. Ideally I would have much more information than this, but I can use a central tendency estimate risk estimate to approximate health benefits to the population. I can't do anything useful or informative with a "conservative" or "protective" risk estimate.

Third, it usurps the authority of the EPA Administrator, who is charged by Congress with making oftentimes hard choices. When EPA staff give the Administrator an exaggerated risk estimate, the Administrator cannot make a fully informed decision. He faces extraordinary pressure to ratify the policy preferences the staff have hidden away. If the Administrator learns that EPA staff are sandbagging him and looks elsewhere for more objective information, he will be accused of "ignoring science." Indeed, EPA staff produce so-called "conservative" risk

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[3] My source for this is Wikipedia, which though often inaccurate is accurate enough for present purposes.

Richard B. Belzer, Ph.D.
Regulation, Risk, Economics & Information Quality
Strategy & Analysis Consulting
rbbelzer@post.harvard.edu ❖ 703.780.1850

assessments to tie the Administrator's hands. This enables Agency staff to make critical policy decisions secretly through the back door.

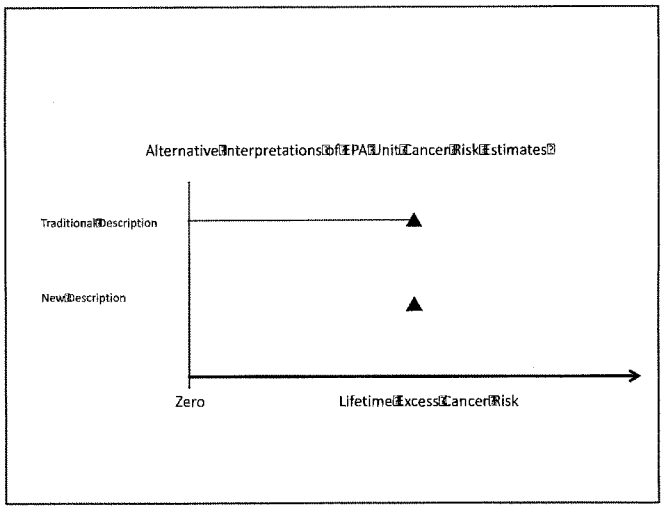
B. Nontransparency about uncertainty

Much of EPA risk assessment inevitably consists of extrapolating to humans from animals, such as rats and mice, and from very high exposure levels in a laboratory to comparatively very low exposures in the environment. These may be reasonable practices for some purposes, but often they are not reasonable at all. Rats and mice are not little people, and effects that occur when biological systems are overloaded, as they are by design in laboratory experiments, generally are not expected to occur under normal conditions.

When I began reviewing EPA cancer risk assessments in the mid-1980s, the Agency's conventional practice was to report risk estimates in a way that accounted for these key uncertainties. A common way this was done was to say, "We estimate lifetime excess cancer risk to be as high as *x*, but it could be as low as zero." And zero was understood to be the best risk estimate if, for example, extrapolating from rats or mice was biologically incorrect, or if there was a human exposure threshold below which carcinogenesis was not reasonably expected to occur. About 20 years ago, EPA abandoned the practice of qualifying its cancer risk estimates this way. Now, EPA reports them in ways that do not reveal uncertainty.

The difference between these two approaches can be seen in Figure A below. The traditional description of a cancer risk estimate told decision-makers and the public that there was substantial uncertainty, and that the true (but unknown) risk could be as low as zero. The modern description does not communicate this uncertainty.

Figure A



III. Fundamental Characteristics of EPA Safety Assessment

A lot of what the public understands to be "risk assessment" actually isn't risk assessment at all. The correct term is "safety assessment" because its purpose is to identify a "safe" level of exposure, not to estimate risk. But a safety assessment isn't science; it's a policy decision draped in scientific clothing. The reason it isn't science is science has no definition for "safety." Science is about ascertaining facts, not divining policies or making philosophical judgments.

In EPA world, the primary example of a safety assessment is the Reference Dose, often abbreviated "RfD."[4] If you are exposure below the RfD, you're said to be "safe." Except in truly extraordinary cases, you are likely to agree because the methods used to derive Reference Doses are very, very "conservative."

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4 The Reference Concentration (RfC) is an analogous tool for the inhalation pathway.

Richard B. Belzer, Ph.D.
Regulation, Risk, Economics & Information Quality
Strategy & Analysis Consulting
rbbelzer@post.harvard.edu ❖ 703.780.1850

A. EPA safety assessments are, by design, controlled by undisclosed policy judgments

Nonscientific considerations are spread throughout the RfD process. To see this, let's look at EPA's definition:

> An estimate (with uncertainty spanning **perhaps** an order of magnitude) of a daily oral exposure to the human population (including **sensitive subgroups**) that is **likely** to be without an **appreciable** risk of **deleterious** effects during a lifetime. It can be derived from a **NOAEL**, **LOAEL**, or **benchmark dose**, with **uncertainty factors** generally applied to reflect limitations of the data used. Generally used in EPA's noncancer health assessments.[5]

I have highlighted in **bold** terms within the definition that are substantially or exclusively policy, not science. It's useful for Members to understand that EPA acknowledges that a Reference Dose is uncertain by a factor of 10. But wait. EPA says Reference Doses are uncertain by *perhaps* a factor of 10. Does that mean they might be uncertain by a factor of 100? A factor of 1,000? We don't know.

What is a "sensitive subgroup"? Is a subgroup containing a single person in the United States too small? How about 100 persons? How large must it be? One percent of the U.S. population – clearly a small fraction – means 3.25 million people. How sensitive must these people be? Twice as sensitive? Ten times as sensitive?

"Likely" means a probability greater than 50%. To what does that probability apply? According to the definition, it applies to risk of "deleterious" effects? How bad must they be to qualify? They must be "appreciably" "deleterious." Only a lawyer could tell you what it means to experience "an appreciable risk of deleterious effects." There are no scientific answers to these questions; only policy judgments. When lawyers rule, science does not.

Ambiguity in the definition of the Reference Dose goes on and on and on. It's no wonder that an EPA Administrator, trying to play it straight, does not know how to interpret this information.

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[5] U.S. Environmental Protection Agency (2017). There are also Reference Dose definitions that apply to different durations of exposure (e.g., "acute," "subchronic," "chronic") and pathways (e.g., "oral").

A. Nontransparency about uncertainty

Therefore, it's for good reason that the EPA Administrator may not know how to use a Reference Dose to inform decision-making. Let's assume for simplicity that uncertainty is exactly a factor of 10. Figure B below shows many ways the RfD might be interpreted.

Row 1 shows what EPA conventionally reports to the public.[6] It's what is called a "point estimate," meaning that no uncertainty about the estimate is communicated. Row 2 shows what the EPA staff author of the RfD probably intends; uncertainty lies *above* the RfD. But because this information is poorly communicated, and EPA Administrators have limited knowledge about the derivation process and are inclined to be worrisome when public health is involved, they may think the 10-fold uncertainty contained in the definition is *below* the RfD. Rows 4-6 show other ways this 10-fold uncertainty might be understood, and none of these interpretations is necessarily incorrect.[7]

While it is sometimes possible to use an EPA *risk* assessment to estimate the benefits of a regulation, it is impossible to use an EPA *safety* assessment for that purpose. The definition of the Reference Dose tells us nothing about how much risk is reduction is obtained by any reduction in exposure. That means we can't estimate health benefits.

Finally, I want to add that nothing I have just testified to is new. On behalf of OMB, in 1990 I wrote a chapter for the *Regulatory Program of the United States Government*. Most of that chapter, titled "Current Regulatory Issues in Risk Assessment and Risk Management,"[8] remains valid 27 years later.

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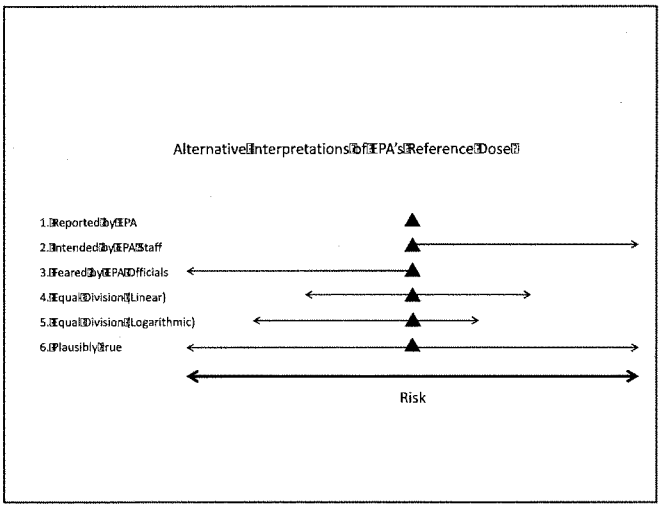
[6] U.S. Environmental Protection Agency (2016).

[7] Alternative interpretations of different players in the drama are described by Felter and Dourson (1998).

[8] Office of Management and Budget (1990).

Richard B. Belzer, Ph.D.
Regulation, Risk, Economics & Information Quality
Strategy & Analysis Consulting
rbbelzer@post.harvard.edu ❖ 703.780.1850

Figure B



IV. Implications for Benefit-Cost Analysis

EPA uses risk assessments as inputs to its benefit-cost analyses. "Conservatism" in risk assessment is therefore propagated into the Agency's estimate of regulatory benefits.[9] So, all other things being equal, EPA will not be "knowingly underestimate" benefits. But that means they *will* overestimate benefits. Whether they "grossly" overestimate benefits depends on how "conservative" the risk assessment is, whether EPA has disclosed enough detail to permit third parties to figure it out, and whether there is a venue in which errors can be corrected Sometimes, a single "conservative" assumption is enough.[10]

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[9] This was the key point in Office of Management and Budget (1990), and it is the reason why OMB guidance on benefit-cost analysis requires agencies to estimate benefits objectively. See Office of Management and Budget (2003). OMB lacks the tools to enforce this requirement.

[10] EPA's "central estimate" of the present value of benefits from regulations promulgated under the Clean Air Act from 1990 to 2020 at $12 trillion. See U.S. Environmental Protection Agency (2011). Estimated annual benefits, $1.3 trillion, are 7% of U.S. Gross Domestic Product. Almost all benefits vanish if EPA's assumed causal relationship between low

A typical Agency benefit-cost analysis includes benefit estimates derived from these unreliable inputs. You should not be surprised if benefit estimates in these analyses are highly overstated. And you should pay no attention to OMB's Reports to Congress on the benefits and costs of federal regulation.[11] OMB does not report objective benefit or cost estimates, or their own estimates based on independent review. OMB merely summarizes what the agencies said in their published benefit-cost analyses, even if the OMB staff know that these estimates are wrong. Congress faces a similar problem with respect to reports submitted to the Comptroller General pursuant to the Congressional Review Act (5 U.S.C. § 8012(a)(1). These reports are generally unreliable, and GAO lacks the expertise and time to critically review them.

V. Implications for Congress

Consistent with the policy set forth in the 2004 EPA Staff Paper, wherever you see a nonscientific, policy term in the definition of a putative scientific concept such as a risk or safety assessment, you can be confident that EPA staff have chosen to be "conservative" – that is, they have made assumptions that do not "knowingly underestimate or grossly overestimate" the factor of interest. Risk and safety assessments are constructed using multiple "conservative" assumptions. So, while we can be quite sure that actual cancer risk is likely to be less than an EPA cancer risk estimate, and that exposures to noncarcinogens below the Reference Dose poses essentially zero risk, these risk and safety assessments are unreliable for use in benefit-cost analysis.[12]

The House recently passed H.R. 26, the "Regulations from the Executive in Need of Scrutiny Act of 2017." This is not the time or place to debate the merits of this bill. However, if the bill were enacted into law, it is certain that Members will be poorly informed about the benefits and costs of major regulations intended to reduce human health risk. Benefit estimates based on "conservative" EPA risk assessments will be exaggerated and unreliable, so Members who rely on such estimates will be misled.

PM2.5 concentrations and premature mortality is relaxed. Unsurprisingly, EPA's causality assumption is controversial. See, e.g., Cox, Popken and Ricci (2013).

[11] These Reports are mandated by the Regulatory Right-to-Know Act of 2000, Pub. L. 106-554 (title VI, Sec. 624; 114 Stat. 2763A-161) The most recent draft Report to Congress was published in draft form on December 23, 2016. See Office of Management and Budget (2016).

[12] A group of 19 experts recently published a listicle identifying 10 things non-experts should look out for in benefit-cost analysis. Number 6 on the list warns against relying on risk assessments that are not transparent or objective. See Dudley, Belzer, Blomquist, Brennan, Carrigan, Cordes, Cox, Fraas, Graham, Gray, Hammitt, Krutilla, Linquiti, Lutter, Mannix, Shapiro, Smith, Viscusi and Zerbe (2017).

Richard B. Belzer, Ph.D.
Regulation, Risk, Economics & Information Quality
Strategy & Analysis Consulting
rbbelzer@post.harvard.edu ❖ 703.780.1850

Probably the most effective way Congress could improve the quality of the scientific information on which regulatory decision-making depends is to require all agency science and economics to adhere to the principles set forth in OMB's Information Quality Guidelines.[13] These Guidelines have been in place for 15 years, but there is little to show for it because agencies simply do not comply. And the main reason they do not comply is no one has standing in federal court to compel them to do so. Agency performance would improve dramatically if this loophole in the law were corrected.[14]

Thank you for the opportunity to testify today. I look forward to answering any questions you might have.

VI. References

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[13] Office of Management and Budget (2002).

[14] The Information Quality Act (2000), , 114 Stat. 2763A–153-154, says the government must "establish administrative mechanisms allowing affected persons to *seek and obtain* correction of information maintained and disseminated by the agency that does not comply" with OMB guidelines (emphasis added). Agencies have implemented the law so that the public may *seek* correction all it wants, but cannot o*btain* them.

Richard B. Belzer, Ph.D.
Regulation, Risk, Economics & Information Quality
Strategy & Analysis Consulting
rbbelzer@post.harvard.edu ❖ 703.780.1850

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Richard B. Belzer, Ph.D.
Regulation, Risk, Economics & Information Quality
Strategy & Analysis Consulting
rbbelzer@post.harvard.edu ❖ 703.780.1850

Definition of the Reference Dose

(Substantially or wholly nonscientific, policy terms in bold)

An estimate (with uncertainty spanning **perhaps** an order of magnitude) of a daily oral exposure to the human population (including **sensitive subgroups**) that is **likely** to be without an **appreciable** risk of **deleterious** effects during a lifetime. It can be derived from a **NOAEL**, **LOAEL**, or **benchmark dose**, with **uncertainty factors** generally applied to reflect limitations of the data used. Generally used in EPA's noncancer health assessments.

Source: U.S. Environmental Protection Agency. 2017. IRIS Glossary; Terms and Acronyms; 'Reference Dose'.

Richard B. Belzer

EDUCATION

- Ph.D. 1989 (Harvard University)
- M.P.P. 1982 (John F. Kennedy School of Government)
- M.S. Agricultural Economics 1980 (University of California, Davis)
- B.S. Agricultural Economics 1979 (University of California, Davis)
- Hometown: Torrance, California

EMPLOYMENT

- President, Regulatory Checkbook (2001-present)
  - Regulatory oversight
  - Information quality
  - Organization of scientific peer review
- Managing Editor, NeutralSource.Org (2006-present)
- Independent Consulting in Regulatory Policy, Economics and Risk Analysis (2001-present)
- Visiting Professor of Public Policy, Washington University in St. Louis (1998-2001)
- Economist, Office of Information and Regulatory Affairs, Office of Management and Budget (1988-1998)
  - Division Performance Awards (1991, 1992)
  - Special Achievement Award (1990)

ACHIEVEMENTS RELEVANT TO TODAY'S HEARING

- Prepared the final version of OMB first guidance on how to prepare Regulatory Impact Analyses (1990)
- Authored OMB white paper on risk assessment and risk management (1990)
- Contributed significantly to the text of Executive Order 12,866 (1993)
- Contributed significantly to OMB's Principles for Risk Analysis (established by the Clinton Administration in 1995 and ratified by the George W. Bush Administration in 2007)
- Fellow, Cecil and Ida Green Center for the Study of Science and Society (1995)

PROFESSIONAL AFFILATIONS

- Society for Risk Analysis
  - Elected Treasurer (1998, 2000)
  - Outstanding Service Award (2003)
- Society for Benefit Cost Analysis
  - Elected Secretary/Treasurer (2008, 2010, 2012)
  - Elected Treasurer (2014)
- American Association of Wine Economists

Chairman SMITH. Thank you, Dr. Belzer.

Before we get to questions, I'd like to ask unanimous consent that the gentleman from California, Jerry McNerney, be allowed to participate in today's hearing. He has been selected to serve on the Science Committee, and his official appointment is imminent. So, Jerry, we welcome you back.

Mr. MCNERNEY. Well, I thank the Chairman. I've spent two terms on this Committee, so it's a great thing to be back, and I look forward to our work together.

Chairman SMITH. Absolutely. I thank the gentleman from California for his comments.

Let me recognize myself for questions. And, Dr. White, I'll address my first question to you, and that is why would the EPA hide data that they say justifies regulations from the American people, and why should that data be made public? Turn on your——

Dr. WHITE. There we go.

Chairman SMITH. Yes.

Dr. WHITE. Transparency in the way that EPA evaluates its science and what data it selects to underlie its decision-making is extremely important. So that information should be made available so that folks can actually go through and evaluate those scientific evaluations.

One of the things that we do need to take into consideration as making that data publicly available is that there are adequate protections for confidential business information to ensure that we keep innovation and competitiveness available for the marketplace.

Chairman SMITH. Okay. Thank you, Dr. White.

And, Mr. Holmstead, does the EPA use a biased modeling system to calculate and determine the benefits from its proposed regulations? And if so, why can that not be justified?

Mr. HOLMSTEAD. Well, thank you for the question. I think at least for the last few years EPA has used methodologies that clearly overstate the benefits of their regulations. And I'm happy to submit something that would provide you a bit more detail, but let me just summarize very quickly. Virtually any air regulation that's been done over the last eight years is based on the benefits of a single pollutant known as PM2.5 or fine particles. And people are surprised when they understand this. EPA issues a regulation for mercury, EPA issues a standard for ozone, EPA issues a standard for diesel emissions, and yet, when you look at the underlying evaluation, EPA's claim is all of these things are justified because in some fashion EPA predicts that that will reduce levels of fine particles in the environment.

There is a whole other program for regulating these chemicals. There's a way of evaluating them, and yet the benefits that EPA claims is consistent—is—I'm sorry, is completely inconsistent with the way they do it in this other program. So, again, this is an issue that probably deserves more than a short answer, and I'd be happy to provide that to you. But there's no question that EPA has started to use cost-benefit analysis as a way to promote its regulations rather than a tool that can be used to inform good regulatory decisions.

Chairman SMITH. Okay. Thank you for that response. And yes, we'll take the details and we'll make them a part of the record as well.

Dr. Belzer, let me ask you about cost-benefit analysis and why should there be a cost-benefit analysis for all EPA regulations?

Dr. BELZER. Mr. Chairman, it comes to benefit-cost I'll say—that would be the appropriate term, by the way—benefit-cost I'll say—I finished eight years as Treasurer—Secretary Treasurer at the Society for Benefit-Cost Analysis, and so I am an avid defender and advocate of practice. And the reason is fairly simple, that it is impossible for decision-makers, whether in an agency or on Capitol Hill, to understand what the implications are of actions that are being taken without benefit-cost analysis. It can't be done without that. Otherwise, it's based on emotion, it's based on politics, based on cronyism. It's based on other factors.

But—and also I would say it can't be based on science because science is the underpinning to the benefits assessment in a benefit-cost analysis. So agencies should be doing that all the time, and it's also cost-effective. It costs relatively little money to do a benefit-cost analysis, and it can save an enormous amount of regulatory costs and it can also dramatically improve the benefits of a regulation.

I'm particularly fond of one that I managed when I was at OMB, which the Department of Agriculture was trying to ban a product, and they were going about it all—in a way that made no sense scientifically and economically. With help, I was able to change the justification for it, and they were successfully able to ban this product because people were dying from it. Without the analysis, that can't be done. That would have been litigated, and the Department of Agriculture would have lost.

Chairman SMITH. Okay. Thank you, Dr. Belzer.

And, Dr. Holt, my last question for you. The editor-in-chief of your publication Science recently said "Dr. Bates raises some serious concerns. After the results of any appropriate investigations, we will consider our options," and those options "could include retracting that paper."

Dr. Bates is a well-respected scientist who was given one of NOAA's highest awards for developing standards for preserving climate data records. Are AAAS and Science committed to taking his allegations seriously and launching a thorough investigation?

Dr. HOLT. I think you're referring to the Karl paper that appeared in Science a couple of years ago, and the blog that appeared over the weekend—this past weekend by one former scientist Mr. Bates—Dr. Bates from NOAA. Dr. Bates said in an article published today the issue here is not an issue of tampering with data. He does not—then further it is written he does not believe that they manipulated the data. All he is doing "is calling out a former colleague for not properly following agency standards." This is not the making of a big scandal. This is an internal dispute between two factions within an agency.

Chairman SMITH. Right. I—Dr. Holt, I've——

Dr. HOLT. There's nothing in the paper, the Karl paper, that, at our current analysis, suggests retraction.

Chairman SMITH. Well, I am——

Dr. HOLT. We are always looking at our papers to see whether there is anything——

Chairman SMITH. Dr. Holt, my time——

Dr. HOLT. —truly erroneous.

Chairman SMITH. My time is up. I encourage you to talk to Dr. Bates because everything that I have read that he has said about the Karl report suggests to me that NOAA cheated and got caught. They did falsify the data to exaggerate global warming. The Karl study cannot be replicated because, supposedly, the computer crashed maybe like the computer at the IRS and the EPA. And clearly, he suggests that the Karl study violated scientific integrity rules. To me, all that adds up to an investigation and possibly a retraction. I just simply ask you to look at it. It may even be a lot more serious than you think.

Thank you, Dr. Holt. That concludes my questions. And the Ranking Member, the gentlewoman from Texas, is recognized for hers.

Ms. JOHNSON. Thank you very much, Mr. Chairman. And at the end of my statement, I'd like to submit to the record a statement related to this very discussion with Dr. Bates.

Chairman SMITH. Okay. Without objection.

Ms. JOHNSON. We hear a lot, Dr. Holt, of artfully crafted positions from nonscientists invoking science and the scientific process to support positions. Unfortunately, this is the kind of tactic that can generate confusion and doubt about the actual state of the scientific consensus. As a scientist and a former Member of Congress, how should science inform policymaking? And how can we as policymakers be sure that an agency like EPA is making agency actions based on the best available science? And how should we view disagreements between scientists over a particular study? And is it dangerous to assume that any such disagreement undermines the overall findings of such a study?

Dr. HOLT. Thank you, Representative Johnson.

The approach is to ask whether the procedure, the process of science has been followed, not to second-guess the results or let one's dislike for an outcome lead one to challenge the outcome for unscientific reasons or to challenge the process because sometimes the results are either unclear or unpalatable.

The process, as I was saying earlier, for developing regulations at EPA, is based on legislation that holds the science to be paramount. And it is I think the job of oversight to make sure that the processes of science are working, not to try to reform them or substitute other processes.

So, for example, whether health studies that are used for regulations are based on something other than peer-reviewed scientific research, not whether different standards of releasing personal information should be used. It is whether the standards of the field are being observed. So, for example, the Harvard health study that was used for regulations on air contaminants had some data, personal information about deaths, about families that would not be made available on the internet openly. That's not to say that there's any conspiracy here of hiding data.

Ms. JOHNSON. Thank you very much.

Also, as a scientist and academic, you've had the opportunity to help shape the educations and careers of many young scientists and engineers who were interested in pursuing careers in the STEM fields. What type of impact would undermining the integrity of the scientific process by casting doubt on the accuracy or type of data that the EPA collects have on future scientists in the environment and health professional fields? And what impacts would it have on scientists and engineers broadly?

Dr. HOLT. Well, I've certainly talked with scientists who find it uncomfortable and unattractive to work in fields where they feel they are constantly second-guessed by politicians and not in a sense allowed to operate freely in their work. That has a—sometimes a chilling effect, but at least it generates a level of discomfort that I think probably drives some scientists to go into areas that are maybe less relevant to public policy. And that of course is a real loss. We need the best science applied to things that effect people's health and livelihood.

Ms. JOHNSON. Thank you very much. My time is——

Chairman SMITH. Thank you, Ms. Johnson.

The gentleman from Oklahoma, Mr. Lucas, is recognized for his questions.

Mr. LUCAS. Thank you, Mr. Chairman.

Mr. Holmstead and Dr. White, last Congress the House passed one of my bills, the Science Advisory Board Reform Act. I'd like to visit with both of you about that body and where it might be improved. I'm concerned that the body has become perhaps an echo chamber for the EPA. Would you both agree with that concern? And how else would you suggest that the EPA better utilize the Science Advisory Board?

Dr. WHITE. Well, maybe I'll start.

Mr. LUCAS. Please.

Dr. WHITE. So, yes, the Science Advisory Board is one mechanism that the EPA has to conduct its peer-review process. As I mentioned in my comments, the peer review needs to be independent and objective. That means the making sure that the peer-review process has appropriate expertise, also the depth in the committee to evaluate the assessments at hand.

There also needs to be clear balance and—in the reviews and the peer reviews of the people participating in that process. So you need to make sure that there's—if there's conflict of interest on the panel or if folks have actually made specific recommendations about a particular chemistry that they're evaluating on that panel, that that also is balanced in the review.

What we've often seen most recently in the peer reviews conducted by the SAB is that EPA plays a prominent role in that peer-review process. So oftentimes, the conversations that are happening in that peer review get stymied by EPA's input during the peer-review process so it's not as independent as it should be.

Additionally, currently in the peer-review process conducted under the SAB, there's not really an adequate opportunity for the peer-review panel to address or respond to peer review and public comments. And so there will be a large set of public comments that'll be submitted into the process for the peer review, and that information is often discounted or ignored. And there's really no vo-

lition by the peer-review panel or indication by the EPA that they need—that the peer review needs to address those public comments in any way.

And finally, when the peer review actually generates their report and makes recommendations into the EPA, there's not a volition that the Agency has to abide by all of the recommendations that have been identified by that peer review. So there seems to lack a check-and-balance of—the peer review makes strong recommendations for improving the scientific quality of a particular assessment, but that information doesn't really get incorporated wholeheartedly into the final assessment.

Mr. LUCAS. Mr. Holmstead.

Mr. HOLMSTEAD. I'll give just a quick answer. I do think it's problematic that it's the EPA Administrator, based on EPA staff recommendations, that make all these appointments. Because that's the way the process works, the folks appointed often have views that are consistent with the views of the EPA staff. And there's nothing wrong with that, but there does need to be additional views involved when there are other serious scientific points of view that are not represented on the panel. So I think there does need to be a way to ensure that there's a little more balance and that the science isn't so one-sided.

I also want to just quickly make a—raise a particular concern. One of the subsidiary bodies underneath the SAB is known as the Clean Air Science Advisory Committee, the CASAC. By statute, they are supposed to look at certain issues and advise the Administrator on those issues, including the CASAC is supposed to advise the Administrator on the adverse economic and energy effects of certain regulations. CASAC has refused to do that for many years. Finally, the CASAC Chairman said I understand we have that obligation; the problem is we don't have anybody on our committee who has expertise in that, and so I look to the Administrator to appoint someone. And a few days later the Administrator said she had no interest in appointing anybody to do that issue even though that's supposed to be done by the statute.

And so there needs to be a way to ensure that appointments are actually appropriate to look at the issues that Congress and others asked these committees to look at.

Mr. LUCAS. Clearly, Mr. Holmstead and Dr. White, the general public that depends on us to analyze the results of these studies, that depends on the EPA to formulate policies based on the input from these various groups, it doesn't necessarily matter so much to my constituency who is in charge, and that changes from Administration to Administration. But the quality of that input needs to be something that folks can be reassured about back home.

So clearly, you both described a circumstance where work needs to be done, and I think the Committee will work legislatively on that.

And with that, Mr. Chairman, I'll yield back the balance of my time.

Chairman SMITH. Thank you, Mr. Lucas.

Without objection, I'd like to make a part of the record the entire comments by Dr. John Bates that were posted on February 4. The headline is "Climate Scientist Versus Climate Data," as well as an

article by Timothy D. Clark called "Science, Lies, and Video-Taped Experiments."

[The information appears in Appendix II]

Chairman SMITH. And the gentlewoman from Texas is recognized.

Ms. JOHNSON. I'd like to also place in the record Dr. Bates today said that there was no manipulation. I would like to enter this article into the record, which contains his quote.

Chairman SMITH. Without objection.

[The information appears in Appendix II]

Chairman SMITH. And the gentleman from Illinois, Mr. Lipinski, is recognized.

Mr. LIPINSKI. Thank you, Mr. Chairman.

Dr. Holt, I want to thank you for your work here in Congress previously and your work at AAAS.

As a scientist, although some people would—I'm a social scientist so some people question that, but as a scientist, I understand the important role of science. And I think we all agree that science should be what guides us in making decisions where that is appropriate. Politics should not be what is trumping science.

I want to give you an opportunity—I don't know if there was anything else that you wanted to add about the controversy with Dr. Bates because I think you—the Chairman had run out of time. If there was anything you wanted to add to that or——

Dr. HOLT. Thank you. Of course, I don't know the internal disagreement, and it quite clearly is largely an internal disagreement. Dr. Bates was not part of one research group that produced the Karl—the data that became the basis for the Karl, et al., paper. And he does not like the way the other group conducted their research.

What's most important is not that there was an internal disagreement within NOAA about how to handle this but that a number of other studies, including one most recently in one of AAAS's other publications Science Advances, have replicated the work, have come up with the same conclusions so that, you know, it's—it is not profitable, considering the process of science, to dwell on this one internal dispute about how to handle the data. If the Inspector General at NOAA wants to look at that, that's fine.

If there are indeed erroneous or deceptive procedures followed in answer—in further answer to Chairman Smith, I know our journals will consider retraction, but there just doesn't seem to be anything like that now. There's nothing that suggests there are problems with that work. And most importantly, even Mr. Bates says this does not change the policy-relevant conclusions about climate change. So I think that's most important to get out there.

Mr. LIPINSKI. Thank you. And while the—Dr. Holt, while the EPA is generally thought of as a regulatory agency—we're talking about research here—we know that it's research and science that's led to a number of important findings such as the link between air pollution and cardiovascular health and also has led to research at EPA that's led to innovations such as the development of low-cost sensors for nutrient pollution in our nation's water. So it's important that we understand that part of the Agency's mission is not

just to regulate but also do research as another way to help protect health.

Are we not sacrificing potentially some of this important work by meddling in EPA's practices or cutting its budget or, as some in Congress have—would like to do is completely cutting out the EPA? What would we be losing in research—in research, you know, even take it away right now from research on climate change but other research and the developments that have come out of that—out of the EPA?

Dr. HOLT. I think it is very important to have a scientific basis for the regulation for EPA to conduct research intramurally, to sponsor research outside of EPA because—take the National Science Foundation for example. They do not—you know, they do basic research that ultimately may affect toxicology studies and so forth, but they don't do toxicology studies. They don't do air pollution epidemiology studies. Those are things that EPA can best do both with internal scientists in an intramural program and through external sponsorship. And that would include, I would argue, social sciences. We at the AAAS believe that empirically based answers to questions that can be verified are science. That includes social science, as well as the earth sciences and physical sciences.

Mr. LIPINSKI. Thank you. I yield back.

Chairman SMITH. Thank you, Mr. Lipinski.

And the gentleman from California, Mr. Rohrabacher, is recognized.

Mr. ROHRABACHER. Thank you very much, Mr. Chairman. And I'd like to, first of all, thank you, Mr. Chairman, for having the political courage to actually look into something that obviously is going haywire in our country. I mean, we—there's obviously something wrong in the arena, the scientific arena in our country. We have people telling us global warming is causing the drought in California. Now, it's global warming is causing the flooding in California. And whatever malady we have we end up having people telling us that the amount of CO_2 that's being put into the atmosphere is the ultimate culprit.

And then we have all of these contradicting results. And that's what this is all about today is whether or not there really was a pause in the actual increase in the temperature of the planet since I think 1998 I guess it was. And the fact is, since 1998, we have had large increases in CO_2, so if there had been a leveling off or it actually declined in temperature in some areas, that would mean the CO_2 theory is wrong. Okay. That's a basic scientific area that needs examination and truth associated with it.

Because there's been so many things that have been told to us that seem contradictory, we have every reason to be skeptical that our scientific community is maintaining its integrity. Why wouldn't it? Why would there be some sort of loss of integrity among America's scientific areas? Eisenhower warned us of that years ago. Everybody always remembers Eisenhower warning us, and this was exactly correct, about the military industrial complex. And I will tell you that I think his words of warning there are something we should look at today because today, it even threatens the well-being of our country.

But he also warned us about how government contracts become virtually a substitute for intellectual curiosity and warned us "The prospect of domination of the nation's scholars by federal employment, project allocations, and the power of money is ever-present and is gravely regarded." And just as he warned us against the military industrial complex, he warned us that our scientific—the integrity of our scientific endeavors could be undermined by this very same type of government interaction with our science community.

And I think that you, Mr. Chairman, have been courageous in taking this on because there are people and very powerful forces at work in this world today that have their own agenda and are trying to justify it based on manipulating science, basically manipulating the people who did or did not get the scientific contracts to do specific research projects over the last 20 years. And I would hope that we'd actually call Mr. Bates in to testify. I hope that this is the beginning hearing to try to determine truth.

What counts as truth? What counts is whether or not we have people whose scientific findings have actually been influenced by whether or not they get a government contract for research. And the story—there are stories of this that a number of scientists have come to me telling me where before a certain time period they were receiving government research contracts. Afterwards, after it became clear they didn't agree with the CO_2 theory, no more contracts.

Well, this is all about the integrity of science in the United States of America. Dr. Holt, would you like to refute everything I just said?

Dr. HOLT. In nine seconds, Mr. Rohrabacher?

Mr. ROHRABACHER. That's it.

Dr. HOLT. Okay.

Chairman SMITH. The gentleman's——

Dr. HOLT. There has been no pause. It has been examined literally from many perspectives, and scientists are fiercely independent. They would resent horribly if they felt their work was being manipulated. It's not.

Chairman SMITH. The gentleman's time has expired.

The gentlewoman from—I was looking for the gentleman from California, Mr. Bera, who's not here. The gentlewoman from Connecticut, Ms. Esty, is recognized for her questions.

Ms. ESTY. Thank you, Mr. Chairman. I want to thank our witnesses and our former colleague, Dr. Holt, for being with us here today.

I want to follow up a little bit, Dr. Holt, with the scientific process. I think there—I hope and believe there is agreement on this Committee that sound science is absolutely essential to good decision-making, whether it's by the EPA, any other administrative agency, or Members of Congress.

So I'd like to ask you a little bit—so there's been a lot of criticism from opponents of EPA regulations that sometimes EPA gives more weight to some studies than to others. Now, some see that and have characterized that as showing bias or favoritism. Can you explain a little bit more within the scientific process whether that's

appropriate and why that might be appropriate to give more weight to some studies than others?

Dr. HOLT. Thank you, Representative Esty.

Some studies are extensive, longitudinal, long-range, comprehensive studies. Others are more superficial. Which one do you think would be—which ones do you think would be more useful for making decisions about your children's health? It is true that sometimes short, brief studies uncover things and then they should be explored.

My—in my opening remarks I was saying if you want to be on the right side of any issue, you would do well to go with the evidence, the best understanding at the time of what it's going to be, not some fringe idea. Scientists are always poking around the periphery trying to find new understanding, but we shouldn't think that that is the center of gravity.

You know, take the climate change issue we're talking about this morning. This is an internal dispute about a detail of how you might measure land temperatures or water temperatures. It is not a departure from the general understanding of what's happening to temperatures in our globe. And so some studies are indeed more worthy of trust and more—a better basis for sound regulation than others.

Ms. ESTY. I'm glad you raised some of that because I am—live in the State of Connecticut, and we have very high rates of asthma. We are downwind from many of the power plants that have caused real problems. And we see it in higher cardiopulmonary issues; we see it in asthma in children. There's a great deal of concern in my State that some of the longitudinal studies will not be allowed anymore under some of the proposals we're looking at in secret science, the—we have—the American Thoracic Society has provided some of that information.

Can you explain and flesh out a little bit more—you referenced the longitudinal studies, and I know in my State there's a great deal concern because we've seen real health benefits from some of those changes that EPA has promulgated based on these kinds of studies. Can you flesh that out a little bit what that would mean for those studies?

Dr. HOLT. Thank you. You know, the—I guess you're referring more to the Secret Science Acts of previous Congresses. Now, I don't know what the Chairman or others will be proposing in this Congress, but in the past, the legislation, in the name of providing openness, has meant that certain kinds of studies could not be considered because some of the data in those studies—and I mentioned this Harvard study on atmospheric pollution—well, actually on community health—could not be used.

It would also, for example, hinder fast response. For example, some of the data from the Freedom Industries spill on the Elk River in West Virginia recently—I mean, a couple of years ago—some of that data could not or was not publicly disclosable, and that hindered EPA in their response. Chairman Waxman at the time actually contacted the manufacturer of the chemical to find out what it was and—but the secret science research—Secret Science Act probably would have prevented that fast action.

Chemically induced birth defects, no family wants the newspapers or the web pages to be listing information about their kids' birth defects. But if you're going to study birth defects you've got a look at actual kids and study the epidemiology. The Secret Science Reform Act presumably would have prevented that kind of necessary research.

Ms. ESTY. Thank you very much, and I see my time is expired.

Chairman SMITH. If the gentlewoman would yield real quickly, I think Dr. Holt knows as well as anybody that there's such a thing as redactions, and if it's personal information, that information can be redacted.

I will go now to the gentleman from Florida, Mr. Posey, for his questions.

Mr. POSEY. Thank you, Mr. Chairman.

Dr. White and all of you, I think we're all concerned when agencies promulgate rules that they say are based on sound data, and when they're asked to share the data, they say no, we're not going to tell you what the data is, you just have to accept our word for it.

So, Dr. White, your written testimony you highlighted the peer-review process as a critical tool for insuring government policy is informed by sound science. I think we can all agree on the importance of objective peer reviews, which is why I'm alarmed by some of the reported problems with the current peer-review process at EPA and other agencies. Can you briefly explain why a transparent peer-review process is so important to ensuring the quality of scientific information?

Dr. WHITE. Sure. Thank you very much, Congressman, for that question.

So as I mentioned, peer review is a critical piece of the science evaluation process. Having a third-party group of scientific experts evaluate and look at EPA's assessments is very important to establishing trust from the public. And making sure that you have the appropriate expertise, one, on that peer-review panel, that the EPA actually heeds the advice that they get from those peer reviewers and incorporates that information into their final assessment is critical. I think what we've seen is that that has not always been the case for the peer-review process and we'd like to see that change.

Mr. POSEY. Okay. Follow-up, given what you've just told us, should agencies strive to allow the public to incorporate information from peer reviews into their comments on a proposed rule?

Dr. WHITE. So the Agency definitely should take into consideration the public comments that it receives. One part of the peer-review process is getting in public comments. Those comments come from scientific experts that have evaluated the available data and provided that information into the Agency. Unfortunately, in some of the programs, while they get the information that has been provided by the public, they don't incorporate or respond to that information.

Specifically, in EPA's Integrated Risk Information System one of their processes in step five is actually a disposition of the public comments so actually writing down how they will respond or address peer review and public comments. Unfortunately, in the last

three assessments that they put out they have not responded to those public comments, and so again this is kind of a violation of their own processes that they have currently in place.

Mr. POSEY. Thank you very much, Dr. White.

Dr. Holmstead, continue on the same point. Can you discuss how both the scientific and rulemaking process is injured when interested and qualified parties are unable to comment meaningfully on science supporting a proposed rule?

Mr. HOLMSTEAD. Well, thank you for your question and for calling me doctor. Unfortunately, I'm the only one on the panel here who is only a lawyer and—

Mr. POSEY. We don't need to waste time on that. Go ahead.

Mr. HOLMSTEAD. In both policy questions and science questions I've certainly seen that thoughtful discussions from different points of view leads to better outcomes. And I just think it's—I think both on the regulatory side and the science side that EPA and the public would be better served if we actually had people who were well-qualified, who have different perspectives and different points of view, if they can try to resolve some of these issues instead of stacking some of these groups with people who share EPA's view.

Mr. POSEY. Okay. Well, thank you, Mr. Holmstead.

Before my time runs out I'd just also like to thank the Chairman for the opportunity to explore this issue. I know a lot of us have been concerned about this, and a lot of our citizens are threatened by a torrent of bureaucratic attacks that they don't understand that affects their lives and their livelihoods every day. And, Mr. Chairman, I just again want to applaud you for taking this issue on.

Chairman SMITH. Thank you, Mr. Posey. Since you have a little time left, let me suggest to Mr. Holmstead that he seize on the juris doctor and start something new and go with the doctor.

The gentleman from Virginia, Mr. Beyer, is recognized for questions.

Mr. BEYER. Thank you, Mr. Chairman.

You know, a lot of attention has been paid to the title of today's hearing "Making the EPA Great Again." Under President Obama, the EPA took aggressive action to tackle climate change, which most scientists agree is the major threat to the planet and to the human race. It will not help anyone by disputing climate science with stories from white nationalist websites like Breitbart.com or tabloids like the Daily Mail.

Scott Pruitt, the President's nominee to head the EPA, has sued the Agency 14 times over its effort to regulate the oil and gas industry and has not said whether he would recuse himself from ongoing cases against the EPA. This is not a recipe for greatness.

The new Administration has scrubbed the EPA's website, has frozen its grants and contracts, has placed what amounts to a gag order on EPA employees, and requested names of employees who worked on climate change. A Trump transition official suggested cutting the Agency's workforce by 2/3 in a bill introduced by a fellow Republican will eliminate the EPA altogether. Mr. Chairman, this is not greatness.

Alarming environmental disasters like revelations on the consequences of pesticides and cities covered in smog and rivers catch-

ing on fire spontaneously drove a Republican President, President Richard Nixon, to create the Environmental Protection Agency.

And since then, the EPA's legacy has been one of great achievement. Among its most important accomplishments, the EPA banned the pesticide DDT. It significantly reduced levels of sulfur dioxide and nitrogen dioxide, the leading ingredients in acid rain. It mandated lead-free gasoline. It regulated toxic chemicals and established a national commitment to protecting our air and our water.

The EPA's achieved so much in recent years, often over the misguided objections of this Committee and a Congress which has tried to make things worse. Mr. Chairman, Members of the fellow Science Committee, please, please listen to the Science Committee. Climate change is real and environmental problems can't be wished away or pretended away because they're going to affect us, our children, and future generations.

This Committee should be leading the charge to protect the planet and our environment for future generations and instead, it attacks the credibility of scientists, casts doubt on accepted science, and makes life difficult for the people trying to solve urgent crises.

The Science Committee's contribution now is like that of the Emperor Nero fiddling while Rome burned down around him. This is irresponsible and dangerous. It is not leadership and will not make the EPA or America great. In fact, I think we should retitle this hearing, that we keep the EPA great or maybe just save the EPA or protect our water.

So with that, Dr. Holt, let me actually pose a question. Dr. Belzer said, and I quote from his written testimony, "The EPA will strive for the highest estimate of risk that does not bring upon the Agency unbearable ridicule." How would you respond to Dr. Belzer's comment that they always take the most extreme version of risk in their calculations?

Dr. HOLT. You know, I'm not a historical scholar of EPA, but I can say that in the history that you touched on, because these—this legislation from past decades was based on science, it has managed to keep up with growing knowledge and improved understanding of human health and the effects on human health and on the environment of various kinds of water and atmospheric contaminants. And the record is very good.

So, you know, I—I'm not a—you know, Dr. Belzer is—he is a scholar of benefit-cost analysis. He wants it phrased that way I think. I would not say that I am. But I do look at the results, and the results have been good.

Mr. BEYER. Okay. Thank you.

Juris Dr. Holmstead, in your testimony you wrote that referring to the Science Advisory Board Act that we took up in the last Congress, that by focusing on disclosure rather than disqualification that we would ensure that any potential conflicts, financial and otherwise, are publicly disclosed. But one of the objections we had to the act last time was that it in fact didn't do full financial disclosure. Are you aware that what will be intended this year will actually prompt full financial disclosure for people that would be appointed to that board?

Mr. HOLMSTEAD. You know, I don't know anything about the bill that may be introduced this year, but I do believe that full disclosure, not only of financial conflicts but other financial interests, is an important part of the process. So again, I'm not familiar with legislation that may be forthcoming from this Committee, but I do think that's an important thing.

Mr. BEYER. I hope you don't mind us using your testimony when the bill comes before us later so—thank you. Mr. Chair, I yield back.

Chairman SMITH. Thank you, Mr. Beyer. By the way, my cap is going to read "Keep the EPA Honest." But I appreciate the gentleman.

The gentleman from Texas, Mr. Weber, is recognized for his questions.

Mr. WEBER. Thank you, Mr. Chairman.

To my friend there from Virginia I want to say I'm not going to quote any of those websites that you did. I'll choose an article by The Hill, not necessarily known as a right-wing newspaper, "Drowning by EPA Overreach," June the 17th of '15 by Will Coggin. "The EPA recently found itself in hot water. The New York Times revealed the Agency colluded with environmentalist groups in a campaign to manufacture public comments in favor of a new rule that expands its own power. The Agency's actions and the shenanigans of its environmentalist supporters shed light on how a bad rule can flow through a regulatory process," not exactly your right-wing publication.

But it does point to the fact of why there's questions and why, with legitimate oversight, notwithstanding my friend's comments about this Committee was wasting its time, why we have a legitimate oversight role to play and we should be and that doesn't make us a useless Committee.

And I applaud the Chairman on his efforts to make sure that we hold the EPA accountable.

Quick, Mr. Holmstead, how did he term you? My dearest Holmstead? Was that what he said?

Mr. HOLMSTEAD. Most—many people refer to me that way.

Mr. WEBER. I've got that, and I'm glad to hear you all have an intimacy there that few do.

The EPA will often bury—well, let me do this. Adam Peshek from another publication said sometime back in 2011—actually, he said in an article two examples of EPA overreach that "Measures taken to protect the environment are necessary and welcomed, but concerns for air quality should always be measured against the larger context of the economy and real-world achievability." So I think the fact that we're here having this discussion gives us pause for concern that we ought to be able to have these discussions and question the science.

The EPA will often bury the cost of its regulations while inflating benefits. Measure what Adam Peshek said against real-world achievability. Do you think, Mr. Holmstead, it would improve regulations if EPA were more transparent in its cost-benefit analysis?

Mr. HOLMSTEAD. I do believe that transparent cost-benefit analysis is important. I also believe it's important that the Agency fairly present the results because in some cases, if you dig into the de-

tails, you'll find that EPA has been relatively transparent, but the way they present their conclusions gives you a very different picture than——

Mr. WEBER. You can understand why, when articles come out from The New York Times that they've colluded with environmentalist groups to further their own power-making authority, their own rulemaking authority, that the Chairman of the Science Committee might have cause for concern and say we might ought to have some oversight of that. You'd understand that?

Mr. HOLMSTEAD. No, absolutely, and I do think that was inappropriate. I was surprised to hear about that comment.

Mr. WEBER. Thank you. Let me move on to the next. Over the past eight years, based on what we just talked about, EPA has skewed its regulatory cost-benefit analysis to accomplish policy goals. Do you think this undermines public confidence in their analysis and could you understand why?

Mr. HOLMSTEAD. I think that the short answer is absolutely. It would be better for everyone, for the public, for all of us in this room if there could be more confidence in some of the conclusions that we get from EPA and other regulatory agencies, and that's why I think some of the reforms that you're talking about are very important.

Mr. WEBER. Thank you. I appreciate that. And I'm going to go on to Dr. White.

Dr. White, I'm concerned that the EPA has stacked its Science Advisory Board and Clean Air Science Advisory Committee with supporters, those that we talked about. The New York Times article said they colluded with environmentalists, for example. The EPA rolls out a regulation that says that it's supported by its scientists but no one is there to offer an opposing view, no one. So do you think there should be more balance on these advisory committees in your opinion, Dr. White?

Dr. WHITE. Absolutely. Balance is one of the keys that's important for having a peer review. You want to make sure that you have enough folks on the peer-review committee and that there's appropriate balance so that you have discussion about what EPA has done. If you only have one side of the story being told at the meeting or you only have one set of views on the peer-review panel, then you're not getting a full picture and you're not really having a robust peer review.

Mr. WEBER. So to you as a scientist, what does that say to you about those scientists that they really believe in an open and fair process or that they're pretty much consigned to just those who support their already predetermined analyses, for example?

Dr. WHITE. I think what it shows is that there's just not appropriate balance on the committee——

Mr. WEBER. Yes.

Dr. WHITE. —and so you really have to make sure that if you build a committee that only has one set of views, then you're likely——

Mr. WEBER. Right.

Dr. WHITE. —going to get a certain answer——

Mr. WEBER. You need a devil's advocate.

Dr. WHITE. Right.

Mr. WEBER. Yes. Thank you. I yield back, Mr. Chair.

Chairman SMITH. Thank you, Mr. Weber. The gentlewoman from Nevada, Ms. Rosen, is recognized for her questions.

Ms. ROSEN. Thank you, Mr. Chairman. And I want to thank everybody here today for their thoughtful testimony on what is clearly a very controversial, and all slogans aside, we need to listen to the scientists who create a hypothesis, go through scientific method, come up with a conclusion. No matter who we have on the panel, there really is a process there and that's what we need to pay attention to.

So my question is for Dr. Holt. Although we would never want to replicate or should we replicate natural disasters or manmade disasters like the Gulf oil spill, an earthquake, what have you, I want you to talk a little bit about how excluding one-time events, things that can't be repeated nor should be, will impact the EPA and we won't get the maximum scientific return if we don't study them.

Dr. HOLT. Thank you, Representative Rosen, and I'm pleased you've chosen to serve on this Committee.

It's not just one-time events in the cases of emergencies, for example, disasters. Many studies cannot be repeated in exactly the same way. The populations have changed. Those people have grown up or moved away or the forest that you're studying has been overtaken by an invasive. Whatever it is, you sometimes cannot repeat it the same way. And the Secret Science Act is based—as it has previously been introduced has been based on a misunderstanding of how science works.

You—the gold standard is to find other approaches to come up with the same conclusions. Rarely can you repeat an experiment in exactly the same way, and so this cry that you have exact data that somebody else will take and put it through their computer instead of your computer, yes, sometimes that makes sense, but what makes much more sense is that you approach the problem with a new perspective.

And so that's not what the—that's not where this secret science legislation is heading. It's a misunderstanding of what it means to replicate experiments. So I think that—well, anyway, you've said it.

Ms. ROSEN. Well, thank you. So I guess you would say then that the goal of science is to take these studies, collaborate, innovate, and inspire the next scientist to take the foundation of what you have put there to look at new hypotheses and new ways to find solutions or analysis of a situation.

Dr. HOLT. Science doesn't make progress by doing the same thing over and over again. Science makes progress by looking at problems from new perspectives and testing it this way and that way and the other way and ultimately converge on an understanding that is more reliable than you have from one experiment. And that's how science works.

It also means, of course, that the conclusions might have to be refined. The—you know, I know it bothers some Members of this Committee that sometimes they hear that different standards might be set. Well, yes, as the science progresses, you may change.

Ms. ROSEN. Where the data takes you.

Dr. HOLT. And you should change——

Ms. ROSEN. Yes.

Dr. HOLT. —but not on the basis of political whim or personal preference.

Ms. ROSEN. Thank you. I appreciate that. And I yield back my time.

Chairman SMITH. Thank you, Ms. Rosen.

And the gentleman from Arizona, Mr. Biggs, is recognized for his questions.

Mr. BIGGS. Thank you, Mr. Chairman. I appreciate you addressing this issue. I thank each member of the panel for being here today.

I would like to just shift discussion briefly to an issue I think we haven't heard much about today and that's the Waters of the U.S. or the WOTUS rule. The EPA has consistently claimed that the WOTUS rule would not significantly expand its jurisdiction, but I look upon that claim with some skepticism because of what I'm hearing from my constituents in Arizona.

And so I'm asking Mr. Holmstead, right now, do you agree with the EPA that the Waters of the U.S. rule is not a significant expansion of their jurisdiction over waters and, in the case of Arizona, dry wash beds?

Mr. HOLMSTEAD. No, I think it's pretty clear that the WOTUS rule does significantly expand EPA's jurisdiction. They—what—part of their claim is that they've cast a broad net and then you can come in and somehow be excluded, but that process takes a lot of time and effort. But the jurisdiction that EPA claims is certainly much broader than we've seen before.

Mr. BIGGS. It seems to speak to an idea that perhaps there can be an institutional bias, and I would categorize it as something in line with something like institutional maintenance, and in this instance it's to regulate to expand jurisdiction. And I think you've just indicated that you agree with that perhaps maybe not that there's a bias but certainly the rule is. Do you see a bias there?

Mr. HOLMSTEAD. I think institutionally, EPA—and this is not just EPA. I think this is regulatory agencies at federal and local and state levels, but that there is a tendency for them to want to increase their regulatory power basically because they want to have an opportunity to impose their own will on many of these choices. So I think there's no question that we've seen EPA expand its regulatory power or at least try to expand its regulatory power over the years.

Mr. BIGGS. Many manufacturers have indicated they're going to be impacted by this particular rule, the Waters of the U.S. rule. Can you explain what areas of manufacturing that might be impacted by this and any other anticipated results that we might see of WOTUS being applied to them?

Mr. HOLMSTEAD. You know, most of the concerns that I have heard have been with regard to not existing manufacturing plants but people who want to build anything that's new, whether that's a transmission line or a pipeline or a new facility. If it turns out you want to locate whatever you're building in an area that's included within this broad definition, it becomes much, much more difficult to do that.

Mr. BIGGS. So I guess, Mr. Holmstead, to be more direct, we're talking really about permitting issues really become a major problem with this expansion of rules?

Mr. HOLMSTEAD. Yes, that's absolutely right. The fact that you need government approvals to—and in some cases many different approvals for many different agencies I think is a shame. It's become very hard to build new things in this country, and I think that kind of permitting reform—not only reforming the WOTUS rule but other permitting programs to get them to function more efficiently would be a huge step in the right direction.

Mr. BIGGS. Thank you. Thanks, Mr. Chairman. I yield back.

Chairman SMITH. Thank you, Mr. Biggs.

And the gentlewoman from Oregon, Ms. Bonamici, is recognized.

Ms. BONAMICI. Thank you, Mr. Chair.

Welcome back, Dr. Holt. It's nice to see you again. We certainly miss you on the Committee, but I very much appreciate the work you do with the AAAS. And before I ask my question, I want to make sure that all of our colleagues know about the event that the AAAS helps to organize each year, the Golden Goose Awards. This is an award that is earned by groups of federal hardworking researchers whose seemingly obscure federally funded research has led to major breakthroughs in national security, public health, computing, energy, and the environment. So it's a great annual bipartisan event and I hope all of our colleagues join us to support scientists and federally funded research.

I want to align myself with the remarks by my colleague from Virginia, Mr. Beyer. Dr. Holt, as you know, our planet is facing the real consequence of anthropogenic climate change, and I'm deeply disappointed that we're not beginning this session focusing on how the EPA can address that critical issue as part of its mission to protect human health and the environment.

In my home State of Oregon, the renewable energy industry has created thousands of jobs and is growing, and I wanted you to follow up on—I know Representative Esty asked you about the human health effects. Can you also talk about the economic benefits of addressing climate change?

Dr. HOLT. I know that there have been many criticisms over the years of the cost of environmental regulation and the cost of addressing climate change as one of the biggest environmental challenges. And most of that debate pays insufficient attention to the cost of not addressing it, the costs in lives and dollars. And that goes for climate change, as well as regulations restricting pollutants and other environmental hazards. And there's an extensive literature on it. It's difficult, of course, to calculate because some of the benefits and some of the costs are second and even third order and indirect.

But it's pretty clear, I would say, that a cleaner environment such as we have obtained through environmental regulation implemented by the EPA—that a cleaner environment is economically better in addition to being better for human health.

And for climate, you know, it remains to be seen how hard we're going to work to bring climate change under control and how expensive it will be if we don't do a good job——

Ms. BONAMICI. And I want to talk——

Dr. HOLT. —and the benefits from the industries in trying to do a good job.

Ms. BONAMICI. Thank you. And I wanted to follow up on a discussion that was already brought up today about the—a piece of legislation that has been introduced that proposes abolishing the Environmental Protection Agency and basically like going back to the days when States would regulate clean air and water.

So I know that under the Obama Administration there were partnerships between States and the EPA. This is something that's going to take federal and international solutions. Can you talk a little bit about what would happen going back to the days when there was no EPA if we let States regulate clean air and clean water?

Dr. HOLT. Well, Representative Beyer has touched on this. Rivers caught fire, people lived in levels of smog that we only see in China these days. Well, actually they're probably seeing it a little bit worse even. But the—and so my response to that is the regulations by and large have worked, and that's what we should be looking at.

Ms. BONAMICI. And, Dr. Holt, also in the 114th Congress the House considered legislative proposals that would've applied the research restrictions that are on the EPA from this so-called Secret Science Reform Act to research efforts at all agencies. If such a proposal were to become law, what would the effect be on the research mission not only of the EPA but our other federal agencies as well? And this Committee has jurisdiction over the NSF, for example.

Dr. HOLT. Well, there isn't an agency in the government that doesn't have significant science components. We at AAAS wrote to every one of President Trump's nominees for a Cabinet position, and I said in the letter "I hope you realize that you are heading up a science agency." The Attorney General, yes, he would benefit by having a better understanding of forensic science. The Housing and Urban Development, yes, there is a social science and other science that is done by the Department but also science that must be used by the Department if they're going to make good decisions.

The point is science-based policymaking is important in every aspect of our government. And if the scientific process, the free communication, the free collaboration, the ability to operate without intimidation is compromised anywhere, it will hurt our government's functioning, it will harm the economy and human welfare.

Ms. BONAMICI. Thank you very much. My time is expired. I yield back. Thank you, Mr. Chairman.

Chairman SMITH. Thank you. Thank you, Ms. Bonamici.

And the gentleman from Indiana, Mr. Banks, is recognized for his questions.

Mr. BANKS. Thank you, Mr. Chairman. And thanks to the each of the four of you for being here today to educate us and discuss ways that we can look at reevaluating the role of the EPA in 2017 and beyond.

Growing up and fighting for and subsequently representing northeast Indiana before coming to Congress in the Indiana State House has made me aware of the many ways in which the EPA's policies impact hardworking constituents in my district with citizens from every walk of life, from farmers to small-business own-

ers, workers, and even family members. I've heard from many of these constituents over the years raising concerns directed at the broad, burdensome, and relatively clandestine authority exercised by the EPA.

So therefore, I've been a firm believer that our government, where authorized, should implement environmental policies based on sound science that focus on innovation rather than regulation. Sound science is the foundation of sound regulatory decision-making.

So with that, I'm fortunate to have an opportunity on this committee—and I thank the Chairman for giving me the opportunity on this committee—to be a part of investigating and reforming the EPA's regulatory power.

And with that, I will direct my first question to Mr. Holmstead. In your written testimony you highlighted the importance of both scientific accuracy and transparency in the EPA's decision-making process. On that note, should the risk assessment process and the cost-benefit analysis process of the EPA be examined and reformed to make it more scientifically based and objective? And with that, what should Congress's role be in reforming that process?

Mr. HOLMSTEAD. I'll give you a short answer and you might want to ask Dr. Belzer, who really has had much more experience in risk assessment. I think it is appropriate for Congress to step in and to institute some reforms that could improve the process. And I want to be clear. I think EPA does a lot of very important, good things, but I also know from my own personal experience that there are some things that really do need to be reformed.

We do enjoy a cleaner environment in large part because of EPA, but in some ways we pay a lot more than we should for the protection that we get. There are better ways to do these things, and I think that's one of the things that this Committee is looking at is are there better ways for EPA to do its job.

Mr. BANKS. Okay. Thank you. And, Dr. White, there used to be a time where EPA's Science Advisory Board and Clean Air Science Advisory Committee would issue reports with dissenting views. Now, it seems like those bodies just come together in a general consensus that almost always aligns with environmental or conservation groups. Do you think the EPA's Scientific Advisory Boards could do a better job at encouraging a broader or more transparent range of views?

Dr. WHITE. Absolutely. The process needs to be balanced. I mentioned that earlier in my testimony today that if you have balance on the committee, you're going to get dissenting views, and that information needs to be captured and considered by the Agency. That's really the value of having an open and independent peer-review process is so that you get a really robust review of EPA's evaluation of the science and what it's going to use to make those decisions before they are finalized.

Mr. BANKS. Okay. Thank you. And back to you, Mr. Holmstead. Do you think it would help if the EPA presented its regulatory impact analysis in a more concise or easier-to-read format?

Mr. HOLMSTEAD. You know, I'm not quite sure how to answer that question because when you—sometimes when you make things too simple, you don't do justice to the complexities involved.

So I agree that there's room for improvement, but again, you want to be sure that these documents—that this documentation gives people enough information so that they understand that things aren't always quite as black-and-white as the Agency would make them appear.

Mr. BANKS. And then one final question for you that you might or might not agree with. The EPA has historically, in my opinion, had a shoot-first style of regulating. The Agency issues a regulation before it is fully thought through whether it could be accomplished. How does this unnecessarily create burdens for businesses and manufacturers or would you agree that it does?

Mr. HOLMSTEAD. Oh, sure, I mean, I think they're—I've long been critical of one part of the Clean Air Act that imposes a legal requirement on States and local governments that in many cases is impossible to meet. It's just not achievable. And that has all kinds of implications for businesses that want to locate there, and I just don't think it makes any sense to give an agency authority to mandate things without just—without considering whether they're achievable.

Mr. BANKS. Thank you. Thank you, Mr. Chairman. I yield back my time.

Chairman SMITH. Thank you, Mr. Banks.

The gentleman from California, Mr. McNerney, is recognized.

Mr. MCNERNEY. Well, I thank the Chairman for holding this hearing.

It seems to me that the issues of contention today would be how science is conducted, meaning does it follow standards, does it follow standards that science usually follows, is it peer-reviewed, and is it influenced by politics or not, and, on the other hand, how science is used in rulemaking.

So my first question will go to Dr. Holt. Would the Secret Science Reform Act ensure that science follows science standards that's used at the EPA?

Dr. HOLT. No, I think the problem is it's an attempt to substitute a different view of what is meant by openness and a different view of what is meant by sharing than is the standard in the practice of science. And my earlier plea today was that—I'm not saying oh, trust the scientists. I'm saying trust the process

Mr. MCNERNEY. Right.

Dr. HOLT. —and don't try to rework the process because you think you'll get better results because you won't.

And with regard to the Science Advisory Committee, I mean, that is a Science Advisory Board. It will not function better by having fewer scientists on it. It is supposed to look at science. But in the name of balance and diversity, it—there's an effort to make it, well, less scientific, and that, it seems to me, not the way to go. That's not what the SAB is for. There might be other places in the Department where you bring in industry representatives to talk about the cost of the regulations to the industry, but I don't think the Science Advisory Board is the place to do that.

Mr. MCNERNEY. Well, would the Secret Science Reform Act lessen the influence of politics in the scientific process?

Dr. HOLT. No, I mean I think it's fundamentally substituting a politically originated revision of the process for the scientific process that has grown up over the ages.

Mr. MCNERNEY. Thank you. The second part is how science is used in rulemaking. My first question goes to you, Dr. Belzer. What are the standards used in the cost-benefit analysis? And I know you don't like cost-benefit. I know you like benefit-cost, but I'll use it anyway. What are the standards used for human health and human life in cost-benefit analyses?

Dr. BELZER. We have 30 to 40 years of experience doing that. This is within the field of benefit-cost analysis. There isn't any controversy, regardless of one's political orientation, as to whether that ought to be done. There are technical arguments about how to do it, but there isn't any dispute in the field about whether it ought to be done and the effort ought to be put into doing it.

It's the—again, think—if you think——

Mr. MCNERNEY. Well, I mean, what I'm asking is——

Dr. BELZER. —of economics as a scientific field—and I should point out that a lot of physicists feel the same way; they become economists——

Mr. MCNERNEY. Right, but what I'm asking is what are those standards? How do you include human life and human health in cost-benefit analysis? How do you do it?

Dr. BELZER. The standard mechanism for dealing, let's say, with premature mortality, which is the largest component of benefits for most environmental regulations, is to estimate the number of premature lives that are lost. And there's premature death and then there are—there's an extensive economic literature on valuing the premature death. And so that goes into the benefits assessment.

And I should point out EPA is very fond of this. EPA uses this all the time. This is not a matter of controversy among economists. EPA has used this same literature to estimate the benefits of the Clean Air Act, and they've done so repeatedly. So the controversies with that had to do with things that are at a more technical level, not at a principal level.

Mr. MCNERNEY. So what is the cost or benefit of premature deaths?

Dr. BELZER. I'm sorry—

Mr. MCNERNEY. Well, I mean, how much does it cost? What does a premature death cost?

Dr. BELZER. What does it cost to——

Mr. MCNERNEY. Right, in your analysis.

Dr. BELZER. I'm sorry. I'm mostly deaf, so I need clarity in——

Mr. MCNERNEY. So you say it's just a technical matter. Well, then——

Dr. BELZER. Yes.

Mr. MCNERNEY. —what is the cost of a human life in your cost-benefit analysis?

Dr. BELZER. What value—agencies differ in the values that they use. I believe that EPA's figure is on the order of 9 or 10 million dollars per premature life—essentially, the value of saving or preventing a premature death. I think the Department of Transportation uses a number that's quite a bit lower than that. OMB provides guidance on how to do it but doesn't tell them what to do.

And the circumstances may well vary so there isn't a fixed value. There are underlying procedures about how to estimate for a given situation, and those procedures are pretty much the same across agencies. But agencies do differ.

Mr. MCNERNEY. So there's leeway then?

Dr. BELZER. There is leeway but it's not leeway in terms of the methods. It's leeway in terms of where the data and analysis lead you. This is the same principle Dr. Holt is talking about. Economics is a science in my view, and so we apply scientific tools, scientific method to developing these estimates.

Mr. MCNERNEY. The gentleman's been——

Dr. BELZER. There are things that are hard to estimate——

Mr. MCNERNEY. —generous in letting me run over, so I'm going to yield back at this point.

Chairman SMITH. Thank you, Mr. McNerney.

The gentleman from Alabama, Mr. Palmer, is recognized.

Mr. PALMER. Thank you, Mr. Chairman.

Dr. Holt, and Dr. White, I have very high regard for your professions. I just want to ask you, are you saying that, for instance, the scientists who work on the Intergovernmental Panel on Climate Change for those whose work has been in regard to climate change should be held in high regard subject to same peer review of everybody else but given respect? Is that—Dr. White?

Dr. WHITE. Yes.

Mr. PALMER. Okay. How about you, Dr. Holt?

Dr. HOLT. Yes, I think so. I'm not quite sure I understood your question but—

Mr. PALMER. Well, I'm just asking if these people are——

Dr. HOLT. —the scientists who work on the Intergovernmental Panel on Climate Change that I know personally I highly respect, and when I look at the work that has come out of that, I would say that's good science.

Mr. PALMER. And would you say that's true of the other scientists that work on that? I mean, they have to be held in pretty high regard to be added to that panel.

Dr. HOLT. Yes.

Mr. PALMER. Okay. I just want to introduce this for the record, Mr. Chairman, that there's a number of scientists who worked on the Intergovernmental Panel on Climate Change who now fiercely dispute what the panel has produced in terms of their projections on climate change. In fact, one of them says, "Warming fears are the worst scientific scandal in history. When people come to know the truth—what the truth is, they will feel deceived by science and scientists."

So if I may, I'd like to submit that for the record.

[The information appears in Appendix II]

Mr. PALMER. Dr. Belzer, there's been a lot said here about asthma and health as a result of human activity. I just want to share some things with you. From 1980 to 2012, our gross domestic product increased 467 percent. Vehicle miles traveled went up 94 percent. Populations increased 38 percent. Energy consumption is up 22 percent. Emissions, however, are down 50 percent. But the interesting thing is that—and these are EPA stats, these are U.S. government stats—is that even though our air quality and water

quality are demonstrably better, particularly air quality in regard to asthma, than they were in the '60s and '70s and '80s, asthma rates have exploded. Can you give any explanation for that?

Dr. BELZER. I am familiar with some of that literature, and I suspect but don't know that one of the things that's changed is the definition of asthma has expanded. And if you increase the—if you make the definition broader, you're going to have more people in it. So that certainly could be part of it.

But I do agree that this is a conundrum with declining air pollution that you would have increasing asthma is certainly contrary to the expectation and belief of many people who think or believe that asthma is caused by air pollution. So this is a problem. This is one that good science ought to be put to it, and we ought not to impart upon it a policy judgment first as to what the answer is.

Mr. PALMER. Okay. Here's what I want to point out. First of all, I'm not a climate denier, as some claim. I think that it is caused by natural variations. I think there's enough science out there that indicates that that's a viable position to have. But also in regard to what's been said in this committee that it's clear, I think, from particularly what some of my colleagues on the other side have said that they want to link asthma to human activity, pollution. And in every respect—I mean, lead, carbon monoxide, sulfur dioxide, the volatile organic chemicals, nitrogen dioxide, PM10, PM2.5, everything is down, yet they want to make it about that. And I think that's a misrepresentation of science, the very thing that we're trying to avoid here.

And I also have an article, Mr. Chairman, I'd like to enter into the record from Scientific America to your point, Dr. Belzer. We don't know what causes asthma. There's everything—hypothesis from hygiene to obesity to sedentary lifestyles to more—to poor housing quality for lower-income families. So I really do think where we're trying to go with this committee, to be able to validate the science, to get the politics out of it is the place we need to be.

I yield back.

Mr. LUCAS. [Presiding.] The gentleman yields back.

Without objection, the gentleman from Illinois, Mr. Foster, and the gentlelady from Hawaii, Ms. Hanabusa, are authorized to participate in today's hearing. They've been selected to serve on the Science Committee but have not officially yet been appointed.

Seeing no objection, the Chair now recognizes Mr. Foster for five minutes.

Mr. FOSTER. Thank you, Mr. Chairman. And if it's—it's my understanding that just as we speak I think I'm likely to be officially appointed to the committee but——

Mr. LUCAS. You're right.

Mr. FOSTER. Yes. So, Dr. Holt, you know something about Science magazine, and there was an interesting article a couple weeks ago entitled "The Polluted Brain," which had a very interesting discussion of the growing evidence for the link between particulates that are about an order of magnitude's too small to be detected by normal air-quality monitoring equipment and dementia and Alzheimer's and had some actually rather alarming numbers in it. The—and as well as an honest discussion of the scientific uncertainty in this.

And the potential economic impacts of this are huge. About roughly 1/3 of all of our Medicare spending is projected to be due to Alzheimer's within a couple of decades. You know, if Alzheimer's did not exist, there would not be long-term financial stress on Medicare. And so this is a—it's a huge issue to the extent that it is attributable to this.

And there's also difficulty that is traditionally not—these are things—instead of 2.5 microns and above, these are 200 nanometers and below. And so that there is a significant belief that they penetrate through the blood brain barrier and actually do damage to the brain. It's well-documented that they cause things like asthma, cancer, and recently, heart disease. But now, the fact that they could be responsible for a significant amount of that is I think a source of growing concern and potentially a subject that this Committee will be discussing a lot and the EPA should be raising.

And so I raise it because it's sort of a prototypical example of how science is a moving target in environmental regulation. And it strikes me that there are two dangers that we have, one of which is that, as soon as this danger begins to have some scientific plausibility associated with it, large commercial interests will try to suppress that science. I mean, we saw that in cigarettes and a number of other areas.

And so I was wondering if any of you could comment on the best way to prevent that from happening because there will be a large number of manufacturing products, consumer products, and so on that will be responsible for exposing people to these very small particulates and, you know, this may cause changes in the business model. And so I was wondering if you—any of you have comments on how to make sure that that does not happen in a way that happened with cigarettes. Or can't it happen anymore?

Dr. HOLT. Well, Representative Esty asked earlier why are studies some studies more creditable—credible than others? And, you know, one can certainly discount studies that seem to be driven by special interests. It's not enough to just disclose where your funding comes from but that at a minimum should be done.

But then, as you know in the tobacco case and in some of the other cases, it's dependent on finding forensic evidence, you know, internal communications of collusion. That was certainly true when Henry Waxman exposed the collusion in the tobacco industry. So, you know, there's no easy way that I know of to do that.

Mr. FOSTER. And Dr. Belzer, to sort of follow up on Representative McNerney's question about—you know, there's a number—I think you cited $10 million per human life or some number like that. How do you value one year of suffering from Alzheimer's? Is there a number associated with that when we do the cost-benefit or is it only deaths that we typically consider?

Dr. BELZER. It is certainly true that much—or most of the research effort has gone into estimating benefits from mortality. The reason—logical one is that mortality is the worst health effect that one normally can imagine. I am hesitant to agree with that because Alzheimer's is one of those things that might well be worse for a lot of people. My mother endured almost 20 years of it before her death in 2012, and it is——

Mr. FOSTER. As my mother did.

Dr. BELZER. —but coming—but figuring out how to estimate it, it requires some resources to be devoted to it. I don't think there are any economists who would shy away from the effort. They would find it extremely interesting and challenging and would put forth the best possible objective effort to do so in collaboration with the neurobiologists who would be best equipped to help us.

Mr. FOSTER. Would that effort to get to the real scientific and economic analysis be easier or harder if the size of the EPA staff was cut by factor of three?

Dr. BELZER. I'm not sure that it has any correlation at all with the size of the EPA's staff.

Mr. LUCAS. The gentleman's time is expired.

The Chair now recognizes the gentleman from Texas, Subcommittee Chairman Mr. Babin, for five minutes.

Mr. BABIN. Thank you, Mr. Chairman. I appreciate it.

Risk assessment guidelines will enable the EPA to achieve consistency in the conduct of chemical risk assessments and will help avoid manipulation of evidence and assumptions to achieve predetermined results. It is my understanding that existing risk assessments guidelines are out of date and do not offer critical guidance that relates to current and evolving risk sciences.

It's also painfully apparent that the EPA risk assessors do not consistently follow all aspects of their existing guidelines. For example, the Science Committee's investigation into EPA's assessment of glyphosate found significant flaws in the scientific process of reregistering chemicals that warrants further examination.

I believe that updated risk assessment guidelines should contain clear criteria for causal analysis so that there is as little room as possible for subjective judgment that reflects the policy leanings of the analyst. The guidelines should identify, through careful evidence integration, the conclusions that have the strongest scientific support.

And I'd like to start with you, Dr. White. Do you believe the EPA should update and revise its risk assessment guidelines? Do you believe that the development of updated risk assessment guidelines and adherence to these guidelines will inject more objective scientific rigor into EPA's chemical risk assessments? And will that make the development of risk assessments more transparent?

Dr. WHITE. Thank you, Congressman, for that question. Transparent and consistent framework for evaluating chemicals is necessary. It needs to be transparent and, like I said, it needs to be consistent and science-based. As the science changed, risk evaluations need to be updated. So do the processes that are used.

I mentioned 2016 Congress passed the Lautenberg Chemical Safety Act, which requires the EPA to make its decisions using the best available science and a weight-of-evidence process. What that means is a weight of evidence utilizes the—all the available science. It clearly identifies the criteria that it will use to identify the available data. It evaluates the quality of that data.

And I think it's important to note when we talk about the quality of the data, just to go back to something Dr. Holt said about studies funded by industry and whether they should be discounted, I do not feel that a study should be discounted solely based on the funding authority. It should be reviewed and evaluated based on

the merits of the actual scientific context and the study design and the value that that information will provide to the assessment.

When EPA or any other agency evaluates the available scientific information, it needs to determine whether or not they have high-quality information, looking across the board at all studies. One of the reasons that it's important to do a weight-of-evidence process is because it allows the Agency to look across the board at all the available evidence to evaluate the quality of that evidence and then use that information to integrate and make decisions. This is information both on available human data, on the animal data that might be relevant to human exposures, and any mechanistic data that will tell us about how a chemical may be acting in the body.

Mr. BABIN. Okay. And how would you suggest risk assessment guidelines account for uncertainties in the scientific evidence? When and how is it appropriate to use default assumptions?

Dr. WHITE. So when scientific information is available, so when we actually have human data or animal data, that information should be used instead of a default. When we're looking specifically at uncertainties, sometimes in research we have animal data and we may not have available human information. So we need to have and be able to account for how that animal data is relevant or not relevant to the human exposure incident.

Mr. BABIN. Okay. And then one last thing. Do you believe that exposure assessments should be included in revised risk assessments guidelines?

Dr. WHITE. Exposure is a key piece of the risk assessment process, so not only do you need to look at the toxicity information but also whether or not that toxicity is relevant to actual human exposures.

Mr. BABIN. Okay. Thank you very much. Would anybody else like to add to that?

Dr. HOLT. Mr. Babin, if I may——

Mr. BABIN. Yes.

Dr. HOLT. Well, first of all, in your first question, it may be true that the risk assessment guidelines need to be updated, but I just wanted to make sure that it didn't seem that I was implying that industrial research should be discounted on the face of it. Maybe Dr. White thought I said that. I don't—I certainly didn't mean that.

Mr. BABIN. Okay. Thank you. Anybody else? With that, I will yield that the balance of my time, Mr. Chairman. Thank you.

Mr. LUCAS. The gentleman yields back one second.

The Chair now recognizes the gentleman——

Mr. BABIN. Generous.

Mr. LUCAS. —from the big First District of Kansas, Mr. Marshall, for five minutes.

Mr. MARSHALL. Thank you so much, Mr. Chairman. My first question is for Dr. White.

Dr. White, I represent the largest agriculture-producing Congressional district in the country, and I'm so proud that the air we breathe there, the waters that my children and grandson swim in are cleaner today than they were when I was growing up. As you can imagine, our farmers and ranchers utilize quite a range of tools, including herbicides and pesticides.

And kind of a follow-up to Dr. Babin's thoughts or his question on glyphosate, I'm new here so I was trying to understand it was in a report, it was out a report, but at the end of the day it looked like the conclusion was glyphosate Roundup was not a carcinogen. It's been around since 1960 and somehow that was taken out of the report and I'm just trying to fill in a few loose ends. Why was it left out of the report or taken out of it?

Dr. WHITE. So I can't speak to exactly why it was included or taken out of the report, but what I will tell you is this is why it's so important to have a consistent framework for how science evaluations are conducted is that it's clear and transparent. You can see very clearly what processes EPA would have used to evaluate glyphosate, what science they used to make their conclusions, and it would be right there in front of you so there would be no question about whether or not that data was accurate or valid.

Mr. MARSHALL. Okay. Thank you. My next question is for Dr. Belzer. I'm trying to understand these regulatory impact analyses. RIAs is another acronym for me. When they report RIAs to the OMB, is there any type of independent peer review before it's submitted?

Dr. BELZER. I would say sometimes there is an internal peer review conducted by an agency, but generally, that's not the case. There's—basically, OMB is performing the peer review. And one of the problems with that from my perspective is that OMB doesn't disclose the results of its peer review. It works for the President and so its advice is proprietary to the President. I think that things would be a lot better if OMB actually disclosed its independent reviews of these documents, and I've advocated that since I worked there and told to mind my own business.

Mr. MARSHALL. Do you think independent peer reviews would be of any help to maybe not every report but certain number of them randomly selected?

Dr. BELZER. Certainly, the larger ones ought to be. I think that the existence of an effective peer-review process improves the quality of what agencies produce in—I mean, just simply the knowledge that you're going to be peer-reviewed goes a long way toward improving quality. Then, as Dr. White's pointing out, the panel—having a good peer-review panel is very helpful.

I'm the only person here who is actually a member of a Science Advisory Board panel, and I know the processes at least for my panel how it goes on. It is a challenging enterprise with a panel of 15 members for typically only two might be knowledgeable about a given issue that's actually on the table.

So I think the peer-review process does need to be restructured to make sure that there really are experts in individual areas and that they don't—and that they have intellectual diversity. I don't care at all about who they work for, but intellectual diversity is the key toward teasing out the best scientific knowledge.

Mr. MARSHALL. Okay. My last question is for Mr. Holmstead. You look bored there. No one's asked you a question. It has to do with WOTUS, Waters of the U.S. I had the pleasure of sitting down with Senator Bob Dole six, seven months ago and had the conversation—he was one of the original co-authors of WOTUS, and of course I had to ask him, "Well, what does navigable stream mean

to you and what was your intention?" And of course he said, "Well, you know, there's only three navigable streams in Kansas, and it's very obvious what we meant."

And now there's concern for my farmers—that WOTUS is actually managing land. If you regulate water to such a great extent it feels like you're regulating land even though the Agency said it doesn't want to regulate private property. So I guess my question is how can the Agency deny that by expanding vastly the definitions of WOTUS that it's effectively limiting the activities that can occur on private property? I guess I'm trying to understand. WOTUS has expanded so much under the current interpretation that it's impacting private property.

Mr. HOLMSTEAD. You know, people are—I don't think anybody at EPA could dispute that they're regulating private property. I mean, that's what it's all about. So even if you own your land, even if you owned your land for 100 years, if somebody comes in and says there's a wetland on your land, you have to get a permit to—and you can be prevented from using that land. That's why there's been so much debate and discussion over what really is a wetland and this most recent rule that's been referred to as WOTUS really does significantly expand the net, expand the jurisdiction that—the amount of land that's covered by that process.

And I think that's inappropriate, and I also think it's the—well, I think it's probably illegal. But I also think this is an area where Congress could really step in and provide some guidance because this is a debate that's gone on for many, many years now with EPA making several stabs at trying to, you know, define the type of land over which EPA and the Corps should have jurisdiction. And that's been difficult to do, but EPA has tried several times and the courts have sent it back saying no, you've gone too far.

And I assume that this new EPA will make another stab at that and try to do something that's more reasonable and that's more understandable for people who really do care about what is a wetland.

Mr. MARSHALL. Thank you for being so candid.

Mr. LUCAS. The gentleman's time is expired. The Chair now turns to the Ranking Member for a unanimous consent request.

Ms. JOHNSON. Thank you very much. Mr. Chairman, I'd like to enter these letters, comments, and op-eds from the Asbestos Disease Awareness Organization, American Lung Association, the American Thoracic Association, and The National Environmental Health Association for—on their comments on EPA.

The article—the Intercept article "Republicans are Using Big Tobacco's Secret Science Playbook to Gut Health Rules," and in the introduction to the record of the four peer-reviewed studies in the record that collaborate the findings of the NOAA study authored by Dr. Tom Karl and published by the Science magazine in 2015.

Mr. LUCAS. Seeing no objection, so ordered. The information will be added to the record. Thank you.

[The information appears in Appendix II]

Mr. LUCAS. As we conclude this hearing, I would like to take a moment and note I think on behalf of myself and Chairman Smith, to my old colleague Dr. Holt, Rush, on the issues involving Dr. Bates' concerns, I believe that it would be good for you to take the message back to the AAAS that they owe a fellow scientist making

such claims the honor of withholding judgment until the matter is fully investigated. It's a very relevant and important issue.

And with that, I thank the witnesses for their testimony and the Members for their questions. The record will remain open for two weeks for additional written comments and questions from the Members.

This hearing is adjourned.

[Whereupon, at 1:15 p.m., the Committee was adjourned.]

Appendix I

Answers to Post-Hearing Questions

ANSWERS TO POST-HEARING QUESTIONS

Responses by The Hon. Jeffrey Holmstead

QUESTIONS FOR THE RECORD
The Honorable Lamar Smith (R-TX)
U.S. House Committee on Science, Space, and Technology

Making EPA Great Again

Wednesday, February 22, 2017

Questions for and Answers from Mr. Jeff Holmstead

1. Over the past eight years, EPA has skewed its regulatory cost-benefit analyses to accomplish policy goals. Do you think this undermines public confidence in the rulemaking process?

For the last several years, many of EPA's cost-benefit analyses have been used primarily as promotional materials designed the promote the Agency's policy goals. Rather than being used as tools for making good regulatory and policy decisions, EPA has used cost-benefits analyses to claim that virtually all its air pollution regulations provide enormous economic benefits. Understandably, this practice does undermine public confidence in the rulemaking process.

This is made possible by the way in which EPA quantifies the benefits of reducing a pollutant known as "PM2.5." The term PM2.5 means "particulate matter with a diameter of 2.5 microns or less." It is sometimes called "fine particles" or fine particle pollution. Under the Clean Air Act, EPA is required to set a national ambient air quality standard for PM2.5 that is "requisite to protect public health with an adequate margin of safety." Several years ago, the Obama EPA reviewed the standard for PM2.5, and decided to lower it from 15 to 12 micrograms per cubic meter (ug/m3). Thus, according to the Obama EPA, when PM2.5 levels are below 12 ug/m3, public health – including the health of the most sensitive individuals – is protected with an adequate margin of safety.

PM2.5 is not a typical pollutant but is actually made up of many different substances, and almost all other pollutants (SO2, NOx, VOCs, ammonia, among others) contribute to levels of PM2.5 in the ambient air. This means that virtually anything that EPA does to reduce any type of pollution reduces levels of PM2.5.

In the last 8 years, EPA has claimed that virtually all its air rules pass a cost-benefit test with flying colors – that the benefits are much, much greater than the costs. However, virtually all the benefits that EPA claims are from reducing levels of PM2.5 below 12 ug/m3 – that is, *from reducing PM2.5 below the level that EPA has found to be safe.* No matter what EPA is actually supposed to be regulating, most of the claimed benefits come from reducing PM2.5.

The so-called "Mercury and Air Toxics Standards" (MATS) rule is the most egregious example. It was promulgated under a CAA program for reducing emissions of substances listed as hazardous air pollutants or HAPs. PM2.5 is not a HAP but a criteria pollutant, and criteria pollutants are regulated under other parts of the CAA. The sole legal basis for the MATS rule was an EPA finding that mercury emissions from coal-fired power plants pose a risk to children

and pregnant woman, but the MATS rule also set emission limits for other HAPs, primarily for non-mercury metals that are found in coal and emitted in very small amounts.

When EPA did its cost-benefit analysis for MATS, it found that the human health and environmental benefits of reducing emissions of these HAPs was very small. Using a long-standing EPA methodology for estimating the benefits of reducing emissions of heavy metals like lead and mercury, EPA found that the benefits of the mercury reductions required by the MATS rule were less than $10 **million** a year and that the benefits of reducing the other HAPS were small but unquantifiable. EPA estimated that the MATS rule would cost the industry and ratepayers $9.6 **billion** a year (compared to $10 **million** in benefits for reducing mercury emissions).

But the Obama EPA argued that MATS was a great deal for society because, according to the EPA website "the value of the air quality improvements for people's health alone [from MATS] totals **$37 to $90 billion** each year. That means that for every dollar spent to reduce this pollution, Americans get **$3-9** in health benefits." (The bolded words are EPA's, not mine.)

However, *almost all these benefits come from reducing concentrations of PM2.5 below the level that EPA has found to be safe,* because EPA found that in order to meet the emission limits for mercury and other HAPs, coal-fired power plants would either shut down or install controls that would reduce pollutants like SO2 that contribute to PM2.5. So on one hand, EPA says that, when PM2.5 levels are 12 ug/m3 or less, public health is protected with an adequate margin of safety, even for people who are especially sensitive to air pollution. Then, on the other, EPA claims that tens of thousands of people are dying every year because they are exposed to levels of PM2.5 below 12 ug/m3, and the MATS rule will save as many of 11,000 of them every year.

In my view, the cost-benefit analysis that EPA has done over the last few years does not represent a serious effort to make good policy. The documents that were supposed to provide a rigorous tool for policymaking became promotional materials that EPA could use to sell its regulations publicly (as can be seen from EPA's website for MATS). These issues may seem obscure, but they do have important implications for regulatory policy, because EPA is allowed (or even required) to consider costs and benefits when taking certain actions. Using good science to understand whether and to what extent certain pollutants affect public health is the only way to look at the benefits of EPA regulations.

2. EPA has a shoot first style of regulating. The Agency issues a regulation before it has fully thought through whether it can be accomplished. How does this unnecessarily create burdens for business and manufacturing?

EPA usually does a fairly good job of trying to understand the actions that a regulated industry will need to take in order to comply with proposed regulations and to estimate the cost that such regulations will impose on the industry. The Agency does underestimate the costs in some cases, but for major rules, it normally does try to analyze the impact that a rule will have on the regulated industry. In some important cases, however, the Agency makes no such effort. The most notable example is EPA's recent decision to lower the national ambient air quality standards (NAAQS) for ozone.

Rather than trying to evaluate the measures that will actually be needed to attain the more stringent ozone NAAQS, EPA simply tried to calculate the total tons of pollution reduction that would be needed and then assumed a relatively low "cost-per-ton" at which all these reductions could be achieved. This assumed cost-per-ton number is not supported by any real analysis and is much lower than the "cost-per-ton" already being incurred in several parts of the country. Moreover, states and industry have already taken the most cost effective steps to reduce pollution, and additional measures will be increasingly more expensive. EPA's assumed cost-per-ton number is simply not credible.

Moreover, regulators in some parts of the country – those who have many years of experience trying to reduce ozone levels – do not believe it will be possible to achieve the new ozone standard. EPA does a disservice to the public when it intentionally understates the cost of its regulations and insists that they are achievable without any real attempt to evaluate whether this is the case.

In the case of ozone and other national ambient air quality standards, the U.S. Supreme Court has ruled that EPA must set a standard based solely on what is requisite to protect public health with an adequate margin of safety – and that it cannot consider the cost of achieving the standard. However, refusing to do a serious analysis of the cost of achieving the standard – or even whether it will be achievable in certain areas – EPA does a disservice to policymakers, regulated industry, and the public. If EPA were to disclose the true cost to society of meeting the ozone standard – and that it is not achievable in certain areas of the country – it is likely that Congress would amend the statute to adopt a more reasonable approach for dealing with air pollution.

3. President Trump recently announced a review of streamlined permitting processes and regulatory burdens on American manufacturing. Are there issues regarding the EPA air permitting processes that you think should be included in this review?

There is a Clean Air Act (CAA) permitting program that imposes a substantial and unnecessary burden on American manufacturing – a program known as "new source review" (NSR) that now makes it impossible to get a permit to build or expand a manufacturing plant in certain parts of the country and makes it prohibitively expensive in others. Even where it is possible to get an NSR permit, the permitting process is often lengthy, burdensome, and unpredictable.

Under the current NSR program, before a company can even begin construction on a new industrial facility or on a project to expand an existing facility, it must first go through the NSR permitting process and obtain a permit that, among other things, ensures that the new or expanded facility will employ up-to-date pollution control technology. This requirement, however, is not normally an obstacle because plant owners have already identified the most effective pollution controls and are planning to install them.

It is other NSR requirements that impose a large and unnecessary burden on U.S. manufacturing. The first is the "offset" requirement that applies in any part of the country that does not meet all the NAAQS set by EPA. Such areas are called "nonattainment areas." Anyone who wants to build a new major plant in such an area – even one with state-of-the-art pollution controls – must obtain pollution "offsets" from other facilities in the same area. It is this offset requirement that

effectively prohibits new plants in some parts of the country because there are no other facilities in the same area. With EPA's increasingly stringent standards – especially for ozone – some areas with no industrial facilities do not meet the standard because of natural background pollution and emissions from areas that may be more than a hundred miles away.

The idea behind offsets is that, in order to build a new industrial facility in a nonattainment area, a company must pay someone else to reduce emissions in that same area by an amount that exceeds the emissions that will come from the new facility. Depending on the area, it must obtain offsets that are between 10 and 50 percent greater than the projected emissions from the new facility.

Not surprisingly, offsets cannot be created on the basis of actions already required by EPA or state regulations. To be counted as an offset, an emissions reduction must go beyond what is required by law. But for more than 40 years, EPA and states have been looking for every conceivable way to reduce emissions related to ozone. In many areas, all the cost-effective emissions reductions have been mandated by regulation. Where any reductions can be made, they are very expensive.

For example, the Houston area, especially near the Houston Ship Channel, has numerous industrial facilities, but they are generally very well controlled. Because there is so much industry, it is possible to purchase offsets, but they are very expensive. Houston area offset prices vary from $150,000 to $200,000 per ton for volatile organic compounds (VOCs) and $80,000 to $100,000 per ton for nitrogen oxides (NO_x).[1] Even a relatively small facility with state-of-the-art controls will emit more than 100 tons per year of these pollutants. The so-called "offset ratio" in the Houston area is 1.4 to 1, meaning that the new facility would need to offset 140 percent of its projected emissions. Thus, even if the new facility will emit only 100 tons per year of NO_x and VOCs, the company trying to build it would need to purchase 140 tons of NO_x offsets and 140 tons of VOC offsets. At current offset prices, this means an upfront cost of $32 million to $52 million just to purchase emissions offsets.

In the South Coast nonattainment area in California, average offset prices in 2014 were $23,500 per ton for VOCs and $63,000 per ton for NO_x.[2] Moreover, the quantities involved in these emissions offset transactions are relatively small compared with the emissions from a new manufacturing plant.[3] If the applicant does not have a facility in the nonattainment area that it can readily control (or tear down) to provide offsets, then emissions offsets for five or more years in the future are reportedly hard or even impossible to find.

The second part of the NSR program that imposes a substantial burden on American manufacturing is a requirement that applies in areas that do meet EPA's air quality standards.

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[1] MIKE TAYLOR, SEPTEMBER 9, 2014, UPDATE ON SCARCITY OF HOUSTON-GALVESTON-BRAZORIA (HGB) EMISSION REDUCTION CREDITS (ERCS) AND ALLOWANCES, AND USE OF NO_X ERCS FOR VOC ERCS (2014), http://www.awma-gcc.org/docs/Sept2014Presn.pdf; TEXAS COMMISSION ON ENVIRONMENTAL QUALITY (TCEQ), TRADE REPORT (2015), www.tceq.texas.gov/assets/public/implementation/air/banking/reports/ectradereport.pdf.
[2] CALIFORNIA AIR RESOURCES BOARD (CARB), EMISSION REDUCTION OFFSET TRANSACTION COSTS SUMMARY REPORT FOR 2014 (2015), http://www.arb.ca.gov/nsr/erco/erc14.pdf.
[3] NSR generally applies to sources emitting 100 tons/year of a precursor ozone pollutant.

Such areas are called "attainment areas." To obtain an NSR permit in such areas, the permit applicant must show, to the satisfaction of the permitting authority, that (1) projected emissions from the new plant will not result in changes in ambient air quality that would cause the area to exceed the NAAQS for any pollutant; and (2) even if projected emissions will not violate a NAAQS, they will not result in an increase in ambient concentrations of any pollutant that exceeds the allowable PSD "increments" set by the CAA.[4]

The requirement to show that emissions from a new facility will not "cause or contribute" to a violation of any NAAQS standard will be challenging or even impossible under the new ozone standard because many areas of the country that have always been in attainment do not meet the new standard. Until these areas are formally designated as nonattainment areas, a permit applicant would need to show that the proposed plant will not "contribute to" a violation of the new standard, which would appear to be impossible in or near areas that are already in violation of the standard.

EPA has said that it intends to create at least two options that would address this concern: (1) by setting certain *de minimis* emissions thresholds below which a new facility would be deemed not to "contribute" to a violation of the NAAQS; or (2) by allowing the permit applicant to purchase offsets. Given the history of CAA regulation, it is likely that these options, when finalized by EPA, will be challenged in court. Even if they pass muster in the courts, it remains to be seen whether either of these options will be practically viable—especially for large industrial facilities.[5] If not, it will not be possible to build or expand a new industrial facility in certain areas, even if the facility would use state-of-the-art technology to control its emissions and even if the local community desperately wants it to be built.

The ideal solution to these problems would be a narrow statutory change that would allow manufacturing facilities to be built or expanded as long as they use the best available technology to control their emissions and the state or local environmental agency finds that projected emissions from the new or expanded facility will not pose an appreciable risk to human health or the environment. But even without any statutory change, EPA could take a number of steps to expand the pool of offsets and allow permit applicants to use more reasonable approaches to show that a new plant will not cause or contribute to a violation of a NAAQS or exceed an applicable increment.

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[4] The CAA established PSD increments for PM and SO_2 for the three classes of attainment areas: Class I (pristine), Class II (intermediate), and Class III (growth). EPA has established PSD increments for the other conventional pollutants through rulemaking.

[5] For example, it appears that a number of rural areas may exceed the new 70 ppb ozone standard—not because of local emissions but because of background ozone and pollution transported from distant sources. Some areas have no local stationary sources and thus no way to generate offsets that can be used by new plants. In such cases, the offset requirement will impose a de facto ban on most types of industrial development

4. Many environmental laws leave regulatory decisions to the discretion of the EPA Administrator, but EPA bureaucrats often hijack the process to pressure their desired outcome. Are there ways in which the Administrator and his subordinates can reclaim the policy-making process at EPA – particularly for air policy?

It is certainly true that some EPA career staffers are inclined to impose stringent requirements on industry whenever they can, even when the cost of such requirements far outweighs the benefits. But effective political leadership can overrule such actions, and this is one of the primary roles for political appointees at EPA. I do not believe that the career staff can be blamed for the regulatory overreach that we have seen at EPA over the last 8 years.

Based on my experience as the head of the EPA Air Office during the George W. Bush Administration, I would say that most EPA career staffers are true civil servants and are dedicated to implementing the decisions made by the Agency's political leadership. This is certainly true among senior career officials in Air Office, which implements the CAA. Several senior career staffers worked very hard to implement and defend major regulatory reforms that were my highest priority as a political appointee – reforms that were strongly opposed by environmental activists because they reduced the regulatory burden on industry. But I know that many of these same staffers worked just as hard on the most costly and overreaching regulations issued during the Obama years. I am confident that they will work just as hard on the regulatory reforms that I hope and expect to see from the Trump Administration under Administrator Pruitt.

I also know of several cases in which the EPA career staff made recommendations to Obama officials that would have reduced the cost of EPA regulations, but the Obama appointees rejected those recommendations in favor of more stringent and burdensome requirements. Especially when it came to coal-fired power plants, the Obama EPA often looked for ways to load up the regulatory burden as much as possible in the hope that such plants would shut down and be replaced by the type of generating facilities favored by the Obama Administration.

In my view, the best way to reign in EPA would be for Congress to pass legislation to reform parts of the Clean Air Act and other environmental statutes that have proven to be particularly burdensome and even counterproductive. But even without new legislation, a President who is opposed to overregulation can appoint political officials who understand EPA's regulatory programs and are committed to reforming them in ways that would substantially reduce the costly and unnecessary regulatory burden now imposed by EPA regulations. I believe that President Trump has made one such appointment in Administrator Pruitt and will continue to make others, and I am confident that they will be able to accomplish this goal.

Responses by Dr. Kimberly White

House Committee on Science, Space, and Technology
Hearing Titled: Make EPA Great Again
Hearing Date: February 7, 2017
Questions and Responses for the Record

Question 1: President Trump recently announced a review of streamlined permitting processes and regulatory burdens on American manufacturing. Are there issues regarding the EPA air permitting processes that you think should be included in this review?

Response to Question 1: A review to improve and streamline the EPA's air permitting process will help facilitate the timely processing and implementation of necessary permits. The following issues should be evaluated in the review:

- A process for determining the utility and benefit achieved from fully implementing and attaining existing air quality standards before new standards are considered
- A process for developing and disseminating timely guidance when the Agency issues new National Ambient Air Quality Standards (NAAQS)
- A process and associated guidance to ensure that the science evaluation uses a weight of evidence process and that the findings support the revision of existing air standards
- A process for establishing guidance and polices associated with the review and granting of Prevention of Significant Deterioration (PSD) permits that:
    - Allows a source to make the PSD required demonstration of compliance with a distinct secondary standard through a demonstration of compliance with the primary NAAQS
    - Indicates how facilities may obtain offsets and demonstrate they do not "cause or contribute" to a violation of the NAAQS when obtaining a PSD permit

Question 2: Many environmental laws leave regulatory decisions to the discretion of the EPA Administrator, but EPA bureaucrats often hijack the process to pressure their desired outcome. Are there ways in which the Administrator and his subordinates can reclaim the policy-making process at EPA – particularly for air policy?

Response to Question 2: EPA's policy-making process must be firmly based on the best available and relevant scientific information and modeling approaches. In order to have a transparent, objective process for making policy decisions, the Agency must establish clear, consistent and systematic approaches for identifying, evaluating and integrating science information to support regulatory decisions. There is also a clear distinction between science and policy judgments. The Agency's policy judgments need to be identified, clearly discussed and documented in the administrative record. Notably, a 2009 report by the Bipartisan Policy Center titled: Improving the Use of Science in Regulatory Policy, included a recommendation that "*The Administration needs to promulgate guidelines (through executive orders or other instruments) to ensure that when federal agencies are developing regulatory policies, they explicitly differentiate, to the extent possible, between questions that involve scientific judgments and questions that involve judgments about economics, ethics and other matters of policy.*"

Questions 3: I'm concerned that the EPA has stacked its Science Advisory Board and Clean Air Science Advisory Committee with supporters. EPA rolls out a regulation and says that it is supported by its scientists, but no one is there to offer an opposing view. Do you think there should be more balance on these advisory committees?

Response to Question 3: The chartered Science Advisory Board (SAB), the Clean Air Science Advisory Committee (CASAC) and other EPA federal advisory committees are tasked with providing independent scientific and technical peer review, consultation, advice and recommendations directly to the EPA Administrator on the scientific basis for EPA actions and programs. The usefulness and value of this advice relies heavily on the expertise, independence and balance of the peer review committee. In order to ensure comprehensive, appropriate and effective peer review, the members of the SAB and CASAC need to be free from potential conflicts of interest, and should represent a cross-section of the scientific community.

The Federal Advisory Committee Act (FACA) governs the operation of the SAB and CASAC. FACA requires that these advisory groups be fairly balanced in terms of points of view for the function to be performed by the committee. Additionally, the EPA's 2015 Peer Review Handbook, 4th Edition highlights the need for appropriate scientific expertise and balance of perspectives in all of EPA's various peer review mechanisms. Specifically, the handbooks notes "*As part of the peer review process, the Agency (or the contractor managing the peer review) must select peer reviewers who have technical expertise in the subject matter that is needed to answer specific charge questions ..... These reviewers must not only be subject matter experts, but also must be independent and free from ethics issues such as potential conflicts of interest (COIs) or an appearance of a loss of impartiality (see Sections 5.3.4 and 5.3.7) so that the integrity of the peer review is not brought into question.*"

To ensure a robust and balanced peer review process the below recommendations should be implemented consistently across all committees responsible for providing scientific and technical peer review to EPA.

- The nomination and selection process for members of the peer review committee should be open and transparent.
- All nominations submitted for consideration to the peer review committee should be subject to notice and open for public comment.
- EPA shall adequately vet selected members of the peer review committee for potential bias or conflicts of interest. This would include the appearance of inherent bias based on past public positions taken on issues relevant to the peer review committee's charge.
- Appointments to peer review committees should be based prineipally on the scientific credentials, demonstrated accomplishments, and professional credibility of the nominee. It is also important to ensure that there is sufficient representation (i.e. more than one person for a given area of expertise).

Question 4: There used to be a time where EPA's Science Advisory Board and Clean Air Science Advisory Committee would issue reports with dissenting views. Now it seems like those bodies just come to a "consensus" that almost always aligns with environmental groups. Do you think EPA's science advisory boards could do a better job at encouraging a broader range of views?

Response to Question 4: Scientific discourse and discussion of different scientific views are cornerstones to an effective peer review process. They allow for objective and independent advice that reflects the state of the scientific debate. EPA must ensure that its peer review process consistently facilitates the transparent representation of the views discussed during the review process, including any disparate and dissenting views by peer review committee members. The recommendations below should be implemented to ensure incorporation of a broad range of views.

- Peer review committee members should receive clear guidance regarding their role in the process, EPA's role in the process, and the role of public stakeholders.
- The charge to the peer review committee and charge questions directing the peer review activities should be structured to encourage and facilitate objective consideration of all relevant scientific data and views.
- A report summarizing peer review consensus and any critical or dissenting views should be developed and made publically available.
- An independent process should be put in place to oversee and ensure that EPA has adequately addressed peer review committee recommendations.

Responses by The Hon. Rush Holt

Questions for the Record
Before the
Committee on Science, Space and Technology
Hearing on "Making EPA Great Again"
by
Rush Holt, Ph.D.
Chief Executive Officer
American Association for the Advancement of Science
Executive Publisher, *Science*
March 15, 2017

1) How do you respond to those in Congress, and within the Administration, who are attempting to dismantle EPA?

> The Environmental Protection Agency's mission is to protect human health and the environment. That is something we should all agree on. While debates occur about the scope of EPA's ability to issue regulations to achieve its mission, I would stress that what should not be under debate is the EPA's support for, access to, and use of the best available science to inform these regulations. Oversight of EPA's regulatory authority is a natural occurrence that demonstrates the sign of a healthy democracy and has been transpiring since EPA's founding in 1970. However, what is unhealthy is denying EPA access to the best available science in order to undercut its regulatory authority, or unnecessarily attacking research that EPA has used because of a disagreement on regulations that cite particular studies.
>
> In addition, to carry out its mission, the EPA funds extramural research on topics like toxicology, ecosystems and climate change. This empowers scientists to discover new ways to make our drinking water safer, breathe cleaner air, and protect our environment. It also provides a powerful tool for policymakers to utilize this scientific knowledge in setting rules and regulations through traditional rule-making procedures. Attempts to dismantle the funding of this research will only limit the ability of scientists to discover ways to help us all live in a healthier, cleaner society.

2) During the hearing, one of the witnesses stressed the importance of a "benefit-cost" analysis in federal rulemaking, stating "that it is impossible for decision-makers, whether in an agency or on Capitol Hill, to understand what the implications are of actions that are being taken without benefit-cost analysis. It can't be done without that. Otherwise, it's based on emotion, it's based on politics, based on cronyism. It's based on other

factors. But – and also I would say it can't be based on science because science is the underpinning to the benefits assessment in a benefit-cost analysis."

- Do you believe this statement accurately reflects the current role of science in decision making?

  Scientific evidence can present both positive and negative outcomes, so agencies can base decisions on science. That does not discount the fact that agencies also weigh other factors when crafting policies. It is not an either-or, but a balance between the two.

- How should science be used in the decision making across the federal government

  The role of science for the federal government should be to present evidence, after peer review and thorough testing, to assist policymakers in making the best-informed decisions. The scientific process is one of removing bias and attempting, as best you can, to engage in evidentiary-based decision making. As a result policymakers can benefit tremendously from the outcomes of scientific research and input from scientists and engineers across a range of fields. Science should be an integral element across every agency of our government so policymakers have access to data and experts that enable them to make decisions based on the evidence. That is why AAAS wrote a letter to each incoming secretary of the Trump Administration pointing to the insights of scientific research and how science relates to their department, so they can make life safer and more prosperous for all of us.

3) How would you distinguish between the need to base regulations on studies that are reproducible as opposed to replicable? Why is this distinction important? Is it possible to have regulations supported by science that is reproducible and not replicable?

   The important principle of science is that the collection, analysis, and open communication of evidence can be verified through a deliberate process. In other words, it is essential that a scientist's work is subjected to verification by others in an unbiased way. It is often impossible to repeat an experiment exactly down to the last detail, and so the verification process leads to examining the procedure the experiment used, or determining whether the experimental results fit well with other experiments and the fabric of scientific understanding.

Some experiments may be fully replicable. For example, the preparation of a potential drug compound may be completed over and over given that the experimental methods are described adequately and with access to the same materials. However, consider experiments that involve a collection of samples from a large forest over the duration of many years or a clinical trial involving 10,000 patients. In these cases, it would be essentially impossible to exactly repeat these experiments for logistical reasons or because the same population no longer exists. Nonetheless, these experiments could be checked if the data were collected and described fully so that another research team could examine the methodologies, the data and repeat the analysis.

In the context of policy deliberations and regulations, it is important that the scientific process has been applied to the data. The principal point I tried to make to the committee is that it should not be a political judgment to perfect the scientific review process. Elected officials are not well suited to perfecting the scientific process and should not try. Changing the composition of scientific advisory committees or how studies are conducted, as legislation introduced before this committee in the past two congresses has tried to do, are political attempts to perfect the scientific process which are therefore misguided.

4) How do principles of transparency and scientific integrity help build public trust in government regulations? Why is that important?

Trust and accountability are integral to the research enterprise and the sharing of scientific information. AAAS has been a leader on this subject, collaborating with the National Academy of Sciences to promote informed discussions as well as resources on the standards for ethical conduct in all fields of study. If science is cited in the promulgation of government regulations, the principles of transparency – within the limitations imposed by national security, privacy, and proprietary interests -- alongside scientific integrity help build public trust because the public will know that the science used in regulations was conducted in an appropriate manner.

I mentioned in my testimony, scientists -- whether in industry, academia, or the government -- must have confidence that they can conduct their work in an atmosphere free of intimidation or undue influence. Policymakers should never dictate the conclusions of a scientific study or disparage scientists because the results of a study contradict a previously held belief. The integrity of the process must be upheld so that the public can trust in both the scientists conducting the research and the policymakers utilizing the research.

5) In your testimony you stated that "removing concepts like reproducibility and independent analysis from the hands of scientists and into a legislative chamber or a court room" impacts the scientific process. Can you please describe what those impacts would be?

As mentioned in my response to question 3, scientists have processes and procedures to verify research results through reproduction, replication, and independent analysis. These are terms and practices of the scientific enterprise which scientists undertake to review their work and test their theories. Legislatures and courts can ask whether these principles are upheld in the practice and use of science by an agency, but legislatures and courts should not attempt to redefine the scientific process to suit their wishes. If they do, these principles will then be taken out of the scientific community and thrust into an inappropriate forum. This risks individuals or groups not familiar with the process of science altering the scientific process and turning science into a political combat zone. This would have a chilling effect on any scientist whose research is used by the EPA that their work could be litigated. This is not the type of message we should be sending the scientific community. We should be encouraging more science and scientists to be engaged in the policy process, not subjecting their work to lawsuits.

6) How would undermining the scientific integrity of research undertaken at a federal agency affect the draw for immigrants to pursue research in this country, and more importantly, to stay and work in the US after receiving their degrees?

If the scientific integrity of research is compromised or undermined at a federal agency, it would send a dangerous message to scientists both nationally and internationally. If scientists can't trust a society or government to allow them to follow the scientific method without coercion or undue influence, it stymies progress. As I mentioned in my testimony, scientific progress depends on openness, transparency, and the free flow of ideas and people. These are the principles that have helped the United States attract and richly benefit from scientific talent. However, should there be interference of these principles at a federal agency, there is no doubt that scientists will not want to work there, whether a US citizen or immigrant. This will thwart the United States' role as the global leader on innovation and scientific advancement.

Responses by Dr. Richard Belzer

U.S. House Committee on Science, Space, and Technology

Hearing: "Making EPA Great Again"

February 7, 2017

Questions and Responses for the Record

Submitted February 23, 2017

1. **"EPA will often bury the costs of its regulations while inflating benefits. Do you think it would improve regulations if EPA were more transparent in its benefit-cost analyses?"**

Every agency, including but not limited to EPA, has powerful incentives to understate costs and overstate benefits. I saw this routinely during my tenure at the Office of Management and Budget, and I have seen it routinely since I left OMB in 1998 – nineteen years ago.

The reason is very simple. Agency analysts (and their contractors) work for the program offices that have decided to propose or promulgate a regulation. Regardless of their education, training, skill or intellect, the analysts' job is to provide an economic justification for what program managers want to do. If analysts fail to support program managers, they will be denied promotion and encouraged to resign (and if they are contractors, they will not be rehired). Program managers may not have to work very hard to secure analysts' cooperation, either. Analysts (and contractors) are recruited with a keen eye for candidates who are comfortable with this role.

There is a "market" for benefit-cost analysis in regulatory decision-making. On the demand side, there are agency program managers, agency lawyers, agency appointed leadership, OMB and the rest of the Executive Office of the President, Congress and the public. Agency program mangers hire the analysts who prepare regulatory benefit-cost analyses, so they have functional control over supply as well as demand.

From the perspective of agency analysts, by far the most important of these "customers" are agency program managers. They want benefit-cost analyses showing substantially greater benefits than costs, and often they do not care what analysts must do to achieve this. An agency economist once told me that his job was to find three times as much in benefits as he found in costs. His work was not complete until he had done so, and if he failed to accomplish

Richard B. Belzer, Ph.D.
Regulation, Risk, Economics & Information Quality
Strategy & Analysis Consulting
rbbelzer@post.harvard.edu ❖ 703.780.1850

this objective, he'd be relieved of his duties and replaced with someone who could succeed, by hook or by crook. Needless to say, this environment is not conducive to honest work.

In short, there is a market failure in the production of regulatory benefit-cost analysis. For many market failures, government can, through regulation, play a decisive role by providing what the market will not. But the market for regulatory benefit-cost analysis is unlike all private markets. The government cannot solve by regulation a problem of its own creation. An agency is both the sole buyer and the sole seller of a regulatory benefit-cost analysis. This enables agency program managers to choose whatever level of quality they want. If they expect to have to defend a regulation in court and high-quality analysis is required by law, program managers will set and ruthlessly enforce high quality standards. But if they expect not to be challenged in court, or if they expect courts will defer to them on matters of analytic quality, then program managers will choose the lowest level of quality sufficient to satisfy an undemanding judge.

Transparency is a key attribute in regulatory benefit-cost analyses, but whether it's a desirable or undesirable attribute depends on whose interests are considered. It's certainly ironic that agencies practice transparently rarely, but they demand it without reservation from those they regulate. Transparency is a highly undesirable attribute from the perspective of many agency program managers, and oftentimes agency appointed leadership as well. During my years at OMB, I sought to improve transparency at every opportunity. However, I encountered implacable resistance from agency program managers. I also encountered resistance from agency lawyers, for whom transparency threatened to undermine their ability to defend agency actions in court.

Congress can try to mandate transparency through legislation, but it appears to be impossible to directly overcome the myriad internal agency incentives arrayed against it. An information quality approach is more likely to be successful. That is, Congress can act to reward agencies that rely on transparent regulatory benefit-cost analyses. Congress would need to create a straightforward legal pathway whereby an agency's lack of transparency (and failure to adhere to substantive information quality standards, such as objectivity) is justiciable in federal court. To further reward agencies that are transparent, Congress should consider making prospective litigants eligible to recover their attorneys' fees if they prevail. And, if Congress really wants to encourage transparency, it could direct that attorneys' fees be paid from the agency's budget rather than the Treasury Department's Judgment Fund. (Agencies do not care about the award of attorneys' fees if they are paid by the Judgment Fund.)

Agencies can be rewarded for practicing transparency other ways, as well. For example, an agency seeking constructive input from the public would publish an Advanced Notice of Propose Rulemaking well before it is ready to publish a proposal. That ANPRM would be accompanied by a transparent preliminary analysis of benefits and costs. If public review led to

Richard B. Belzer, Ph.D.
Regulation, Risk, Economics & Information Quality
Strategy & Analysis Consulting
rbbelzer@post.harvard.edu ❖ 703.780.1850

a consensus that effects are unlikely to exceed $100 million in any one year, then Congress could exempt the agency from performing a benefit-cost analysis along with a proposed rule (unless, of course, the proposed rule was materially different).

Agencies that fully disclose all their data, assumptions, models and the like could be rewarded even more. First, they would save money by enabling interested parties to prepare benefit-cost analyses for them. While competing interests might produce significantly different work products, they would be held to the same transparency and information quality requirements. Agencies could summarily discard third-party analyses that are not transparent or fail to comply with key information quality principles, such as objectivity. (Agencies that relied upon substandard third-party analyses would face serious legal risk.)

Second, agencies would be able to promulgate higher-quality regulations – regulations that achieved their statutory purposes cost-effectively. This would significantly reduce public controversy over rulemaking, which would benefit almost everyone. That includes a reduced propensity to litigate final rules and a greater likelihood that agency actions would withstand judicial review.

2. **"OMB issues guidance like "Circular A-4" on how agencies should perform benefit-cost analyses for regulations. How well has EPA adhered to such guidance? Do you have any other advice and thoughts?"**

Circular A-4 is generally not a helpful guidance document. It provides too little guidance for agency analysts who need help, and it imposes only a minimal barrier to agency analysts who willfully violate the established norms of benefit-cost analysis.

To give an obvious example where many agency analysts likely need help, Circular A-4 mentions "opportunity cost" 13 times. This is the fundamental principle of cost assessment. Circular A-4 correctly states that cost is properly measured in terms of the value of benefits that must be foregone in order to pay for a regulatory requirement. But the Circular provides little assistance to agency analysts who want to know how to do this. Guidance that correctly identifies the destination but omits any discussion about which highway to take (and where the potholes are located) is not particularly helpful.

Experienced agency analysts understand the opportunity cost principle, but they are free to ignore it without penalty. That's because nothing in Circular A-4, no matter how elementary or critical the provision, is actually required. Three hundred seventeen times, OMB says agencies "should" do something. As every regulator knows, "should" is hortatory and "must" is mandatory.

Richard B. Belzer, Ph.D.
Regulation, Risk, Economics & Information Quality
Strategy & Analysis Consulting
rbbelzer@post.harvard.edu ❖ 703.780.1850

Because Circular A-4 is so limited, some agencies have developed their own guidance. EPA, for example, has its own guidance, and it is a much richer and more complete work.[1] That's not to say the EPA guidance is technically superior on key principles; rather, the EPA guidance simply provides EPA analysts with much more guidance. (Problems arise where Circular A-4 and EPA's guidance conflict. In those cases, EPA analysts can be expected to treat the Agency guidance as mandatory and Circular A-4 as hortatory.)

Circular A-4 is out of date, having been published in 2003, and a case can be made that it needs a thorough updating. Whether this should be done deserves a benefit-cost analysis. On the cost side, the OIRA staff is very limited and fully occupied with other tasks, most notably implementing Executive Order 13771. Right now, what agencies need most from OMB is guidance on the estimation of cost savings from the deregulatory actions agencies are required to undertake pursuant to § 2(c), and which § 2(d) appears to direct OMB to prepare and issue. Redirecting scarce OIRA resources to revising the existing text of Circular A-4 therefore has high opportunity costs. On the benefit side, the potential value of revision to regulatory decision-making is unclear. OMB's authority to enforce Circular A-4 is delegated by the President. If President Trump wants OMB to rigorously enforce Circular A-4, OMB will do so.

When OMB began providing agency guidance on benefit-cost analysis in 1990, the available literature was not nearly as extensive as it is today. Agency analysts seeking high-quality guidance may be better served by consulting this literature. There are dozens of textbooks available for general use, and a rich scholarly literature is available to offer valuable insights on complicated issues or unique applications. On behalf of the Society for Benefit-Cost Analysis (benefitcostanalysis.org), Cambridge University Press publishes a scholarly journal (cleverly titled *Journal of Benefit-Cost Analysis*). The Society hosts scholarly meetings where agency analysts can learn from experts and present their own work. (The 2017 annual conference will be held March 15-17 at George Washington University, so few agency analysts are impeded from attending due to limited travel funds.) Another useful source of advice comes from a group of 19 (!) experienced analysts who recently published a *Consumer's Guide to Regulatory Impact Analysis*, which is undergoing peer review by the Journal of Benefit-Cost Analysis.

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[1] See U.S. Environmental Protection Agency. 2016. Guidelines for Preparing Economic Analyses. Available: https://yosemite.epa.gov/ee/epa/eerm.nsf/vwAN/EE-0568-50.pdf/$file/EE-0568-50.pdf [accessed February 23, 2017]. While the length of a document is not always a good proxy for quality, it is worth noting that Circular A-4 is 48 pages long and EPA's guidance is 302 pages long.

Richard B. Belzer, Ph.D.
Regulation, Risk, Economics & Information Quality
Strategy & Analysis Consulting
rbbelzer@post.harvard.edu ❖🖹703.780.1850

Agency adherence to Circular A-4, or more generally to accepted principle and practices in benefit-cost analysis, has been problematic for the reasons I explained in my answer to Question 1, and because presidential authority for enforcement has waxed and waned. To the extent that EPA has not complied with Circular A-4, it is because OMB has not enforced it administratively. Because it is guidance, it would be inappropriate for the courts to enforce it legally.

Congress can accomplish a great deal by requiring agencies to perform benefit-cost analysis, and creating incentives for them to comply. Congress does not necessarily need to legislate benefit-cost principles, and it should not legislate by reference a particular external authority such as Circular A-4 or ask the National Academy of Sciences to opine. The most important thing Congress can do is to incentivize compliance by making noncompliance a legally vulnerable position. Agencies should not be legally able to rely on analyses that are not transparent (i.e., they cannot be reproduced by qualified third parties) or objective (i.e., free of embedded policy preferences).

Finally, without reforms like this Congress cannot be assured of having before it the best available record of the benefits, costs and other effects of regulation. Congress needs this information, when it considers Joint Resolutions for Disapproval under the Congressional Review Act, of which by my count 48 have been introduced so far this session. Congress would need accurate information much more if it enacted H.R. 26 ("Regulations from the Executive in Need of Scrutiny Act of 2017"). Otherwise, Congress would be ill-advised to rely on the benefit-cost analyses that agencies would submit pursuant to proposed § 801(a)(1)(B)(i).

Richard B. Belzer, Ph.D.
Regulation, Risk, Economics & Information Quality
Strategy & Analysis Consulting
rbbelzer@post.harvard.edu ❖ 703.780.1850

Appendix II

Additional Material for the Record

Climate scientists versus climate data

Posted on February 4, 2017 | 700 Comments

by John Bates

A look behind the curtain at NOAA's climate data center.

I read with great irony recently that scientists are "*frantically copying U.S. Climate data, fearing it might vanish under Trump*" (e.g., Washington Post 13 December 2016). As a climate scientist formerly responsible for NOAA's climate archive, the most critical issue in archival of climate data is actually scientists who are unwilling to formally archive and document their data. I spent the last decade cajoling climate scientists to archive their data and fully document the datasets. I established a climate data records program that was awarded a U.S. Department of Commerce Gold Medal in 2014 for visionary work in the acquisition, production, and preservation of climate data records (CDRs), which accurately describe the Earth's changing environment.

The most serious example of a climate scientist not archiving or documenting a critical climate dataset was the study of Tom Karl et al. 2015 (hereafter referred to as the Karl study or K15), purporting to show no 'hiatus' in global warming in the 2000s (Federal scientists say there never was any global warming "pause"). The study drew criticism from other climate scientists, who disagreed with K15's conclusion about the 'hiatus.' (Making sense of the early-2000s warming slowdown). The paper also drew the attention of the Chairman of the House Science Committee, Representative Lamar Smith, who questioned the timing of the report, which was issued just prior to the Obama Administration's Clean Power Plan submission to the Paris Climate Conference in 2015.

In the following sections, I provide the details of how Mr. Karl failed to disclose critical information to NOAA, *Science Magazine*, and Chairman Smith regarding the datasets used in K15. I have extensive documentation that provides independent verification of the story below. I also provide my suggestions for how we might keep such a flagrant manipulation of scientific integrity guidelines and scientific publication standards from happening in the future. Finally, I provide some links to examples of what well documented CDRs look like that readers might contrast and compare with what Mr. Karl has provided.

Background

In 2013, prior to the Karl study, the National Climatic Data Center [NCDC, now the NOAA National Centers for Environmental Information (NCEI)] had just adopted much improved processes for formal review of Climate Data Records, a process I formulated [link]. The land temperature dataset used in the Karl study had never been processed through the station adjustment software before, which led me to believe something was amiss. When I pressed the co-authors, they said they had decided not to archive the dataset, but did not defend the decision. One of the co-authors said there were 'some decisions [he was] not happy with'. The data used in the K15 paper were only made available through a web site, not in digital form, and lacking proper versioning and any notice that they were research and not operational data. I was dumbstruck that Tom Karl, the NCEI Director in charge of NOAA's

climate data archive, would not follow the policy of his own Agency nor the guidelines in *Science* magazine for dataset archival and documentation.

I questioned another co-author about why they choose to use a 90% confidence threshold for evaluating the statistical significance of surface temperature trends, instead of the standard for significance of 95% — he also expressed reluctance and did not defend the decision. A NOAA NCEI supervisor remarked how it was eye-opening to watch Karl work the co-authors, mostly subtly but sometimes not, pushing choices to emphasize warming. Gradually, in the months after K15 came out, the evidence kept mounting that Tom Karl constantly had his 'thumb on the scale'—in the documentation, scientific choices, and release of datasets—in an effort to discredit the notion of a global warming hiatus and rush to time the publication of the paper to influence national and international deliberations on climate policy.

Defining an Operational Climate Data Record

For nearly two decades, I've advocated that if climate datasets are to be used in important policy decisions, they must be fully documented, subject to software engineering management and improvement processes, and be discoverable and accessible to the pnblic with rigorous information preservation standards. I was able to implement such policies, with the help of many colleagues, through the NOAA Climate Data Record policies (CDR) [link].

Once the CDR program was funded, beginning in 2007, I was able to put together a team and pursue my goals of operational processing of important climate data records emphasizing the processes required to transition research datasets into operations (known as R2O). Figure 1 summarizes the steps required to accomplish this transition in the key elements of software code, documentation, and data.

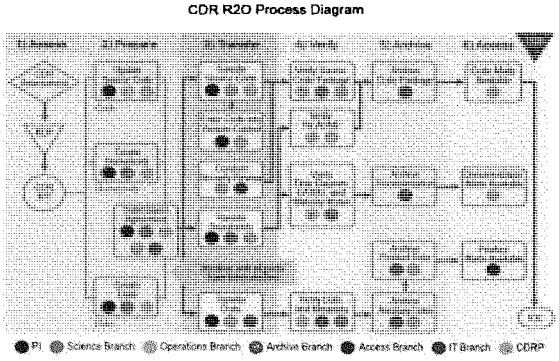


Figure 1. Research to operations transition process methodology from Bates et al. 2016.

Unfortunately, the NCDC/NCEI surface temperature processing group was split on whether to adopt this process, with scientist Dr. Thomas C. Peterson (a co-author on K15, now retired from NOAA) vigorously opposing it. Tom Karl never required the surface temperature group to use the rigor of the CDR methodology, although a document

was prepared identifying what parts of the surface temperature processing had to be improved to qualify as an operational CDR.

Tom Karl liked the maturity matrix so much, he modified the matrix categories so that he could claim a number of NCEI products were "Examples of "Gold" standard NCEI Products (Data Set Maturity Matrix Model Level 6)." See his NCEI overview presentatiou all NCEI employees [ncei-overview-2015nov-2] were told to use, even though there had never been any maturity assessment of any of the products.

NCDC/NCEI surface temperature processing and archival

In the fall of 2012, the monthly temperature products issued by NCDC were incorrect for 3 months in a row [link]. As a result, the press releases and datasets had to be withdrawn and reissued. Dr. Mary Kicza, then the NESDIS Associate Administrator (the parent organization of NCDC/NCEI in NOAA), noted that these repeated errors reflected poorly on NOAA and required NCDC/NCEI to improve its software management processes so that such mistakes would be minimized in the future. Over the next several years, NCDC/NCEI had an incident report conducted to trace these errors and recommend corrective actions.

Following those and other recommendations, NCDN/NCEI began to implement new software management and process management procedures, adopting some of the elements of the CDR R2O process. In 2014 a NCDC/NCEI Science Council was formed to review new science activities and to review and approve new science products for operational release. A draft operational readiness review (ORR) was prepared and used for approval of all operational product releases, which was finalized and formally adopted in January 2015. Along with this process, a contractor who had worked at the CMMI Institute (CMMI, Capability Maturity Model Integration, is a software engineering process level improvement training and appraisal program) was hired to improve software processes, with a focus on improvement and code rejuvenation of the surface temperature processing code, in particular the GHCN-M dataset.

The first NCDC/NCEI surface temperature software to be put through this rejuvenation was the pairwise homogeneity adjustment portion of processing for the GHCN-Mv4 beta release of October 2015. The incident report had found that there were unidentified coding errors in the GHCN-M processing that caused unpredictable results and different results every time code was run.

The generic flow of data used in processing of the NCDC/NCEI global temperature product suite is shown schematically in Figure 2. There are three steps to the processing, and two of the three steps are done separately for the ocean versus land data. Step 1 is the compilation of observations either from ocean sources or land stations. Step 2 involves applying various adjustments to the data, including bias adjustments, and provides as output the adjusted and unadjusted data on a standard grid. Step 3 involves application of a spatial analysis technique (empirical orthogonal teleconnections, EOTs) to merge and smooth the ocean and land surface temperature fields and provide these merged fields as anomaly fields for ocean, land and global temperatures. This is the product used in K15. Rigorous ORR for each of these steps in the global temperature processing began at NCDC in early 2014.

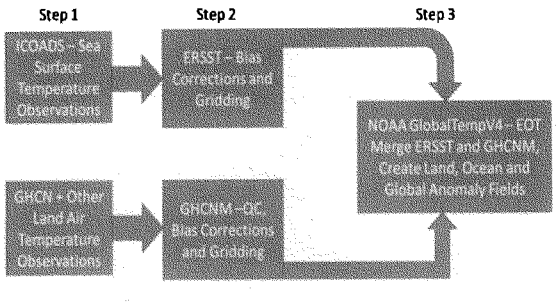


Figure 2. Generic data flow for NCDC/NCEI surface temperature products.

In K15, the authors describe that the land surface air temperature dataset included the GHCN-M station data and also the new ISTI (Integrated Surface Temperature Initiative) data that was run through the then operational GHCN-M bias correction and gridding program (i.e., Step 2 of land air temperature processing in Figure 2). They further indicated that this processing and subsequent corrections were 'essentially the same as those used in GHCN-Monthly version 3'. This may have been the case; however, doing so failed to follow the process that had been initiated to ensure the quality and integrity of datasets at NCDC/NCEI.

The GHCN-M V4 beta was put through an ORR in October 2015; the presentation made it clear that any GHCN-M version using the ISTI dataset should, and would, be called version 4. This is confirmed by parsing the file name actually used on the FTP site for the K15 dataset [link]; NOTE: placing a non-machine readable copy of a dataset on an FTP site does not constitute archiving a dataset). One file is named 'box.12.adj.4.a.1.20150119', where 'adj' indicates adjusted (passed through step 2 of the land processing) and '4.a.1' means version 4 alpha run 1; the entire name indicating GHCN-M version 4a run 1. That is, the folks who did the processing for K15 and saved the file actually used the correct naming and versioning, but K15 did not disclose this. Clearly labeling the dataset would have indicated this was a highly experimental early GHCN-M version 4 run rather than a routine, operational update. As such, according to NOAA scientific integrity guidelines, it would have required a disclaimer not to use the dataset for routine monitoring.

In August 2014, in response to the continuing software problems with GHCNMv3.2.2 (version of August 2013), the NCDC Science Council was briefed about a proposal to subject the GHCNMv3 software, and particularly the pairwise homogeneity analysis portion, to a rigorous software rejuvenation effort to bring it up to CMMI level 2 standards and resolve the lingering software errors. All software has errors and it is not surprising there were some, but the magnitude of the problem was significant and a rigorous process of software improvement like the one proposed was needed. However, this effort was just beginning when the K15 paper was submitted, and so K15 must have used data with some experimental processing that combined aspects of V3 and V4 with known flaws. The GHCNMv3.X used in K15 did not go through any ORR process, and so what precisely was done is not documented.

The ORR package for GHCNMv4 beta (in October 2015) uses the rejuvenated software and also includes two additional quality checks versus version 3.

Which version of the GHCN-M software K15 used is further confounded by the fact that GHCNMv3.3.0, the upgrade from version 3.2.2, only went through an ORR in April 2015 (i.e., after the K15 paper was submitted and revised). The GHCN-Mv3.3.0 ORR presentation demonstrated that the GHCN-M version changes between V3.2.2 and V3.3.0 had impacts on rankings of warmest years and trends. The data flow that was operational in June 2015 is shown in figure 3.

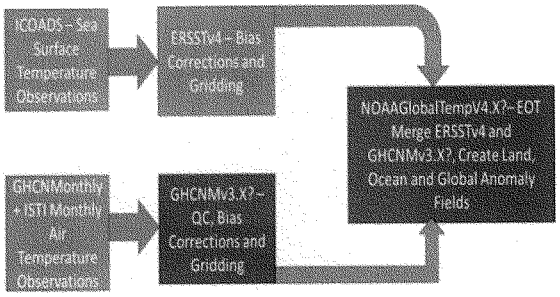


Figure 3. Data flow for surface temperature products described in K15 Science paper. Green indicates operational datasets having passed ORR and archived at time of publication. Red indicates experimental datasets never subject to ORR and never archived.

It is clear that the actual nearly-operational release of GHCN-Mv4 beta is significantly different from the version GHCNM3.X used in K15. Since the version GHCNM3.X never went through any ORR, the resulting dataset was also never archived, and it is virtually impossible to replicate the result in K15.

At the time of the publication of the K15, the final step in processing the NOAAGlobalTempV4 had been approved through an ORR, but not in the K15 configuration. It is significant that the current operational version of NOAAGlobalTempV4 uses GHCN-M V3.3.0 and does not include the ISTI dataset used in the *Science* paper. The K15 global merged dataset is also not archived nor is it available in machine-readable form. This is why the two boxes in figure 3 are colored red.

The lack of archival of the GHCN-M V3.X and the global merged product is also in violation of *Science* policy on making data available [link]. This policy states: *"Climate data. Data should be archived in the NOAA climate repository or other public databases"*. Did Karl et al. disclose to *Science Magazine* that they would not be following the NOAA archive policy, would not archive the data, and would only provide access to a non-machine readable version only on an FTP server?

For ocean temperatures, the ERSST version 4 is used in the K15 paper and represents a major update from the previous version. The bias correction procedure was changed and this resulted in different SST anomalies and different trends during the last 15+ years relative to ERSST version 3. ERSSTV4 beta, a pre-operational release, was briefed to the NCDC Science Council and approved on 30 September 2014.

The ORR for ERSSTV4, the operational release, took place in the NCDC Science Council on 15 January 2015. The ORR focused on process and questions about some of the controversial scientific choices made in the production of that dataset will be discussed in a separate post. The review went well and there was only one point of discussion on process. One slide in the presentation indicated that operational release was to be delayed to coincide with Karl et al. 2015 *Science* paper release. Several Science Council members objected to this, noting the K15 paper did not contain any further methodological information—all of that had already been published and thus there was no rationale to delay the dataset release. After discussion, the Science Council voted to approve the ERSSTv4 ORR and recommend immediate release.

The Science Council reported this recommendation to the NCDC Executive Council, the highest NCDC management board. In the NCDC Executive Council meeting, Tom Karl did not approve the release of ERSSTv4, noting that he wanted its release to coincide with the release of the next version of GHCNM (GHCNMv3.3.0) and NOAAGlobalTemp. Those products each went through an ORR at NCDC Science Council on 9 April 2015, and were used in operations in May. The ERSSTv4 dataset, however, was still not released. NCEI used these new analyses, including ERSSTv4, in its operational global analysis even though it was not being operationally archived. The operational version of ERSSTv4 was only released to the public following publication of the K15 paper. The withholding of the operational version of this important update came in the middle of a major ENSO event, thereby depriving the public of an important source of updated information, apparently for the sole purpose of Mr. Karl using the data in his paper before making the data available to the public.

So, in every aspect of the preparation and release of the datasets leading into K15, we find Tom Karl's thumb on the scale pushing for, and often insisting on, decisions that maximize warming and minimize documentation. I finally decided to document what I had found using the climate data record maturity matrix approach. I did this and sent my concerns to the NCEI Science Council in early February 2016 and asked to be added to the agenda of an upcoming meeting. I was asked to turn my concerns into a more general presentation on requirements for publishing and archiving. Some on the Science Council, particularly the younger scientists, indicated they had not known of the *Science* requirement to archive data and were not aware of the open data movement. They promised to begin an archive request for the K15 datasets that were not archived; however I have not been able to confirm they have been archived. I later learned that the computer used to process the software had suffered a complete failure, leading to a tongue-in-cheek joke by some who had worked on it that the failure was deliberate to ensure the result could never be replicated.

Where do we go from here?

I have wrestled for a long time about what to do about this incident. I finally decided that there needs to be systemic change both in the operation of government data centers and in scientific publishing, and I have decided to become an advocate for such change. First, Congress should re-introduce and pass the OPEN Government Data Act. The Act states that federal datasets must be archived and made available in machine readable form, neither of which was done by K15. The Act was introduced in the last Congress and the Senate passed it unanimously in the lame duck session, but the House did not. This bodes well for re-introduction and passage in the new Congress.

However, the Act will be toothless without an enforcement mechanism. For that, there should be mandatory, independent certification of federal data centers. As I noted, the scientists working in the trenches would actually

welcome this, as the problem has been one of upper management taking advantage of their position to thwart the existing executive orders and a lack of process adopted within Agencies at the upper levels. Only an independent, outside body can provide the needed oversight to ensure Agencies comply with the OPEN Government Data Act.

Similarly, scientific publishers have formed the Coalition on Publishing Data in the Earth and Space Sciences (COPDESS) with a signed statement of commitment to ensure open and documented datasets are part of the publication process. Unfortunately, they, too, lack any standard checklist that peer reviewers and editors can use to ensure the statement of commitment is actually enforced. In this case, and for assessing archives, I would advocate a metric such as the data maturity model that I and colleagues have developed. This model has now been adopted and adapted by several different groups, applied to hundreds of datasets across the geophysical sciences, and has been found useful for ensuring information preservation, discovery, and accessibility.

Finally, there needs to be a renewed effort by scientists and scientific societies to provide training and conduct more meetings on ethics. Ethics needs to be a regular topic at major scientific meetings, in graduate classrooms, and in continuing professional education. Respectful discussion of different points of view should be encouraged. Fortunately, there is initial progress to report here, as scientific societies are now coming to grips with the need for discussion of and guidelines for scientific ethics.

There is much to do in each of these areas. Although I have retired from the federal government, I have not retired from being a scientist. I now have the luxury of spending more time on these things that I am most passionate about. I also appreciate the opportunity to contribute to Climate Etc. and work with my colleague and friend Judy on these important issues.

Postlude

A couple of examples of how the public can find and use CDR operational products, and what is lacking in a non-operational and non-archived product

1. NOAA CDR of total solar irradiance – this is the highest level quality. Start at web site – https://data.nodc.noaa.gov/cgi-bin/iso?id=gov.noaa.ncdc:C00828

Here you will see a fully documented CDR. At the top, we have the general description and how to cite the data. Then below, you have a set of tabs with extensive information. Click each tab to see how it's done. Note, for example, that in 'documentation' you have choices to get the general documentation, processing documents including source code, data flow diagram, and the algorithm theoretical basis document ATBD which includes all the info about how the product is generated, and then associated resources. This also includes a permanent digital object identifier (doi) to point uniquely to this dataset.

2. NOAA CDR of mean layer temperature – RSS – one generation behind in documentation but still quite good – https://www.ncdc.noaa.gov/cdr/fundamental/mean-layer-temperature-rss

Here on the left you will find the documents again that are required to pass the CDR operations and archival. Even though it's a slight cut below TSI in example 1, a user has all they need to use and understand this.

3. The Karl hiatus paper can be found on NCEI here – https://www.ncdc.noaa.gov/news/recent-global-surface-warming-hiatus

If you follow the quick link 'Download the Data via FTP' you go here –
ftp://ftp.ncdc.noaa.gov/pub/data/scpub201506/

The contents of this FTP site were entered into the NCEI archive following my complaint to the NCEI Science Council. However, the artifacts for full archival of an operational CDR are not included, so this is not compliant with archival standards.

Biosketch:

John Bates received his Ph.D. in Meteorology from the University of Wisconsin-Madison in 1986. Post Ph.D., he spent his entire career at NOAA, until his retirement in 2016. He spent the last 14 years of his career at NOAA's National Climatic Data Center (now NCEI) as a Principal Scientist, where he served as a Supervisory Meteorologist until 2012.

Dr. Bates' technical expertise lies in atmospheric sciences, and his interests include satellite observations of the global water and energy cycle, air-sea interactions, and climate variability. His most highly cited papers are in observational stndies of long term variability and trends in atmospheric water vapor and clouds.

NOAA Administrator's Award 2004 for "outstanding administration and leadership in developing a new division to meet the challenges to NOAA in the area of climate applications related to remotely sensed data". He was awarded a U.S. Department of Commerce Gold Medal in 2014 for visionary work in the acquisition, production, and preservation of climate data records (CDRs). He has held elected positions at the American Geophysical Union (AGU), inclnding Member of the AGU Council and Member of the AGU Board. He has played a leadership role in data management for the AGU.

He is currently President of John Bates Consulting Inc., which puts his recent experience and leadership in data management to use in helping clients improve data management to improve their preservation, discovery, and exploitation of their and others data. He has developed and applied techniques for assessing both organizational and individual data management and applications. These techniques help identify how data can be managed more cost effectively and discovered and applied by more users.

David Rose in the Mail on Sunday

David Rose of the UK Mail on Sunday is working on a comprehensive expose of this issue [link].

Here are the comments that I provided to David Rose, some of which were included in his article:

Here is what I think the broader implications are. Following ClimateGate, I made a public plea for greater transparency in climate data sets, including documentation. In the U.S., John Bates has led the charge in developing these data standards and implementing them. So it is very disturbing to see the institution that is the main U.S. custodian of climate data treat this issue so cavalierly, violating its own policy. The other concern that I raised following ClimateGate was overconfidence and inadequate assessments of uncertainty. Large adjustments to the raw data, and substantial changes in successive data set versions, imply substantial uncertainties. The magnitude of these uncertainties influences how we interpret observed temperature trends, 'warmest year' claims, and how we interpret differences between observations and climate model simulations. I also raised concerns about bias; here we apparently see Tom Karl's thumb on the scale in terms of the methodologies and procedures used in this publication.

Apart from the above issues, how much difference do these issues make to our overall understanding of global temperature change? All of the global surface temperature data sets employ NOAA's GHCN land surface temperatures. The NASA GISS data set also employs the ERSST datasets for ocean surface temperatures. There are global surface temperature datasets, such as Berkeley Earth and HadCRUT that are relatively independent of the NOAA data sets, that agree qualitatively with the new NOAA data set. However, there remain large, unexplained regional discrepancies between the NOAA land surface temperatures and the raw data. Further, there are some very large uncertainties in ocean sea surface temperatures, even in recent decades. Efforts by the global numerical weather prediction centers to produce global reanalyses such as the European Copernicus effort is probably the best way forward for the most recent decades.

Regarding uncertainty, 'warmest year', etc. there is a good article in the WSJ: Change would be healthy at U.S. climate agencies (hockeyshtick has reproduced the full article).

I also found this recent essay in phys.org to be very germane: Certainty in complex scientific research an unachievable goal. Researchers do a good job of estimating the size of errors in measurements but underestimate chance of large errors.

Backstory

I have known John Bates for about 25 years, and he served on the Ph.D. committees of two of my graduate students. There is no one, anywhere, that is a greater champion for data integrity and transparency.

When I started Climate Etc., John was one of the few climate scientists that contacted me, sharing concerns about various ethical issues in our field.

Shortly after publication of K15, John and I began discussing our concerns about the paper. I encouraged him to come forward publicly with his concerns. Instead, he opted to try to work within the NOAA system to address the issues –to little effect. Upon his retirement from NOAA in November 2016, he decided to go public with his concerns.

He submitted an earlier, shorter version of this essay to the Washington Post, in response to the 13 December article (climate scientists frantically copying data). The WaPo rejected his op-ed, so he decided to publish at Climate Etc.

In the meantime, David Rose contacted me about a month ago, saying he would be in Atlanta covering a story about a person unjustly imprisoned [link]. He had an extra day in Atlanta, and wanted to get together. I told him I wasn't in Atlanta, but put him in contact with John Bates. David Rose and his editor were excited about what John had to say.

I have to wonder how this would have played out if we had issued a press release in the U.S., or if this story was given to pretty much any U.S. journalist working for the mainstream media. Under the Obama administration, I suspect that it would have been very difficult for this story to get any traction. Under the Trump administration, I have every confidence that this will be investigated (but still not sure how the MSM will react).

Well, it will be interesting to see how this story evolves, and most importantly, what policies can be put in place to prevent something like this from happening again.

I will have another post on this topic in a few days.

Being retired sure is liberating . . .

Moderation note: As with all guest posts, please keep your comments civil and relevant.

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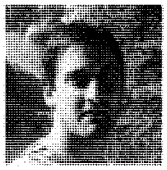
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beththeserf | February 4, 2017 at 6:34 pm | Reply

Incredible ain't it, non-archiving of critical evidence -?
Faith-based cli-sci- not for general exhibition.

> **Nick Stokes** | February 5, 2017 at 2:58 am | Reply
>
> *"Incredible ain't it, non-archiving of critical evidence -?"*
> And just not true. There is an extensive archive. Bates even linked to it. It is here.
>
> Bates complaints seem to be
> 1. The archiving wasn't complete until six months after the paper appeared
> 2. Data is in ascii format which is not "machine readable". Of course it is, it just requires a format statement.
>
> > **johnfpittman** | February 5, 2017 at 7:00 am |
> >
> > NIck you left out complaints:
> > 1. Karl made administrative decisions contrary to data integrity;
> > 2. Karl used 90% rather than 95% standard;
> > 3. The use of non standardized data set implied a greater uncertainty to the data that was not, could not be addressed;



Science, lies and video-taped experiments

Too many researchers make up or massage their data, says **Timothy D. Clark**. *Only stringent demands for proof can stop them.*

Late last month, a US physicist began a jail sentence for scientific fraud. Darin Kinion took funds for research on quantum computing but did not carry out the work he claimed; instead, he invented the data that the research supposedly produced.

Scientists like to think that such blatant dishonesty is rare, but I myself have witnessed several serious cases of scientific misconduct, from major data manipulation to outright fabrication. Most have gone unpunished — in fact, it has been disheartening to see the culprits lauded. It makes little sense for fraudsters to fabricate mediocre data. Their falsehoods generate outstanding stories, which result in high-profile publications and a disproportionately large chunk of the funding pie.

I have noticed a lesser-known motive for bad science in my field, experimental biology. As environmental change proceeds, there is great demand from the public and policymakers for simple stories that show the damage being done to wildlife. I occasionally meet scientists who argue that the questions we ask and the stories we tell are more important than the probity of our investigations: the end justifies the means, even if the means lead to data fabrication. That view is alarmingly misguided and has no place in science. The undeniable anthropogenic impacts on wildlife must be investigated with strict scientific rigour.

One reason some scientists can get away with questionable practices is that the scientific system is based on trust. The burden of proof is on those who suspect and report misconduct. Unless there is overwhelming evidence to the contrary, scientists are believed to have done what they say they did. If the community is serious about tackling misconduct, this must change. It is time to shift the burden of proof onto those who produce the results.

SCIENTISTS HAVE LITTLE EXCUSE NOT TO RECORD WHAT GOES ON IN LAB AND FIELD STUDIES.

In some fields, this proof is often implicit in how scientists collect and report data. Detailed evidence may be provided by the outputs of mostly autonomous equipment. Access to all the raw, non-manipulated data files — as increasingly demanded by journals and peers across disciplines — may be enough.

Science that relies on human observation of remote field work and trials that are difficult to replicate precisely — such as studies in the field of animal behaviour — needs a different approach. Simply, researchers should routinely film their experiments and present the footage to journal editors, reviewers and colleagues alongside their data and analyses. In some disciplines (such as ornithology), photo or audio files may provide better evidence than video.

If extreme athletes can use self-mounted cameras to record their wildest adventures during mountaintop blizzards, scientists have little excuse not to record what goes on in lab and field studies.

Yes, visual evidence can be faked, but a few simple safeguards should be enough to prevent that. Take a typical experiment in my field: using a tank of flowing water to expose fish to environmental perturbations and looking for shifts in behaviour. It is trivial to set up a camera, and equally simple to begin each recorded exposure with a note that details, for example, the trial number and treatment history of the organism. (Think of how film directors use clapper boards to keep records of the sequence of numerous takes.) This simple measure would make it much more difficult to fabricate data and 'assign' animals to desired treatment groups after the results are known.

My colleagues and I are currently using this approach to record studies of how coral-reef fish respond to dissolved carbon dioxide. There would also be benefits for other disciplines, including social-psychology studies based on direct observations.

Sharing visual evidence is straightforward. Video files can be compressed and transferred without excessive loss of resolution. Files can then be uploaded to free data repositories (such as figshare or Zenodo) before manuscripts are submitted for publication. Notably, the online supplementary material of most journals allows for 10–150 MB of storage to accommodate images and detailed descriptions of methodology.

There is more to this than preventing misconduct. Visual evidence can help reviewers (before and after publication) to spot problems that are not obvious from written descriptions and diagrams. Software could help to quantify behavioural features in recorded experiments and mitigate experimenter biases. Plus, scientists who know that their equipment and techniques will be on display will try harder to improve them.

The best way to implement these changes is for academic journals to start mandating visual (and audio) evidence to support a submitted paper. As far as I am aware, no journals routinely do this. Journals must also ensure that their stated requirements are adhered to.

Surveys suggest that I am not unusual in witnessing fraud: some 14% of scientists say that they have witnessed it, too. Although it would be simpler to turn a blind eye to this issue and move on, this situation inhibits so many aspects of scientific progress that I feel compelled to try to fix it. The added logistical difficulties of providing visual evidence are a small price to pay to tackle dishonesty and greatly reduce the number of irreproducible (and often poorly conducted) studies. Mandatory visual evidence will undoubtedly help to reconcile the tens of billions of dollars wasted on irreproducible research every year. In short, show us your science. ■

Timothy D. Clark *is a senior research fellow at the University of Tasmania and the Commonwealth Scientific and Industrial Research Organisation in Hobart, Australia.*
e-mail: timothy.clark.mail@gmail.com

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DOCUMENTS SUBMITTED BY COMMITTEE RANKING MEMBER
EDDIE BERNICE JOHNSON

The Honorable Lamar Smith
Chairman
Committee on Science, Space and Technology
U.S. House of Representatives
Washington, DC 20515

The Honorable Eddie Johnson
Ranking Member
Committee on Science, Space and Technology
U.S. House of Representatives
Washington, DC 20515

Dear Chairman Smith and Ranking Member Johnson:

The American Lung Association calls upon you to ensure the use of sound science to guide public policy and protect the health of all Americans.

Sound science saves lives and must be the foundation of decisions to adopt policy throughout the federal government. It is the backbone of lifesaving work at the U.S. Environmental Protection Agency, the Centers for Disease Control and Prevention, the Food and Drug Administration, the National Institutes of Health, and many other agencies that safeguard the health of the American people. Peer-reviewed research about health informs the public and forms the foundation of lifesaving policies.

We urge you to embrace the following principles to guide the use of science in federal policy development:

- Federal Agencies Must Continue to Make Decisions Based on Peer-Reviewed Science. Science is the bedrock of sound regulatory decision-making. Peer-reviewed research from private organizations, public charities, research universities, corporations, federal agencies, and others is critical to informing standard-setting and health-protective actions. Key information about each study, including funding and affiliations of researchers, ensures that each can be independently evaluated. Federal agencies, including EPA, FDA, CDC, and NIH, must have access to independent scientific information and advice to inform policy.

- Scientific Data Should Not Be Subject to Political Editing. For the sake of public health, science must be uncensored. For example, any political or economic-based suppression or editing of health science at EPA would directly contradict EPA's current scientific integrity policy, which prohibits "all EPA employees, including scientists, managers and other Agency leadership from suppressing, altering, or otherwise impeding the timely release of scientific findings or conclusions."

- Public Access to Science-Based Information is Vital. Access to accurate information enables members of the public to understand threats and take steps to protect themselves. Resources such as air quality data in specific communities and reports on the impacts of climate change can help members of the public protect themselves. The public not only has a right to know about potential risks to their health, but a *need* to know; for example, accurate air quality data allows a person with asthma to plan to spend more time indoors on a day with high levels of outdoor air pollution. The health of children, seniors and other vulnerable neighbors depends on ready access to information about factors that could impact their lungs.

- Patient Privacy Must Continue to Be Protected. Physicians and researchers have a clear legal and ethical obligation to maintain patient privacy. Researchers who evaluate the health impacts of air pollution, for example, must collect sensitive data from participants such as family medical history, geographic location, and personal medical history. Researchers who collect information about tobacco use can aggregate the data to determine patterns, but personal information about specific individuals must remain confidential. Scientists and institutions build in systems to protect this information while still maintaining open access to the collective data. The studies themselves are peer-reviewed and published in transparent processes. However, no way exists to protect patient privacy if the raw patient data are released. The federal government must continue to protect patient privacy by ensuring that patients' sensitive information is never made public – but that does not negate the use of such data to inform policy.
- Public Funding of Science is Essential. The federal government must continue to fund core scientific efforts to ensure that public health is protected, as the Clean Air Act and Tobacco Control Act require. Those core efforts must include research investigating the health impacts of air pollution and climate change; the public health impact of tobacco products; epidemiological surveillance data regarding the prevalence and severity of diseases such as asthma, lung cancer, influenza, COPD and pneumonia; research into effective measures to reduce pollution and prevent and treat diseases; and data collection and evaluation of air quality and pollution levels in communities across the nation. These funds must include funding to state and local governments and tribes to collect air quality data affecting their residents and to develop plans to clean up the pollution to protect their health, as well as funding for state and local departments of health to monitor and implement public health interventions. The Federal government must also continue to fund research at NIH and CDC that will continue to lead to life-saving breakthroughs for lung health.

Politics must not be allowed to undermine the integrity of, or access to, scientific data needed to protect public health. The American people depend on our leaders to maintain public access to scientific data and enforce the nation's public health laws based on accurate science. Censoring science will have devastating impacts on the health of the communities our organizations serve. We urge you to embrace the above listed principles to protect the health of all Americans.

Sincerely,



Harold P. Wimmer
National President and CEO

Cc: Committee on Science, Space and Technology

American Thoracic Society

American Journal of Respiratory and Critical Care Medicine | American Journal of Respiratory Cell and Molecular Biology | Annals of the American Thoracic Society

Written Comments of the American Thoracic Society
Submitted to the House Science Committee
For the hearing: "Make EPA Great Again"
Tuesday, February 7, 2017

The American Thoracic Society (ATS) is pleased to submit the following comments to the House Science Committee hearing: "Making EPA Great Again". By the title of this hearing, we have high expectations for the bold ideas that will be discussed by the committee to improve the ability of the US Environmental Protection Agency (EPA) to protect and improve our nation's air and water quality. However, previous actions taken by this Congress raise concerns that this hearing, and subsequent legislative activity taken by the committee, will only serve the interests of polluters, and will in fact seek to weaken, delay, and deter EPA's authority to protect our nation's children and adults from harmful health effects of air and water pollution.

As background, the ATS is a medical professional organization with over 16,000 members dedicated to prevention, detection, treatment, cure and research of pulmonary disease, critical care illness and sleep disordered breathing. Our members include physicians and scientists who are world-renowned experts on the health effects of air pollution. Our peer-reviewed journals publish cutting-edge science that examines effects of exposure to air pollution on the heart, lung, and human survival. Most importantly, our members treat patients with respiratory conditions like asthma and COPD – who are among most vulnerable to harm caused by air pollution.

Our society of over 16,000 medical professionals recognizes and accepts medical findings once they are supported by multiple studies of different designs, conducted in different settings, and published in reputable peer-reviewed journals. We know that **air pollution is harmful for human health because this finding is supported by decades of research**, consisting of thousands of peer-reviewed scientific studies conducted around the world.

We agree that we live in a society that is increasingly technology-based, and that a clear process for judging and interpreting scientific findings to inform our nation's health policies, is essential. But as indicated by the title of previous legislation considered by this committee, the "Secret Science Reform Act", many members of Congress appear to have a fundamental misunderstanding of how science works and progresses. This misunderstanding severely undermines the ability of Congress to govern this issue wisely and effectively. The aim of this submission is to help clarify the nature of sound science, such as that funded and employed by the US EPA, and to make constructive recommendations to make the science relied upon by the US EPA even stronger.

American Science is Conducted in an Open Process
The phrase "secret science" bears no semblance to reality. To achieve quality and transparency, any science conducted or published in the US requires three levels of external scrutiny before results are considered part of the evidence base, as follows:

External Review Step 1: Study Design Peer Review – The first level of review occurs when an investigator applies for funding, be it public funding or private funding. The investigator's proposal is subjected to peer review, which asks basic questions like:

- Is the question the investigator is seeking to answer scientifically valid?
- Will the answer provide useful knowledge?
- Does the investigator have the research infrastructure and scientific expertise to actually conduct the study?
- Are the methods the investigator is using actually capable of answering the question?
- Are the study sample sizes large enough of provide statistically reliable answers?

And probably most importantly:

- How does the proposed study, compared to other proposed studies, give us the best opportunity to advance our scientific understanding of the world?

Who asks these questions and reviews the answers? Panels of investigators who have specific expertise in that relevant field of study provide peer review. In the case of EPA, this review comes from independent, balanced expert review panels to ensure a broad range of subject matter expertise. . Why is having expertise important? Because experts know the field, so they can best assess whether the proposed study adds significantly to the field, uses the best research tools to answer the questions, and is well suited to answer important questions to advance our understanding of the world.

Simply said, the pre-review is comprehensive, competitive, and, especially in the case of federally funded research such as by the EPA, conducted in a transparent process. And this is only Step 1.

If the study involves human participants, there is another level of scrutiny to ensure the safety and well-being of the study participants. Scientists must submit their research proposal to an institutional review board (IRB), which is an independent ethics committee consisting of scientific professionals and community members to ensure that any risks to humans of the research are outweighed by the expected human health benefits of the research project. The IRB asks questions like:

- What are the benefits and/or risks to the volunteers?
- Does the benefit of the study outweigh the risk to volunteers?
- How should the risk to volunteers be communicated?

- Is there appropriate monitoring during the study to ensure participants' safety?
- Will the privacy of the participants' medical records be maintained?

All scientific studies must protect the privacy of human subjects and their families to ensure that the information gained in the study is not somehow used against them (e.g., by employers, health insurance companies selling policies, or by individuals via social media, for example).

External Review Step 2: Publication Peer Review – the next step taken in peer-reviewed science follows when the results of the study are submitted for publication in scientific or academic journals. In many ways, Step 2 is a quality check on Step 1, including:

- Did you execute the research project as you initially proposed?
- Did you follow and record at the data end points that you collected?
- Did you apply the correct statistical tools to analyze your data?
- Are the conclusions from your data justified?
- Are there alternative conclusions from your data?
- How conld have this study been done better?

These are all examples of questions that are applied in the publication peer view process. For any study published in the peer-reviewed literature, scientists are typically asked to return to their data and provide additional analyses during the peer review process to address reviewer concerns and to convince them that the findings are robust. The publication peer review process is also conducted by experts who know the field and are best able to judge the scientific methods of the study.

External Review Step 3: Broad Community Review – After a study has been completed and published, its finding are then subject to review by the entire scientific community and the public. People who disagree with a research finding can publically comment on it, point out its potential weaknesses and present alternative interpretations of the study findings, and submit letters of disagreement for publication and response in the journal. Further, researchers from other institutions frequently conduct subsequent independent studies that either validate or refute the conclusions of fellow researchers. In this way, scientists compete with one another to advance our understanding of the world. In the practice of medicine and puhlic health, a new scientific finding is generally not accepted as truth until it has been replicated in multiple studies.

We hope these comments help the committee better understand the multiple requirements for peer review that ensure transparency in the scientific process. It is this scientific process that drives EPA's science program, and informs EPA's approach of comprehensively reviewing the peer-reviewed, published scientific studies when setting standards to protect human health.

Beyond the multi-stage peer review process that insures the quality and transparency of the scientific process in the US, it is also important for members of the House Science Committee to consider the criteria that the medical community (and Federal agencies such as the Institute of Medicine and EPA) uses to weigh the evidence from multiple studies.

Consistency Across Studies: rarely does one study or one finding alone radically change our understanding of the world. Multiple independent studies are required to test the consistency and coherence of the results across studies. For example, the National Ambient Air Quality Standards for pollutants like ozone and particulate matter pollution are based on hundreds of studies. Each study adds to the overall understanding and assessment of the health effects of each pollutant.

Consistency Across Study Designs: Scientists use a wide range of scientific tools, approaches, and disciplines to advance our medical knowledge, with each enhancing, refining, and occasionally challenging the findings of sister scientific methods. In some cases, a scientific finding found using one type of study design (e.g. an observational questionnaire study) may not be replicated in a different study design, such as a randomized controlled trial, thus challenging the conclusion. The evidence used by the EPA to reach conclusions about the adverse health effects of ozone and particulate matter pollution are based on studies using a wide range of research approaches including: epidemiology studies, toxicology studies, animal exposure studies, human challenge studies, case control studies, and natural studies. Consistency of findings across multiple study designs substantially adds to the scientific plausibility of tbese findings.

Secret Science Reform Act
The ATS opposes legislation that the committee has previously considered, like the Secret Science Reform Act, that are not intended to improve the scientific process, but rather to suppress key studies from EPA's consideration, create a mandate on researchers to share information beyond what is already required in the peer review process, and to intimidate the research community. Measures such as these may also make it more challenging to conduct medical and environmental health research, because potential study participants will more likely refuse to participate when their private medical records are no longer secure, and might be made public in the name of transparency. It is our hope that today's hearing will not be used as an avenue to discuss the Secret Science bill or other obstructionist legislation, but rather to explore ways to further improve the already excellent science programs at EPA.
In the spirit of the hearing title, "Making EPA Great Again", the ATS offers the following concrete steps that Congress can take to improve operations of the EPA;

Expand the EPA Science Program
The EPA science program has funded high quality and relevant research that provides immediate benefit to the American tax payer by optimizing our nation's environmental protection efforts to focus only on the most health-relevant compounds.

Research on the health effects of air pollution, in particular ozone and particulate matter pollution, have shown that reducing these pollutants provides immediate health benefits that far exceed implementation costs. However, we still do not know which types of particles are the most health damaging and, therefore, which should he the most efficient focus of any risk mitigation measures. Additional funding should therefore be provided to further our understanding of the effects specific environmental exposures on human health, and to thereby optimize approaches to reduce human health effects of pollution.

Expand EPA's Monitoring Network
EPA's air pollution network is concentrated in large urban areas and at sites nearby expected sources of air pollution (major roads, power plants, industry facilities etc.). However, the air pollution levels in many small and rural communities are not monitored at all. These communities are also exposed to air pollution, including unique sources that do not affect urban communities, and they deserve the same level of public information and protection the EPA offers larger communities. We recommend Congress provide the funds needed to expand EPA's monitoring network to better protect both urban and rural populations.

Expand Use of New Monitoring Technology
Advances in satellite and personal monitoring technologies have the potential to both enhance and expand EPA's monitoring capability. Technological advancement has significantly lowered cost and improved the capability and reliability of small personal monitors that can be easily worn by an individual. These personal monitors have been used in many research studies addressing environmental exposures. EPA should take the next step in sponsoring research to better understand how to validate and incorporate data from personal devices into its air monitoring network, and to help doctors protect their most vulnerable patients from the health effects of pollution.

Similarly, environmental satellite data have advanced significantly, and can allow EPA to remotely collect data on air pollution exposure levels, on both a local and global scale, including areas that are left out of the EPA's ground monitoring network. Additional EPA research funding is needed to understand the potential of personal and satellite monitoring, and to develop protocols to validate and standardize data collected with different monitoring technologies into EPA's regulatory framework.

We urge the Congress to provide the US EPA with the funds needed to explore and incorporate these new technologies into their science and regulatory programs, so as to ultimately make their surveillance efforts and regulatory actions more focused and efficient in protecting the health of American children and adults.

ADAO
Asbestos Disease Awareness Organization™
Voice of the Victims

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February 7, 2017

The Honorable Lamar Smith
Chairman, Science, Space, and Technology Committee
U.S. House of Representatives
Via Fax: 202-225-8628

The Honorable Eddie Johnson
Ranking Member, Science, Space, and Technology Committee
U.S. House of Representatives
Via Fax: (202) 225-3895

Dear Chairman Smith and Ranking Member Johnson,

On behalf of the Asbestos Disease Awareness Organization (ADAO), the largest independent nonprofit dedicated to preventing asbestos exposure to eliminate deadly asbestos-caused diseases we are deeply concerned about today's hearing entitled "Making EPA Great Again."

Instead of a hearing to discuss the dismantling the U.S. Environmental Protection Agency (EPA), you should be discussing ways to increase the agency's funding and staffing to protect public health and the environment.

Since 2004, ADAO has been working with the White House, Congress, and Agencies on prevention and policy to end the asbestos man-made disaster. Recently, we worked with the House and Senate to pass TSCA reform legislation, which President Obama signed into law.

I personally know the devastation of asbestos., as mesothelioma, an asbestos cancer, claimed my husband's life. Each year, 15,000 Americans die from preventable diseases caused from thi known carcinogen, yet asbestos imports continue. USGS reported that in 2015, asbestos consumption in the United States was estimated ro be about 360 tons. Even more alarming, the chloralkali industry accounted for an estimated 90% of U.S. asbestos consumption.

In addition, we urge you to stand strong against bills such as H.R.861, a bill to terminate the Environmental Protection Agency, introduced by Representative Matt Gaetz.

American deserve to have air, water, and soil free from toxins and look to you and your committee to ensure the EPA can protect public health and our environment today and the future

Sincerely,

Linda Reinstein, President/CEO, Asbestos Disease Awareness Organization (ADAO)

Asbestos Disease Awareness Organization is a registered 501(c) (3) nonprofit organization
"United for Asbestos Disease Awareness, Education, Advocacy, and Community Support"
1525 Aviation Boulevard, Suite 318 · Redondo Beach · California · 90278 · (310) 251-7477
www.AsbestosDiseaseAwareness.org

The Intercept_

REPUBLICANS ARE USING BIG TOBACCO'S SECRET SCIENCE PLAYBOOK TO GUT HEALTH RULES



Sharon Lerner

February 5 2017, 9:03 a.m.

MUCH OF THE COUNTRY has been watching in horror as Donald Trump has made good on his promises to eviscerate the Environmental Protection Agency — delaying 30 regulations, severely limiting the information staffers can release, and installing Scott Pruitt as the agency's administrator to destroy the agency from within. But even those keeping their eyes on the EPA may have missed a quieter attack on environmental protections now being launched in Congress.

On Tuesday, the House Committee on Science, Space, and Technology is expected to hold a hearing on a bill to undermine health regulations that is based on a strategy cooked up by tobacco industry strategists more than two decades ago. At what Republicans on the committee have dubbed the "Making EPA Great Again" hearing, lawmakers are likely to discuss the Secret Science Reform Act, a bill that would limit the EPA to using only data that can be replicated or made available for "independent analysis."

The proposal may sound reasonable enough at first. But because health research often contains confidential personal information that is illegal to share, the bill would prevent the EPA from using many of the best scientific studies. It would also prohibit using studies of one-time events, such as the Gulf oil spill or the effect of a partial ban of chlorpyrifos on children, which fueled the EPA's decision to eliminate all agricultural uses of the pesticide, because these events — and thus the studies of them — can't be repeated. Although it is nominally about transparency, the bill leaves intact protections that allow industry to keep much of its own inner workings and skewed research secret from the public, while delegitimizing studies done by researchers with no vested interest in their outcome.

The top-billed witness scheduled to provide testimony at the House hearing on Tuesday is a lawyer named Jeffrey Holmstead, who has has worked to block the EPA's efforts to limit mercury pollution while representing coal companies including Duke Energy, Progress Energy, and Southern Company. Meanwhile, Lamar Smith, the Texas Republican chair of the House Science Committee who has been zealously promoting the"secret science" bill, is also in the pocket of the energy companies. Though he's also received funding from Koch Industries and iHeartMedia (formerly Clear Channel Communications), Smith's biggest contributors are oil and gas companies, according to the Center for Responsive Politics. Also testifying on Tuesday will be Kimberly Smith of the American Chemistry Council, the chemical industry trade group.

This bald industry bid to subvert public health-based regulations that can cut into profit isn't new. What's new is that this upside-down environmental attack, in which those who benefit directly from polluting industries are policing the independent scientists who can show the harms of their products, could now succeed. Although the House passed the secret science bill in 2014 and 2015, it never made it to the Senate floor. After it passed the House in 2015, Barbara Boxer called the bill "insane," Bernie Sanders called it "laughable," and President Obama promised to veto it. This time, it's not a joke. With a Republican majority in both houses and Trump in the White House, the secret science act could easily become law.



Graphic: The Intercept

The small group of lawyers and PR strategists orchestrating the secret science effort are closely tied to those attacking the EPA from within. All have connections to either big tobacco, oil, or both — and almost all have been affiliated with a small, right-wing group called the Energy & Environment Legal Institute. It's interesting that E&E should fixate on transparency since the group has gone to great lengths to conceal its donors. Nevertheless, public records document some of the group's ties to big coal companies, including the now bankrupt Alpha Natural Resources, Peabody Coal, and Arch Coal. E&E senior policy fellow Steve Milloy, a former tobacco industry attorney, has perhaps written the most — at least publicly — about the secret science strategy, both in an ebook and for Steve Bannon's Breitbart News. Milloy calls Myron Ebell, who oversaw Trump's EPA transition team, his "friend and hero." In the late 1990s, Milloy and Ebell were both members of the American Petroleum Institute's Global Climate Science Communications Team,

which laid out the oil industry's strategy to undermine the science of global warming. Meanwhile, three of Milloy's colleagues from E&E are also members of the EPA landing team. Among them are David Schnare, E&E's general counsel, who is perhaps best known for harassing Michael Mann and other environmental scientists with FOIA requests, and Amy Oliver Cooke, an energy industry think tanker who created MILF, Mothers In Love with Fracking.

Amy Oliver Cooke describes her love for fracking.

Two other E&E associates have been wrapped up in the secret science strategy for years. The first is Christopher Horner, a senior fellow at both E&E and the Competitive Enterprise Institute, who is also a member of Trump's EPA landing team. Back in the 1990s, Horner worked for Bracewell LLP, the law firm (formerly known as Bracewell & Giuliani) supplying the top witness at Tuesday's hearing. The dawning awareness of the dangers of second-hand smoke was putting tobacco companies on the defensive, including Horner's client, the R.J. Reynolds Tobacco Company. In a 1996 memo, which seems to be the earliest known reference to the secret science strategy, Horner laid out a plan to fight back.

From: Christopher Horner
To: Hyde, Timothy N.; Tompson, Randy
CC:
BCC:
Subject: Federal Agency Science
Date: 12/23/1996 1:56:01 PM

Attachments:

Gentlemen: The following is the document we discussed. Have a happy holiday. CCH
M E M O R A N D U M

TO: Mr. Tim Hyde
Mr. Randy Johnson
RJ Reynolds Tobacco Company

FROM: Mr. Christopher C. Horner
Bracewell & Patterson, L.L.P.

DATE: December 23, 1996

RE: Background and Proposed Program to Address Federal Agency Science

Per our earlier conversations, the following sets forth what needs to be done to reform agency science, focusing on the need based upon your interests, and how you are positioned to take a behind the scenes leadership position. It provides an overview of the issues relevant to this goal, and details a program taking advantage of the increasingly flagrant way regulators have perverted the scientific process, hiding behind a wall of selected scientists to essentially cow industry and Congress into accepting fringe scientific conclusions.

Summary

We propose creating, beginning with congressional oversight and a goal of enacting legislation, required review procedures which EPA and other federal agencies must follow in developing "extra-judicial" documents (i.e., those documents produced as guidance, science or other government products issued by regulatory agencies which are not necessarily at time of publication ripe for judicial review). This is important to your organization because, at some point in the near future, EPA will most likely be ordered to re-examine ETS. The only way to do

http://legacy.library.ucsf.edu/tid/rzb77a00/pdf documents.library.ucsf.edu/tobacco/docs/jfww0019

[redacted]

Horner to RJR Reynolds 1996 Bracewell Giuliani

"We propose creating, beginning with congressional oversight and a goal of enacting legislation, required review procedures which EPA and other federal agencies must follow," Horner wrote in his memo. "This is important to your organization because, at some point in the near future, EPA will most likely be ordered to re-examine ETS [environmental tobacco smoke]." Horner's plan? "To construct explicit procedural hurdles the agency must follow in issuing scientific reports. Because there is virtually no chance of affecting change on this issue if the focus is ETS."

Horner already saw that the secret science approach could subvert far more than the imminent regulations based on the science about second-hand smoke. "Our approach is one of addressing process as opposed to scientific substance, and global applicability to industry rather than focusing on any single industrial sector," he wrote, going on to explain how the strategy could be used to interfere with the EPA's efforts to address mercury emissions, hazardous waste, and dioxins as well as restrictions on air pollution.

The Attack on Air Pollution Protections

By 1998, Powell Tate, a lobbying firm that represented R.J. Reynolds, had helped organize a secret science working group to look at questions of "data access," according to one internal memo. The memo clarified that its intention was to "focus public opinion on the importance of requiring the disclosure of tax-payer funded analytical data."

Though it was the brainchild of tobacco strategists, the energy industry soon followed Horner's advice and adopted the secret science approach as a way to hamper air quality improvement efforts. In the 1990s, the EPA began efforts to reduce the amount of tiny

particles in the air, a kind of pollution known as PM 2.5, that are produced by combustion from power plants, cars, and manufacturing. The clearest evidence of the need to limit such particles came from the "Six Cities" study, in which a team of Harvard researchers clearly tied higher levels of PM 2.5 particles to increased mortality, as well as lung cancer, asthma, and sudden infant death syndrome.

While the new limits were designed to save lives — preventing 15,000 premature deaths annually, according to EPA projections — the rules would also increase costs in some sectors by, for instance, making energy companies install pollution equipment. In response, a group funded by the Koch brothers rose up to challenge the EPA and the scientists on the grounds that scientists were hiding their data from the public. Citizens for a Sound Economy, a forerunner of the Koch brothers' current Freedom Works, demanded that the Harvard researchers provide their original data so an "independent" scientist could analyze it.

At first the researchers refused to share the data, which they had collected from individuals with the promise that their health information would remain confidential. Eventually, after an elaborate and expensive pressure campaign, the Six Cities researchers agreed to allow their data to be reanalyzed by two separate teams of researchers. Both confirmed the group's findings that rates of PM 2.5 were correlated with increased mortality.

The EPA went on to institute the changes. And scientists throughout the world have since come to recognize the dangers posed by small particle air pollution, which accounted for "over 2.1 million premature deaths and 52 million years of healthy life lost in 2010," according to the 2010 Global Burden of Disease report. The report drew on research by more than 450 experts from around the world and was led by the Institute of Health Metrics and Evaluation at the University of Washington; the World Health Organization; the University of Queensland, Australia; Johns Hopkins University; and Harvard University.

8

Despite the scientific consensus, a small group of extremists has continued to fixate on the idea that the science on the dangers of air pollution is somehow a sham. Even more disturbingly, this small extreme group now holds sway in key parts of the U.S. government. Not least among them is Rep. Lamar Smith, who in 2013 subpoenaed the EPA in yet another effort to obtain the data from the Six Cities study.

In an op-ed that ran in the Wall Street Journal shortly afterward, Smith noted that "the data in question have not been subjected to scrutiny and analysis by independent scientists." Smith pressed his point in a House Science Committee hearing a few days later, insisting that independent scientists were being denied access to the air pollution data. When Democrat Donna Edwards pressed Smith about who these scientists were, he mentioned the name Jim Enstrom.

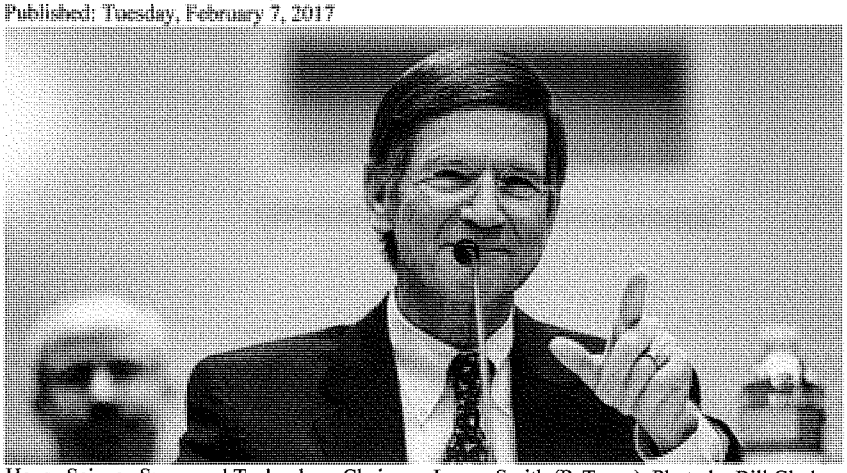
Enstrom, you may not be surprised to learn, has been a research fellow at E&E and has received money from the Council for Tobacco Research, the Tobacco Institute, Philip Morris, and R.J. Reynolds. In part because he didn't disclose his tobacco industry ties in a study he did on the connection between second-hand smoke and mortality (which he found to be inconclusive), he was widely criticized by the scientific community, including the American Cancer Society, and was subsequently dismissed from UCLA.

SCIENCE

'Whistleblower' says protocol was breached but no data fraud

Scott Waldman, E&E News reporter

Published: Tuesday, February 7, 2017



House Science Space and Technology Chairman Lamar Smith (R-Texas). Photo by Bill Clark, courtesy of AP Images.

The federal climate scientist hailed by conservatives as a whistleblower for allegedly revealing manipulated global warming data said yesterday he was actually calling out a former colleague for not properly following agency standards for research.

In an interview with E&E News yesterday, former National Oceanic and Atmospheric Administration principal scientist John Bates had a significantly more nuanced take on the controversy that has swirled since a top House Republican hailed his blog post as proof that the agency "played fast and loose" with temperature data to disprove the theory of a global warming "pause."

Bates accused former colleagues of rushing their research to publication, in defiance of agency protocol. He specified that he did not believe that they manipulated the data upon which the research relied in any way.

"The issue here is not an issue of tampering with data, but rather really of timing of a release of a paper that had not properly disclosed everything it was," he said.

Bates, who recently retired from NOAA's National Climatic Data Center, claimed in his post that the agency rushed research disproving the global warming pause to publish in *Science* magazine before the December 2015 Paris climate talks. Climate skeptics have called that proof of massive fraud among federal climate researchers and said it allowed world leaders to be "duped" into signing the Paris climate agreement to reduce carbon emissions from fossil fuel use.

Bates said the NOAA study relied on land data that were "experimental." Typically, NOAA officials can publish research that relies partially on experimental data, as long as the data are properly identified, especially if there is an urgent situation that requires something to go out quickly because it is related to human health, safety and the environment.

The publishing safeguards are important, he said, because they help protect federal research against lawsuits. Bates added that science suffers if its results cannot be reproduced.

Yesterday, the House Science, Space and Technology Committee portrayed Bates' allegations as a bombshell that required immediate investigation.



Former National Oceanic and Atmospheric Administration meteorologist John Bates says the agency broke its own rules for scientific integrity when it published a study debunking the "hiatus" in global warming. Photo courtesy of the American Geophysical Union.

Committee Chairman Lamar Smith (R-Texas) has issued subpoenas and has for nearly two years attempted to obtain scientists' emails involved in the global warming pause research. A Science Committee aide yesterday said Bates' revelation was evidence that NOAA needed further investigation because its own employees were identifying significant policy breaches.

The aide said the committee would again seek the emails of federal researchers, and if a formal request were ignored, another round of subpoenas could be issued or scientists might be forced to testify in front of the committee.

"I think the brushback that the committee received, and the chairman received consistently, about how science is capable of policing itself and doesn't need anyone outside asking questions, even when the science being discussed is paid for and performed by scientists paid for with the taxpayer's money and used to implement far-reaching federal policies or justify implementation of far-reaching federal policies, doesn't really work," the aide said.

'An incredibly bizarre claim'

Bates laid out his claims, which are largely technical and related to the sharing of data, on the blog run by Judith Curry, a climate scientist who has broken with many colleagues and called into question the actual extent of humanity's influence on the planet.

The report's authors, Bates wrote, put a "thumb on the scale — in the documentation, scientific choices, and release of datasets — in an effort to discredit the notion of a global warming hiatus and rush to time the publication of the paper to influence national and international deliberations on climate policy."

The NOAA administrator under former President Obama, Kathryn Sullivan, refused to turn over the emails because she said doing so could chill the scientific process by making it harder for researchers to communicate openly while they were actively engaged in research. Smith's committee threatened her with criminal charges. The issue is expected to become part of the committee's hearing today into the use of scientific research in crafting federal regulations.

Yesterday, a NOAA spokesman did not directly address the specific allegations, other than to say that they are currently under review.

"NOAA is charged with providing peer-reviewed data to the American public and stands behind its world-class scientists," said the spokesman, who declined to be named. "NOAA takes seriously any allegation that its internal processes have not been followed and will review the matter appropriately."

Whether the research was published to influence the Paris climate talks is a moot point, said Andrew Light, a senior member of the State Department's climate talks negotiating team in 2015. He said the talks had already been underway for about four years when the paper was published and that 188 nations were relying on a tremendous amount of research to support their goal of reducing humans' carbon emissions to slow the warming of the planet. They had also already crafted proposed reductions by the time the research was published, he said.

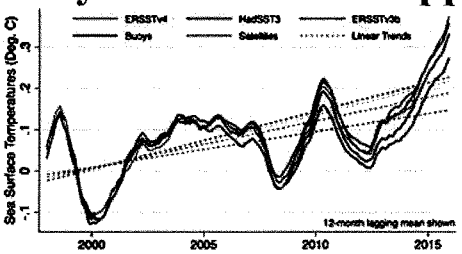
"I never heard it discussed once, let alone this one NOAA report, discussed in Paris, the run-up to Paris or anything after Paris, so this is really just an incredibly bizarre claim," Light said.

Bates: Be careful of bias

For many years, climate scientists were puzzled by an apparent plateau in global temperature rise from 1998 to 2012 as ocean temperatures stayed consistent. The 2015 research paper addressed the issue when it found there was no pause because the method to collect ocean temperatures was flawed.

Since then, multiple independent studies have confirmed NOAA's findings, including one published last month in *Science Advances*.

Buoys and satellites support NOAA record



[+] A study earlier this year using data from buoys, satellites and Argo floats backs up a challenge of the so-called global warming pause by NOAA. Graph courtesy of the University of York Department of Chemistry.

That study replicated NOAA's findings by accounting for different methods of temperature collection over time. For instance, data collected in the engine rooms of ships show slightly elevated levels of warming compared with those collected by buoys. When researchers accounted for that discrepancy, the so-called global warming pause disappears, researchers found.

The American Geophysical Union, which represents thousands of scientists who study climate, pointed out that the results of the 2015 study had been discussed in peer-reviewed journals and that multiple studies had independently backed up the findings.

The reports do not change the fundamental understanding of climate change science, AGU President Eric Davidson wrote in his blog yesterday.

"These types of statements by policymakers that attempt to take one study/dispute and blow it out of proportion are both unhelpful and misleading," he wrote. "We will be working with the science committee to demonstrate the scientific consensus on climate change and to encourage them not to interfere with the scientific process."

Yesterday, Bates said he was contacted by the Science Committee for the first time only after the story broke. He said he has not communicated with anyone there before and was not a whistleblower for the committee previously but that he expected to be invited to Washington to testify at a future hearing.

He said he would accept such an invitation, but cautioned scientists against advocating policy.

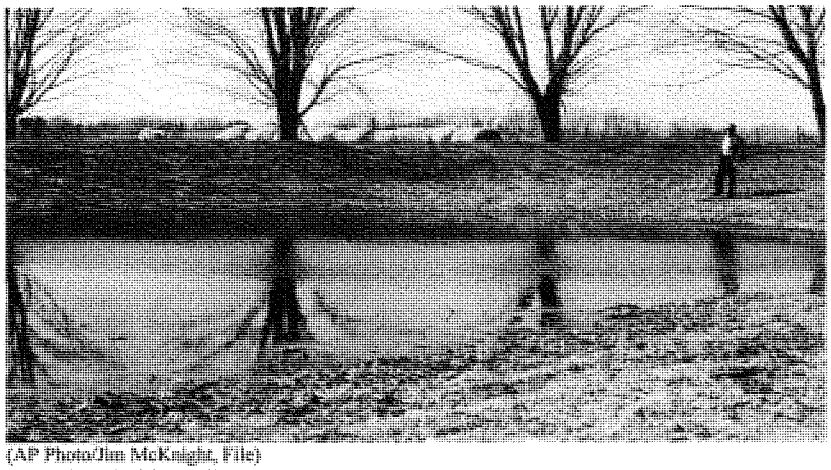
"You really have to provide the most objective view and let the policymakers decide from their role," Bates said. "I'm getting much more wary of scientists growing into too much advocacy. I think there is certainly a role there, and yet people have to really examine themselves for their own bias and be careful about that."

This story also appears in E&E Daily.

Twitter: **@scottpwaldman** Email: **swaldman@eenews.net**

The EPA Is Already Great

The effort on Capitol Hill to undermine the EPA is at once a national tragedy and deeply puzzling.



(AP Photo/Jim McKnight, File)

By David Dyjack | Contributor

Feb. 7, 2017, at 12:00 p.m.

This is not the time to limit the Environmental Protection Agency's scope or throttle back its efforts. Americans should empower the agency to do more on behalf of our families and our standard of living. The agency's successes are worthy of celebration; the cost of retreat is staggering.

Did you know we could save an estimated 5.5 million IQ points by permanently removing lead paint and dust from homes? The economic return on this investment is estimated at $68 billion. For every $1 invested in lead reduction, there is up to an estimated $221 return on that dollar.

Were you aware that the EPA-administered Clean Air Act is a public health success story? In its first 20 years, more than 200,000 premature deaths and 18 million cases of respiratory illness in children were prevented. Estimates suggest that total benefits of the Clean Air Act amount to more than 40 times the costs of regulation.

Americans enjoy some of the safest and most reliable drinking water in the world because of EPA's Safe Drinking Water Act. More than 90 percent of water customers enjoy drinking water that meets all health standards all the time. Alternately, Flint, Michigan, is a case study, which demonstrates why the absence of informed, credentialed environmental health professionals in decisions regarding water safety can lead to disaster.

The EPA Clean Water Act regulates outdoor water quality in rivers and streams. It has been a remarkable success since 1972 and is arguably the crown jewel of the EPA, resulting in water quality improvements and preventing water pollution where people boat, swim and fish.

These opportunities and successes showcase the essence of a government agency that is essential to life in our society. The current effort on Capitol Hill to undermine the EPA is at once a national tragedy and deeply puzzling.

We understand a comically awful, recycled piece of legislation, the Secret Science Reform Act, is about to take center stage once again. The proposed law is based on a bogus proposition, that somehow the EPA is exploiting secret studies to support its rule-making, and is intended to undermine the EPA.

In full disclosure, this is personal for me. The proposed law's intent is to publicize the medical records of people involved in health studies. You read that correctly. If you participate in government-funded research, your private medical information could become fodder for public consumption.

For the record, I am part of a National Cancer Institute-sponsored longitudinal study, which examines the relationship between lifestyle and disease. Do I want my 100-plus page lifestyle questionnaire made public? Of course not. So the net effect of the Secret Science Act will be that fewer people will elect to participate in research, and the opportunity to discover innovative solutions to today's problems will be diminished. This is a page taken directly from the tobacco industry's playbook and is shameful.

Environmental health issues are profoundly local. These issues flow from your kitchen faucet, are found in the meat and produce sections of your neighborhood grocery store, present themselves at your local elementary school and appear in your beaches and streams. The nation's 90,000 environmental scientists are generally modestly paid, humble civil servants, who live in local communities throughout our great country. They rely on EPA science and guidance documents in their efforts to ensure the health, safety and security of you and your family.

The EPA does not, and has not, suppressed or concealed scientific information in its rule-making. Congress should fully fund the EPA and reject sham legislation. The EPA is already great. Let's keep it that way.

Tags: EPA, environment, water, pollution, science, public health, Congress



David Dyjack Contributor

David Dyjack is executive director of the National Environmental Health Association.

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On the definition and identifiability of the alleged "hiatus" in global warming

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Stephan Lewandowsky[1,2] James S. Risbey[3] & Naomi Oreskes[4]

Recent public debate and the scientific literature have frequently cited a "pause" or "hiatus" in global warming. Yet, multiple sources of evidence show that climate change continues unabated, raising questions about the status of the "hiatus". To examine whether the notion of a "hiatus" is justified by the available data, we first document that there are multiple definitions of the "hiatus" in the literature, with its presumed onset spanning a decade. For each of these definitions we compare the associated temperature trend against trends of equivalent length in the entire record of modern global warming. The analysis shows that the "hiatus" trends are encompassed within the overall distribution of observed trends. We next assess the magnitude and significance of all possible trends up to 25 years duration looking backwards from each year over the past 30 years. At every year during the past 30 years, the immediately preceding warming trend was always significant when 17 years (or more) were included in the calculation, alleged "hiatus" periods notwithstanding. If current definitions of the "pause" used in the literature are applied to the historical record, then the climate system "paused" for more than 1/3 of the period during which temperatures rose 0.6 K.

"There was no such thing as the Scientific Revolution, and this is a book about it."
—Steven Shapin, 1996, *The scientific revolution*. University of Chicago Press.

In the public sphere, the claim that global warming has "stopped" has long been a contrarian talking point[1,2]. After being confined to the media and internet blogs for some time, this contrarian framing eventually found entry into the scientific literature[3,4], which is now replete with articles that address a presumed recent "pause" or "hiatus" in global warming[5]. The "hiatus" also featured as an accepted fact in the latest assessment report of the IPCC[6]. Despite its widespread acceptance in the scientific community, there are reasons to be skeptical of the existence of the "hiatus"[5].

Recently, possible artifacts in the global surface temperature record have been noted which, when corrected, suggest that there is little evidence for a "hiatus" relative to the long-term trend used by the IPCC[7]. In addition, multiple other indicators such as ocean heat content point to continued warming[8–10].

In this article, we show that even putting aside possible artifacts in the temperature record, there is no substantive evidence for a "pause" or "hiatus" in warming. We suggest that the use of those terms is therefore inaccurate. Because this conclusion appears to contradict the IPCC's explicit endorsement of the "hiatus", it is important to differentiate between the different ways in which the term "pause" or "hiatus" has been motivated and used in the recent climatological literature.

[1]School of Experimental Psychology and Cabot Institute University of Bristol, 12A Priory Road, Bristol, BS8 1TU, United Kingdom. [2]School of Psychology, University of Western Australia, 35 Stirling Highway, Crawley, WA 6009 Australia. [3]CSIRO Oceans and Atmosphere, Box 1538, Hobart, Tasmania 7001 Australia. [4]Department of the History of Science, Science Center 371 Harvard University, 1 Oxford Street, Cambridge, Massachusetts 02138, USA. Correspondence and requests for materials should be addressed to S.L. (email: stephan.lewandowsky@bristol.ac.uk)

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Research on the "hiatus" has been couched within at least 4 distinct research questions: (1) Is there a "pause" or "hiatus" in warming? (2) Has warming slowed significantly compared to the long-term trend? (3) Has warming lagged behind model-derived expectations? (4) What are the physical mechanisms responsible for the "hiatus"? Here, we are exclusively concerned with the first question: Is there, or has there recently been, a "pause" or "hiatus" in warming? We focus on this question because it is ineluctably tied to the contrarian claim that global warming has "stopped", which has demonstrably affected the political and media landscape[3] as well as, arguably, the scientific community[4]. The question whether there is a "pause" in global warming can be readily tested: Standard dictionary definitions of the words "pause" or "hiatus" imply that a process has been suspended or interrupted. It follows that for the notion of a "hiatus" in global warming to be scientifically well-founded, there must either be a demonstrable and statistically-relevant absence of any trend in global mean surface temperature (GMST) during the time period that is considered relevant or, minimally, the observed trend must differ in a statistically identifiable way from the historical record.

Our focus on the question whether there is a "hiatus" or "pause" implies that we do not address two related issues: First, we are not concerned with the differences, if any, between climate model projections and observed GMST trends. We have addressed the issue whether or not warming has lagged behind model-derived expectations elsewhere[11], and this issue has no bearing on the existence of a "hiatus". Second, we are not concerned with the underlying physical processes that may explain fluctuations, whether positive or negative, in GMST. This is again a different question, which is interesting in its own right but has no bearing on the existence of a "hiatus".

We examine the status of the "hiatus" in three steps: First, we compile an inventory of operationalizations of the "hiatus" in the existing scientific literature and ask whether they converge on a consistent definition. Second, we ask whether the rate of temperature change during the "hiatus", as it is operationalized in the literature, differs meaningfully from the set of rates for equivalent trend lengths observed during the era of modern climate change. This comparison is essential because *any* trend will exhibit periods of statistical insignificance when the sample size (i.e., number of years considered) is small: The existence of the presumed "hiatus" thus cannot be ascertained without a historical comparison to other comparable trend durations at earlier times during which warming was consensually thought to be present. Finally, for the same reason, we ask whether the duration of periods in which there is no significant warming has changed during the presumed "hiatus" relative to the rest of the modern period.

Results

There is no agreed "hiatus" period in the scientific literature. We catalogued a corpus of peer-reviewed articles published between 2009 and 2014 that specifically addressed the presumed "hiatus" in global warming. Table 1 shows that the term "hiatus" was used more than 550 times in this corpus, and the word "pause" in excess of 70 times.

Many articles assumed that the "hiatus" commenced around 1998, at which time temperature anomalies were considerably above the long-term trend. There is, however, considerable heterogeneity in published onset times, with the range spanning a decade (1993–2003). Similarly, there is considerable heterogeneity in the presumed duration of the "hiatus" across the same corpus of articles, with a range 10–20 (median 13 years, $m = 13.5$, $s = 2.86$). For each article, we took the duration to be the number of years since the assumed onset of the "hiatus" to the end of the period being analyzed. This constitutes a lower bound on the presumed duration of the "hiatus" as some authors may have presumed that the "hiatus" was ongoing at the time they published an article. Figure 1 shows the modern global temperature data together with a histogram of the distribution of presumed onset times of the "hiatus" derived from the corpus.

The heterogeneity in onset and duration raises the possibility that the use of the term "hiatus" departs from normal scientific practice, which strives to define phenomena on the basis of clear and generally accepted criteria. The heterogeneity may be explained by the supposition that authors defined the "hiatus" retrospectively, via an ad hoc analysis of the recent trend leading up to the time of writing, rather than on the basis of a priori criteria. This apparent lack of clear and a priori criteria must be of concern in the statistical environment in which the "hiatus" has unfolded, which is known to be sensitive to the particular choice of start and end points that define short-term trends and the comparison baseline[12].

The "hiatus" is an unexceptional fluctuation. If the definitions of the presumed hiatus are highly variable, with many different time periods proposed in the literature, how can we determine whether or not there is one? In order to answer this question, we compared the distribution of decadal warming trends during the "hiatus"—as defined by the articles in the corpus—against the distribution of all possible trends that have been observed during the period of modern global warming. The results are shown in Fig. 2, using three different onset dates for global warming.

The question of when, precisely, greenhouse-driven warming began to be observable against background natural variability is itself contested. An early review[13] that examined the literature back to 1824 finds that scientific concern about global warming arose as early as 1938. Every decade since then has seen increased scientific attention and concern[13], although no consensual onset date for global warming has been identified. Figure 2 therefore uses three different onset dates for the computation of all possible

Article	From	To	Trend	"Slow"	"Pause"	"Hiatus"	Focus	Data
Allan[49]	2000	2012		4	—	—	OM	H, C5, o
	"…energy is continuing to accumulate in the oceans, despite the apparent recent slower rates of global surface warming compared with the late twentieth century and with climate model simulations."							
Brown[59]	≃2001	2012		3	—	"1"	M	C5
	"A slowdown in the rate of warming in the early 21st century has increased interest in unforced decadal variability within the scientific community."							
Chen[51]	≃2001	≃2010		17	—	15	O	o
	"The latter part of the 20th century saw rapid global warming as more heat stayed near the surface. In the 21st century, surface warming slowed as more heat moved into deeper oceans."							
Clement[52]	2000	2013		3	5	"2"+8	O	o
	"A pause in global warming since 2000—a global warming 'hiatus'—has opened up new questions about natural and human activity-driven (anthropogenic) effects on global mean trends in surface temperature."							
Crowley[58]	1997 (2002)	2013	*	—	—	—	O	H, o
	"Stable global temperatures of the last 10–15 years have been a topic of considerable discussion."							
Drijfhout[53]	≃2001	≃2010		2	—	11	O	G, H, o
	"…a slowing of the warming in the 2000s, even though atmospheric greenhouse gas concentrations continued to increase. This hiatus in warming may have been exaggerated by sampling errors [Cowtan and Way, 2014], but a significant slowdown is evident."							
Easterling[54]	1998	2008		—	—	—	O	N, C3
	"Numerous websites, blogs and articles in the media have claimed that the climate is no longer warming, and is now cooling. Here we show that periods of no trend or even cooling of the globally averaged surface air temperature are found in the last 34 years of the observed record, and in climate model simulations of the 20th and 21st century forced with increasing greenhouse gases."							
England[15]	2001	2013		2	—	27	O	G, C5
	"Despite ongoing increases in atmospheric greenhouse gases, the Earth's global average surface air temperature has remained more or less steady since 2001."							
Estrada[55]	late 1990s	2012		15	1	—	O	G, H, o
	"The warming of the climate system is unequivocal as evidenced by an increase in global temperatures by 0.8C over the past century. However, the attribution of the observed warming to human activities remains less clear, particularly because of the apparent slow-down in warming since the late 1990s."							
Fyfe[56]	1993 (1998)	2012	* >1	2	—	"1"	M	H, C5
	"Recent observed global warming is significantly less than that simulated by climate models."							
Fyfe[57]	1993	2012	* >1	—	—	1	M	H, C5
Goddard[58]	≃2003	≃2013		2	—	"1"+7	O	o
	"The 'global warming hiatus'—the fact that globally averaged air temperatures have not increased as quickly in the past decade as they have in previous decades—is a hot topic, so to speak."							
Guemas[48]	2000	2010		26	7	3	O	o
	"Despite a sustained production of anthropogenic greenhouse gases, the Earth's mean near-surface temperature paused its rise during the 2000–2010 period."							
Haywood[60]	2002	2012		4	—	4	M	H, o
	"The slow-down in global warming over the last decade has lead to significant debate about whether the causes are of natural or anthropogenic origin."							
Hawkins[50]	1998	2012	* >1	12	"2"+12	—	O	H, o
	"The recent slowdown (or 'pause') in global surface temperature rise is a hot topic for climate scientists and the wider public."							
Held[61]	1993	2012	* >1	—	1	10	M	H, o
	"A global climate model that factors in the observed temperature of the surface ocean in the eastern equatorial Pacific offers an explanation for the recent hiatus in global warming."							
Huber[62]	≃1998	2012		3	—	"2"+2	OM	H, CW, C, o
	"Global mean surface warming over the past 15 years or so has been less than in earlier decades and than simulated by most climate models."							
Hunt[63]	1998	2010		—	—	22	O	H, o
	"Controversy continues to prevail concerning the reality of anthropogenically-induced climatic warming. One of the principal issues is the cause of the hiatus in the current global warming trend."							
Kamae[64]	≃1998	2012		5	2	9	O	G, C5
	"This global-warming hiatus is a period characterized by a pause in global SAT increase, despite a continued increase in radiative forcing…."							

Continued

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Article	From	To	Trend	"Slow"	"Pause"	"Hiatus"	Focus	Data
Kaufmann[45]	1998	2008		7	—	8	O	H, G, o
	"Given the widely noted increase in the warming effects of rising greenhouse gas concentrations, it has been unclear why global surface temperatures did not rise between 1998 and 2008."							
Kosaka[46]	2001	2012		2	—	28	M	H, o
	"Despite the continued increase in atmospheric greenhouse gas concentrations, the annual-mean global temperature has not risen in the twenty-first century, challenging the prevailing view that anthropogenic forcing causes climate warming."							
Lin[47]	"> decade"		NA	—	1	8	O	N, o
	"The recent global-warming hiatus is attributed to a La Niña-like decadal cooling phenomenon over the eastern tropical Pacific Ocean."							
Lovejoy[48]	1998	2013		"1" + 1	"10" ÷ 21	"1"	M	G, o
	"More troubling, the models over-estimated the post-1998 El Niño global temperatures: they did not anticipate the 'global slow-down'..., 'hiatus'..., or 'pause'..."							
Lu[49]	unspecified			2	—	"1" + 15	M	o
	"The global warming hiatus does not necessarily mean a hiatus in anthropogenic greenhouse gas forcing and forced climate change..."							
Macias[50]	2001	2013		1	—	"1" + 13	O	H
	"Global surface temperature has been increasing since the beginning of the 20th century but with a highly variable warming rate, and the alternation of rapid warming periods with 'hiatus' decades is a constant throughout the series."							
Maher[51]	2001	2013		1	—	"1" + 104	M	G, C
	"The latest generation of climate model simulations are used to investigate the occurrence of hiatus periods in global surface air temperature in the past and under two future warming scenarios."							
McGregor[52]	unspecified		NA	1	2	3	OM	G, o
Meehl[53]	2000	2009		—	—	34	M	o
	"There have been decades, such as 2000–2009, when the observed globally averaged surface-temperature time series shows little increase or even a slightly negative trend (a hiatus period)."							
Meehl[54]	2000	2009		1	—	79	OM	o
	"Globally averaged surface air temperatures in some decades show rapid increases (accelerated warming decades), and in other decades there is no warming trend (hiatus decades)."							
Meehl[55]	2000	2009		—	—	13	M	o
Meehl[56]	2000	2013		2	—	32	M	H, C5
	"The slowdown in the rate of global warming in the early 2000s is not evident in the multi-model ensemble average of traditional climate change projection simulations."							
Palmer[78]	unspecified		NA	—	9	—	M	C5
Ridley[57]	"post 2000"			2	"1"	"1" + 2	O	o
	"Understanding the cooling effect of recent volcanoes is of particular interest in the context of the post-2000 slowing of the rate of global warming."							
Risbey[11]	1998	2012		—	—	"1"	M	H, G, C5, CW
	"The differences between climate model forecasts and projections have come to prominence over interpretation of model simulations of recent temperature trends."							
Santer[25]	1998	2012		4	—	"3"	O	C5, o
	"Despite continued growth in atmospheric levels of greenhouse gases, global mean surface and tropospheric temperatures have shown slower warming since 1998 than previously."							
Schmidt[21]	1997	2013	*	2	—	—	M	G, CW, C5
	"Climate models projected stronger warming over the past 15 years than has been seen in observations."							
Seneviratne[58]	1997	2012		4	"1" + 5	5	O	H, o
	"Observational data show a continued increase of hot extremes over land during the so-called global warming hiatus. This tendency is greater for the most extreme events and thus more relevant for impacts than changes in global mean temperature."							
Stillmann[59]	1996	2010	* >1	1	—	"1" + 10	M	C5, o
	"The discrepancy between recent observed and simulated trends in global mean surface temperature has provoked a debate about possible causes and implications for future climate change projections."							
Smith[60]	≈1998	2012		11	—	—	OM	H, G, N, C5
	"...it is now clear that the rate of warming has slowed substantially over the past 15 years or so and the observations are very much at the lower end of model simulations."							
Solomon[93]	2000	2009		1	—	—	OM	H, G, N, o

Continued

Article	From	To	Trend	"Slow"	"Pause"	"Hiatus"	Focus	Data
Trenberth[58]	2000	2012		4	1	13	O	H, G, N, o
	"Global warming first became evident beyond the bounds of natural variability in the 1970s, but increases in global mean surface temperatures have stalled in the 2000s."							
Trenberth[59]	1999 (2000)	2012	*	2	6	11	O	G, N, o
	"Although the 2000s are by far the warmest decade on record, the rate of increase of global mean temperature since 2000 has slowed..."							
Watanabe[62]	2000	2009		4	--	27	OM	H, G, C3, C5
	"The rate of increase of global-mean surface air temperature... has apparently slowed during the last decade."							
Watanabe[33]	2000	2009		3	2	14	M	H, C3, C5
	"Reasons for the apparent pause in the rise of global-mean surface air temperature... after the turn of the century has been a mystery, undermining confidence in climate projections."							

Table 1. Summary of literature on the "hiatus".

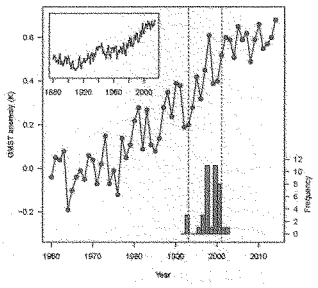


Figure 1. Global mean surface temperature (GMST) anomalies estimated by NASA's Goddard Institute for Space Studies (GISS) data set ([40] http://data.giss.nasa.gov/gistemp/, all analyses based on dataset downloaded on 17 January 2015). The histogram at the bottom represents the distribution of presumed start years for the presumed "hiatus" in the corpus of articles ($N = 40$; see Table 1) considered for this analysis. The vertical lines represent the 5th (1993) and 95th (2001) percentile, respectively, of presumed starting years for the "hiatus". The small inset shows the overall historical temperature anomalies recorded since 1880.

trends. Panel A uses the period 1951–2012, which was used by the IPCC in AR5 as the long term trend against which to define the "hiatus"[6]. Panel B uses 1964 as the onset of modern global warming, whereas Panel C uses 1976. Those two years are two standard deviations ($s = 3$) below and above, respectively, of the best estimate (1970) of the onset of modern global warming in the GISS data set reported in a recent change-point analysis[14]. Panels B and C therefore approximate the lower and upper bound, respectively, of the 95% confidence interval for the onset of modern global warming by the change-point measure. All panels include data through 2012 because many of the articles in the corpus were written when the latest available data were for 2012 (or even earlier). (See the Online Supplementary Material for an extension of our analysis to the entire instrumental record.)

To permit a commensurable comparison, in all panels the distribution of all possible trends has the same propensity of trend durations as the "hiatus" in the corpus. Thus, each possible 10-year trend is replicated 8 times (as 8 articles in the corpus presumed the "hiatus" to extend over 10 years), each 11-year trend 5 times, and so on as determined by the propensity of trend durations in the corpus. The distribution of trend durations is therefore identical between the two histograms in each panel.

Figure 2 demonstrates that, although the distribution of trends during the "hiatus" is shifted downward compared to the overall distribution of trends of the same durations, the "hiatus" distribution falls within the overall envelope of historically observed trends. For the IPCC base period (1951–2012; Panel A) there is little discernable difference between the two distributions. For the two years that bracket the

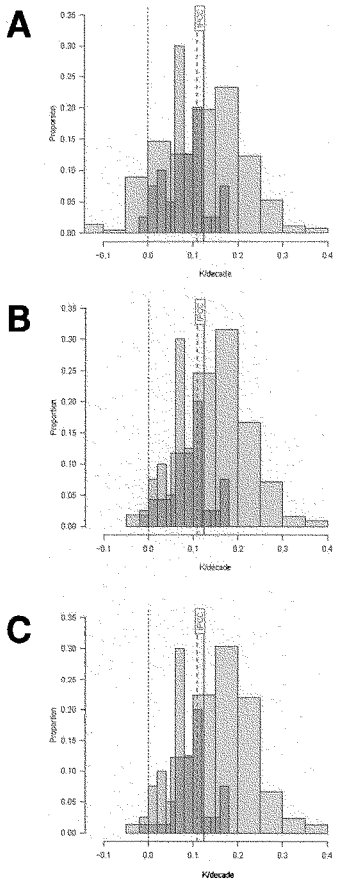


Figure 2. (**A**) distribution of observed decadal temperature trends (GISS) within the "hiatus" windows defined by the corpus of articles considered for this analysis (blue), compared to the distribution of all possible temperature trends from 1950 till 2012, the reference period used by the IPCC to establish the long-term warming trend (pink). (**B**) same distribution of temperature trends within the "hiatus" windows (blue) compared to the distribution of all possible temperature trends from 1964 till 2012 (pink). The year 1964 is the lower bound for the 95% confidence interval of a recent change-point analysis that sought to identify the

onset of modern global warming. (**C**) same distribution of temperature trends within the "hiatus" windows (blue) compared to the distribution of all possible temperature trends from 1976 till 2012 (pink). The year 1976 is the upper bound for the 95% confidence interval of a recent change-point analysis that sought to identify the onset of modern global warming. In all panels, the distribution of all possible trends is obtained by computing all trends of a given duration from all possible years within the time period considered. The duration of trends is weighted by the propensity of presumed "hiatus" durations in the corpus. Thus, each 10-year trend is replicated 8 times (as 8 articles in the corpus presumed the "hiatus" to extend over 10 years), each 11-year trend 5 times, and so on. See Table 1 for details of the distribution of presumed "hiatus" durations in the corpus. The vertical red lines in each panel represents the long-term trend (1951–2012) that was used by the IPCC in their Fifth Assessment Report as a benchmark for comparison with the "hiatus." The solid line is for the GISS dataset[40] analyzed here, and the dashed line is the same long-term trend using the UK Met Office's HadCRUT4 data set[39] used by the IPCC.

most likely change-point onset of the modern warming period (Panels B and C), the "hiatus" distribution is more clearly offset towards the lower end but it is by no means unusual or extreme.

Moreover, for nearly 15% of imputed "hiatus" trends (5 out of 40 articles in the corpus), the warming exceeded the long-term trend used by the IPCC (1951–2012; vertical red lines in Fig. 2). Similarly, nearly 20% of operationalizations (7/40) referred to a period during which temperatures increased significantly (i.e., $p < .05$ in OLS regression), which is not consistent with a "hiatus."

The results in Fig. 2 show that all operationalizations of the "hiatus" in the literature are unexceptional in the context of equivalent-length trends in the record of modern global warming. At most, the operationalizations in the literature support the conclusion that the rates of warming over some recent intervals have been toward the lower end of the historically-observed surface temperature record. However, they do not support the conclusion that there is a "pause" or "hiatus" in the warming.

The "hiatus" has always been there when sample size is small. We next analyzed the GMST data from all possible different vantage points (end years looking back in time) to examine whether a scientist in, say, 2014 or 2010 would have been justified in accepting the existence of a "hiatus" in warming relative to what would have been detectable at any other prior point in time.

Figure 3(A) shows the warming trends that were observable, given the available data at the time, for any vantage point between 1984 and 2014 (horizontal axis). For each vantage point, between 3 and 25 years were included in the trend calculation (vertical axis). The Online Supplementary Material extends this analysis to even longer time scales. Timescales of at least 17 years are known to be necessary for noise reduction and detection of a signal[15].

Figure 3(A) shows that at every year (vantage point) during the past 30 years, the immediately preceding warming trend was always significant when 17 years (or more) were included in the calculation (dots denote $p < .05$). Figure 3(B) presents the same data using a ternary classification of p-values for the linear trend into non-informative (beige), partially informative but not conventionally significant (gray), and significant (terracotta). This panel also includes three diagonal lines that identify the earliest calendar year included in the analysis. Thus, any observation to the Southeast of the line labeled "1975" only includes observations later than that, and so on for the other two lines. The observations to the Northwest of "1965" go back to 1960 (top-left corner; looking back 25 years from 1984 inclusive).

The large beige area in Panel B highlights the well-known fact that when sample size is small, statistical power is often insufficient to differentiate signal from noise. Conversely, the large terracotta area highlights the fact that when power is sufficient, the warming signal has been detectable at any point during the last 30 years, irrespective of vantage point. When one extends the period looking backwards in time, the warming trend is always significant, and the most recent vantage point(s) do not differ systematically from earlier vantage points. It follows that the data do not permit identification of a "pause" or "hiatus" during the last 10–20 years. Significantly, this conclusion is unaffected by the choice of year taken to represent the onset of modern warming (i.e., areas to the Southeast of all 3 diagonal lines in Figure 3(B) permit the same conclusion). The conclusion is also unaffected by the choice of the year during which the "pause" was examined (i.e., the vantage point).

Conversely, if one uses shorter time periods of analysis, one can find many "pauses." Using the operationalizations found in the corpus (mean duration 13.5 years), and a null hypothesis of no warming, we find that the climate "paused" strikingly often during the last 30 years. During that period, the 14-year trend escaped significance 10 times and the 13-year trend 13 times, suggesting that a "pause" occupied between 30% and 43% of a time period during which the climate warmed 0.6 K overall (Fig. 1). If the duration of the defined "hiatus" drops to below 12 years—which applies to 13 out of 40 articles (i.e., 32.5%) in the corpus—then almost everything is a "hiatus", as signified by the preponderance of beige for trends of this duration in Fig. 3(B). Anyone making a "hiatus" claim of this duration will almost always find one, not because something new and different is happening, but because of the fundamental fact that small sample sizes provide insufficient statistical power for the detection of trends. Thus, a third of the articles in the corpus either presumed that the climate has nearly always "paused" during the last 30

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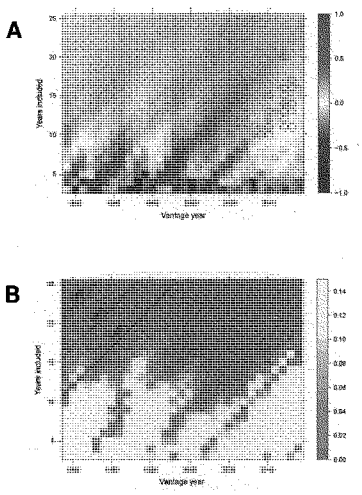


Figure 3. (**A**) Observed magnitude of temperature trends (GISS, K/decade) as a function of vantage year and the number of years included in the computation of the trend. Trends are capped at ± 1 *K* for plotting. For each vantage year, trends are computed for all possible windows between 3 and 25 years duration, all of which end with the particular vantage year. The dots indicate which trends are significant ($p < .05$) in an ordinary least squares analysis of annual means, and the horizontal dashed line indicates the number of years that must be included ($N=17$) for the trend to be significant from all vantage points. The open circles identify combinations of onset and duration that have been used to identify the "hiatus" by articles in the corpus. Multiple articles may contribute to a given circle. The Online Supplementary Material shows that the basic conclusions are unaffected by consideration of autocorrelations, although an additional 2 years are required to reach significance for all vantage points across the entire 30-year period. (**B**) Level of statistical significance for trends (GISS, K/decade) as a function of vantage year and the number of years included in the computation of the trend. Trends that are clearly non-significant ($p > .10$) are shown in beige, those that approach significance ($.05 < p < .10$) are shown in shades of gray, and significant trends ($p < .05$) are shown in shades of terracotta. The diagonal lines identify calendar years that contribute to the analysis. Any observation in the grid that lies to the Southeast of a given line includes only observations from the stated year onward, and any observation to the Northwest also includes earlier years. The observation in the top-left corner is 1960 (i.e., looking backward 25 years from 1984).

years (rendering the term meaningless), or they inconsistently highlighted only one of many events that would qualify with their definition.

These results have been replicated using a variety of additional methods that incorporate autocorrelations in the time series (see the Online Supplementary Material). The results are not sensitive to the trend detection methods employed, and they are also not sensitive to the choice of GMST data set (see the Online Supplementary Material).

We conclude that the evidence does not support the notion of a "pause" or "hiatus" as an identifiable phenomenon that is implied by standard dictionary definitions and common understandings of these terms.

Discussion

We recognize that our claim that there is no "hiatus" will be controversial, particularly in light of the widespread embrace of the "hiatus" in public and scientific discourse. Therefore, it is important to clarify what we are *not* claiming. First, and perhaps most important, we do not argue against the merit of research on decadal-scale variation in the climate. On the contrary, the numerous articles on the "hiatus" have contributed to our understanding of what drives decadal fluctuations in climate, including for example its seasonal aspects[16]. Notably, none of the articles in our corpus indicate that they expect the "hiatus" to continue indefinitely, implying that they do not support some public interpretations that recent fluctuations in the GMST undermine the scientific basis for understanding anthropogenic climate change[17].

Second, our exclusive focus on GMST relative to the null hypothesis of no trend was mandated by our goal to examine the notion of a "pause" or "hiatus" with respect to the observations alone. It does not follow that global trends constitute the only—or even preferable—level of analysis for the climate system.

Third, we do not explicitly address the question whether warming has slowed significantly during the presumed "hiatus" period, although we have suggested elsewhere that it has not[5]. In confirmation, a recent change-point analysis of GMST has shown that there is no statistically-identifiable change in warming trend after the 1970s[14].

Fourth, our analysis does not speak to the apparent or presumed discrepancy between model projections and GMST trends. Research on this question has identified several effects and variables that can reconcile apparent differences between modeled and observed temperatures during the recent fluctuation, such as model-versus-observed differences in the phasing of internal variability[11,18,19], systematic errors in some of the external forcings used in CMIP5 simulations[20,21], and incomplete coverage and quality of observations[7].

Finally, our demonstration that the "hiatus" is statistically indistinguishable from previous fluctuations has no bearing on the question of the physical causes of fluctuations in surface temperature trends. Such fluctuations can be due to internal variability alone[12,22,23], or they may involve variations in external forcings on the climate system such as solar cycles or volcanic eruptions, or both[24–26]. We have no commitment to a particular causal model of those fluctuations.

Conclusions

We have shown that there is a wide range of different operationalizations of the "hiatus" in the literature. For none of these operationalizations is the rate of temperature change meaningfully different from the set of rates of equivalent trend lengths over the modern period. That is, the "hiatus", however defined, is not unusual or unprecedented[27]. Further, the duration of periods over which trends must be extended to generate significant warming trends has not changed noticeably in the "hiatus" periods relative to the rest of the modern warming period. We conclude that there is no "hiatus", and neither has the climate system "paused."

Our conclusion raises at least two questions. First, why has so much research been directed at the "hiatus" when it does not exist? We have addressed the likely reasons for this in detail elsewhere[4]. The notion of a "pause" or "hiatus" demonstrably originated outside the scientific community[3], and it likely found entry into the scientific discourse because of the constant challenge by contrarian voices that are known to affect scientific communication and conduct[4,28,29].

The second question pertains to the broader implications of this apparent discord between data and the discussion in the literature. We suggest that discussing climate change using the terms "pause" or "hiatus" creates notable hazards for the scientific community.

Adoption of the terms "hiatus" or "pause" is not inconsequential because the way in which environmental issues are linguistically and semantically framed contributes crucially to public (mis-)understanding[30]. Scientists may argue that when they use the terms "pause" or "hiatus" they know—and their colleagues understand—that they do not mean to imply that global warming has stopped. Indeed, the use of scare quotes in some articles (Table 1) is clearly intended to imply this. The problem, however, is that these terms have vernacular meanings, and when scientists use a term from the public vernacular to describe a feature of science, confusion results when the vernacular term is not an appropriate description of that feature. This misunderstanding may be particularly acute in this instance because the terms "pause" and "hiatus" originated as contrarian talking points[3,4]. Hence, we argue that scientists should use the term that most appropriately describes what they are studying. In the present case, that implies the use of "fluctuation", not "hiatus," because when scientists use the term "hiatus", this sends a confusing and potentially misleading message to the public. Scientists might tacitly understand that global warming continues notwithstanding the alleged "hiatus", or they may intend the "pause" to refer to differences between observed temperatures and expectations from theory or models, but the public is not privy to that tacit understanding.

Moreover, acceptance and use of scientific propositions carries ethical implications and responsibility[31,32]. Some philosophers argue that holding a belief without sufficient "warrant"—i.e., without support by strong evidence—engenders a moral hazard[33]. An important element of this argument is that any belief, no matter how innocuous or inconsequential, creates the enabling conditions for similar and related beliefs. Any belief or opinion thus contributes to shaping an epistemological landscape, which

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in turn implies a responsibility—or when the belief is unwarranted, a moral hazard—for "downstream" intellectual consequences. Specifically, if unwarranted acceptance of a "hiatus" in global warming contributed to the delay of political action to mitigate climate change, with potentially adverse consequences on innocent parties, then the scientific status of the "hiatus" could become a matter not just of science and philosophy, but also ethics and even law. Lest one consider such a potential hazard remote, the legal aftermath of the earthquake in L'Aquila, Italy, which embroiled scientists in charges of manslaughter for their alleged failure to warn the community[34,35], vividly illustrates the legal and moral hazards that are incurred when the public is not adequately informed of the full envelope of identifiable risks arising from scientific findings. In this context, it is notable that in a blind expert test, the notion that global warming has "stopped" was found to be misleading in light of the data[5].

Those hazards can be largely avoided in this case by clear communication, which includes (although to be sure is not limited to) avoiding the unsubstantiated use of "pause" or "hiatus" when referring to fluctuations of GMST about the longer-term warming rate.

Methods

Corpus of articles. Table 1 summarizes the corpus of 44 articles that explicitly addressed the "hiatus", either by seeking an explanation or by reconciling it with model output. Only articles addressing global (as opposed to regional) temperatures were included. Articles were sourced by the authors with the help of a number of other researchers and climate experts who are conversant with the current literature.

For each article, the table records the number of times that keywords such as "slowing", "pause", or "hiatus" occurred in the text. Occurrences in the reference section, in running heads, or in metadata were not counted. All forms of the stem were accepted; e.g., "slow", "slowed", "slowing", and so on. Note that Crowley *et al.*[36] used another term, namely "plateau", 13 times. In addition, the word "stop" appeared 4 times in two articles[37,38]. Wherever a number is put into quotation marks (e.g., "1") this refers to the number of times the term was put into "scare quotes," implying that the term was not necessarily accepted by the author. When scare quotes were used together with unquoted occurrences, those other occurrences are provided after the "+" symbol.

Where applicable, the table also presents a quotation (usually from the abstract or first paragraph) that was judged to be indicative of the "framing" of the article. Citations or acronyms (or clauses not relevant to the meaning) in the quotation are omitted and replaced by.... When the quotation is absent for an article, a clear identification of framing was not possible. The *Focus* column indicates whether the "hiatus" was discussed primarily with respect to the observations (O) or with respect to the match between models and observations (M), or both (OM). The *Data* column indicates which data set was used by the study, where H = HadCRUT4[39]; G = GISS[40]; N = NCDC[41]; CW = Cowtan & Way[42]; C3 and C5 refer to CMIP3[43] and CMIP5[44] model ensembles, respectively; and "o" refers to other data sets.

The table also records the presumed onset of the "hiatus" as stipulated in each article (column labeled *From*) and the end of the "hiatus" (*To*). Concerning onset, articles sometimes use fuzzy terminology such as "first decade of 21st century" (interpreted to mean 2000–2009) or "2000s" (also taken to mean 2000–2009), or they contain several explicit and mutually incompatible onset times (in which case the first or more explicit one was taken as the article's declaration of onset). Similarly, the presumed end of the "hiatus" sometimes remained unclear as it was often (but not always) the "present" or time of writing of the article. It was not always possible to unambiguously identify the last observation in the data set. Because of those potential ambiguities, a second independent reader who was blind to the purpose of the study audited, and confirmed, the values derived by the first author. Unambiguous identification of onset and duration proved impossible for 4 articles, and the main analyses are therefore based on $N = 40$. The corpus reported in Table 1 does not claim to be exhaustive; note, however, that the inclusion of further articles cannot reduce the range of onset times—it could only extend it.

The *Trend* column indicates if the trend in the observations (NASA's GISS data set;[40]) was significant for the time period specified (* denotes $p<.05$) and whether it exceeded the IPCC's long-term reference trend (1951–2012), denoted by >1. Entries in this column that are labeled NA are not included in the quantitative analysis because computation of the trend was prevented by ambiguity in the operationalization of the "hiatus."

The table omits articles that did not address global mean surface temperature (GMST) but exclusively focused on other indicators such as ocean heat content or temperature[9,45,46]; sea level rise[47]; or wind[48].

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Author Contributions

J.S.R. provided the initial idea for the data analysis and S.L. provided the initial idea for the quantitative literature review. S.L. and J.S.R. conceived and designed the study and co-wrote a first draft the paper. S.L. conducted the principal analysis. All authors contributed to the conceptual framing, editing, and revision of the manuscript.

Additional Information

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CLIMATE CHANGE

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Assessing recent warming using instrumentally homogeneous sea surface temperature records

Zeke Hausfather,[1,2]* Kevin Cowtan,[3] David C. Clarke,[4] Peter Jacobs,[5] Mark Richardson,[6] Robert Rohde[2]

Sea surface temperature (SST) records are subject to potential biases due to changing instrumentation and measurement practices. Significant differences exist between commonly used composite SST reconstructions from the National Oceanic and Atmospheric Administration's Extended Reconstruction Sea Surface Temperature (ERSST), the Hadley Centre SST data set (HadSST3), and the Japanese Meteorological Agency's Centennial Observation-Based Estimates of SSTs (COBE-SST) from 2003 to the present. The update from ERSST version 3b to version 4 resulted in an increase in the operational SST trend estimate during the last 19 years from 0.07° to 0.12°C per decade, indicating a higher rate of warming in recent years. We show that ERSST version 4 trends generally agree with largely independent, near-global, and instrumentally homogeneous SST measurements from floating buoys, Argo floats, and radiometer-based satellite measurements that have been developed and deployed during the past two decades. We find a large cooling bias in ERSST version 3b and smaller but significant cooling biases in HadSST3 and COBE-SST from 2003 to the present, with respect to most series examined. These results suggest that reported rates of SST warming in recent years have been underestimated in these three data sets.

INTRODUCTION

Accurate sea surface temperature (SST) data are necessary for a wide range of applications, from providing boundary conditions for numerical weather prediction, to assessing the performance of climate modeling, to understanding drivers of marine ecosystem changes. However, in recent years, SST records have been hampered by large inhomogeneities due to a marked increase in the use of buoy-based measurements and changing characteristics of ships taking measurements (*1*, *2*). Up until the last two decades, most SST measurements were taken by ships, first with buckets thrown over the side and increasingly through engine room intakes (ERIs) after 1940. Since 1990, the number of buoy-based SST measurements has increased around 25-fold, whereas the number of observations from ships has fallen by around 25% (*3*, *4*). In the last 25 years, SST assay methods have changed from 80% ship-based in 1990 to 80% buoy-based in 2015. Modern ship-based measurements (primarily ERI, although hull contact sensors and other devices are also used) tend to generate temperature readings around 0.12°C higher than those of buoys, whose sensors are directly in contact with the ocean's surface (*1*, *5*, *6*). As the number of ships actively taking measurements available in the International Comprehensive Ocean-Atmosphere Data Set (ICOADS) database (*4*) has fallen, a growing portion of ships are also using non-ERI systems that may introduce further changes in the combined record (*1*). Although buoy records are widely considered to be more accurate than ship-based measurements, their integration with ship records into longer SST series poses a number of challenges (*3*).

The National Oceanic and Atmospheric Administration's (NOAA) Extended Reconstruction Sea Surface Temperature (ERSST) (*5*), the Hadley Centre SST data set (HadSST3) (*1*), and the Japanese Meteorological Agency's Centennial Observation-Based Estimates of SSTs (COBE-SST) (*7*) are composite SST series that assimilate data from multiple different instrument platforms (ships and buoys from ICOADS and some satellite data in the case of COBE-SST) and measurement methods (wood buckets, canvas buckets, engine intake valves, etc.) to create consistent long-term records. These three composite ocean SST series are used by the primary groups reporting global temperature records: NASA's GISTEMP (Goddard Institute for Space Studies Surface Temperature Analysis) (*8*), the Met Office Hadley Centre's and the University of East Anglia's Climatic Research Unit's HadCRUT (*9*), NOAA's GlobalTemp (*10*, *11*), the Japan Meteorological Agency (*12*), Berkeley Earth (*13*), and Cowtan and Way (*14*). Because the oceans cover 71% of Earth's surface, changes to SST series have large impacts on the resulting global temperature records.

ERSST was recently updated from version 3b (ERSSTv3b) to version 4 (ERSSTv4), adding corrections to account for the increasing use of buoy measurements and incorporating adjustments to ship-based measurements based on nighttime marine air temperature (NMAT) data from the Met Office Hadley Centre and the National Oceanography Centre's HadNMAT2 (*5*, *15*–*17*). ERSSTv3b did not include any SST bias adjustments after 1941, whereas ERSSTv4 continues these adjustments through the present. Although the largest changes to the ERSST record occurred during World War II, ERSSTv4 also indicated a higher rate of warming after 2003. This led Karl *et al.* (*18*) to conclude that the central estimate of the rate of global mean surface temperature change during the 1998–2012 period was comparable to that during the 1951–2012 period, in contrast to the Intergovernmental Panel on Climate Change characterization of the recent period as a "hiatus" (*19*). These updates also created a notable divergence between ERSSTv4, HadSST3, and COBE-SST from 2003 to the present and raise the question of which composite SST series provides the most accurate record in recent years.

Over the past two decades, reasonably spatially complete, instrumentally homogeneous SST (IHSST) measurements are available from drifting buoys, Argo floats (*20*), and satellites (see Materials and Methods for details on each IHSST series). To assess how well the composite SST

[1]Energy and Resources Group, University of California, Berkeley, Berkeley, CA 94720, USA. [2]Berkeley Earth, Berkeley, CA 94705, USA. [3]Department of Chemistry, University of York, York, U.K. [4]Independent Researcher, Montreal, Quebec, Canada. [5]Department of Environmental Science and Policy, George Mason University, Fairfax, VA 22030, USA. [6]NASA Jet Propulsion Laboratory, California Institute of Technology, Pasadena, CA 91109, USA.
*Corresponding author. Email: hausfath@berkeley.edu

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records correct for biases due to the changing instrumentation, we compare each of them in turn to IHSST series that were created using only drifting buoys, only Argo floats, and only satellite infrared radiometer data. Because these IHSST series are created from relatively homogeneous measurements from a single type of instrument, they should be less subject to bias due to changing measurement methods, although other factors, such as differences in spatial coverage or instrumental drift (in the case of satellites), need to be carefully accounted for.

Each of the three IHSST series (buoys, Argo floats, and satellites) spans a different period of time. Buoy data have reasonably complete spatial coverage of the oceans from the late 1990s to the present. Argo floats achieve sufficient coverage for analysis from January 2005, whereas reliable satellite data span from 1996 to the present. Two sources of infrared radiometer–based satellite sea skin temperature are considered: the ARC [ATSR (Along Track Scanning Radiometer) Reprocessing for Climate] SST product (*21*) from ATSR data, which provided data only through the end of 2011, and the European Space Agency Climate Change Initiative experimental record (hereafter CCI) (*22*), which combines ATSR and Advanced Very High Resolution Radiometer (AVHRR) data to obtain a continuous record for the whole period. The experimental version of the CCI record is not strictly instrumentally homogeneous and is not fully independent from in situ buoy SST observations but closely matches the independent ARC SST record during the period of overlap; the next official release of the CCI containing AVHRR and ATSR data should be fully independent of in situ observations. Three different Argo-based near-surface temperature data sets—from the Asia-Pacific Data Research Center (APDRC) (*23*), the Japan Agency for Marine-Earth Science and Technology (hereafter H2008) (*24*, *25*), and Roemmich and Gilson (hereafter RG2009) (*26*)—are examined, with a number of different data sets chosen to reflect the uncertainty introduced by attempting to reconstruct near-SSTs using Argo data.

RESULTS

From January 1997 through December 2015, ERSSTv3b has the lowest central trend estimate of the operational versions of the four composite SST series assessed, at 0.07°C per decade. HadSST3 is modestly higher at 0.09°C per decade, COBE-SST is at 0.08°C per decade, whereas ERSSTv4 shows a trend of 0.12°C per decade over the region of common coverage for all four series. We find that ERSSTv3b shows significantly less warming than the buoy-only record and satellite-based IHSSTs over the periods of overlap [$P < 0.01$, using an ARMA(1, 1) (autoregressive moving average) model to correct for autocorrelation], as shown in Fig. 1. ERSSTv3b is comparable to ERSSTv4 and the buoy and satellite records before 2003, but notable divergences are apparent thereafter.

Both the buoy-only and CCI series are very similar to ERSSTv4 during their respective periods of overlap; trends in differences are insignificant in all cases. This strongly suggests that the improvements implemented in ERSSTv4 removed a cooling bias in ERSSTv3b. The ERSSTv4 record is expected to show good agreement with the collocated buoy record, because of new ship-buoy bias corrections and the increased weight attached to buoy observations in ERSSTv4. Thus, this agreement represents a replication of the ERSSTv4 result from the same data using a substantially different methodology. The CCI data are not used in the ERSSTv4 record and therefore represent an independent validation of the ERSSTv4 record.

In addition to ERSST, we also examine how the other two commonly used composite sea surface records, HadSST3 and COBE-SST, compare with the buoy-only and satellite-based IHSST records. Both show significant cool biases in the period from 2003 to the present relative to the buoy-only record, although the magnitude of this cool bias is smaller than that found in ERSSTv3b. Difference series between all four composite records and the buoy-only and satellite-based IHSST records are shown in Fig. 2. Each difference series is constructed by restricting all four composite SST series to common grid cells for each month and by comparing all grid cells where the composite records and the IHSST in question have data available. Our conclusions are similar when we consider all-product common coverage or interpolating products to global coverage; details of the spatial coverage approach and uncertainty calculations can be found in Materials and Methods.

Two of the three Argo near-SST records assessed, APDRC and H2008, agree well with the buoy-only and satellite-based records and suggest a cool bias in ERSSTv3b during the 2005–2015 period, when sufficient Argo data are available (Fig. 3). The RG2009 series is more ambiguous, with trends that are not significantly different ($P > 0.05$) from either ERSSTv3b or ERSSTv4. Similarly, both APDRC and H2008 suggest cool biases in HadSST3 and COBE-SST, whereas RG2009 does not show a significant trend in the difference series with any of the composite temperature records (see Fig. 4). Differences between the Argo series emerge through different interpolation techniques and additional data incorporation: APDRC uses Aviso satellite altimetry for sea surface height estimates, H2008 uses a small amount of data from the Triangle Trans-Ocean Buoy Network and conductivity-temperature-depth profilers (mostly before 2005) (*25*), whereas RG2009 relies solely on Argo data.

To assess the significance of differences between composite series and IHSSTs, we examined whether trends in differences between the data sets were statistically different from 0 (that is, $P < 0.05$), as shown in Fig. 4. We looked at two periods: 1997–2015 (where buoys, CCI, and the four composite series have records) and 2005–2015 (buoys, CCI, three Argo series, and four composite series). When comparing ERSSTv4 to all six IHSSTs during both periods, there are no significant

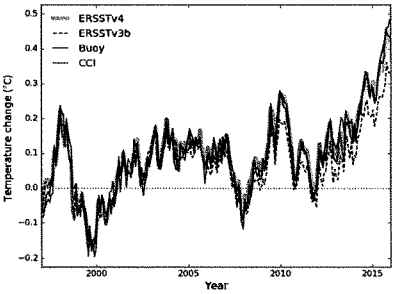


Fig. 1. Comparison of the different ERSSTv3b, ERSSTv4, buoy-only, and CCI SST monthly anomalies from January 1997 to December 2015, restricting all series to common coverage. ERSSTv4 is shown as a broad band for visualization purposes; this band does not represent an uncertainty range. The series are aligned on the 1997–2001 period for comparison purposes. Spatial trend maps are also available in fig. S1, and a similar comparison with Argo data is shown in fig. S2.

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trends in differences between the data sets except in the case of H2008, which showed slightly greater warming over the 2005–2015 period. ERSSTv3b, HadSST3, and COBE-SST show a significantly lower warming trend over the period since 1997, compared to the buoy-only and CCI records (ARC SST shows nearly identical trends to CCI during its period of coverage from 1997 to 2012, as shown in fig. S3). During 2005–2015, ERSSTv3b, HadSST3, and COBE-SST have significantly lower warming trends than the H2008 Argo record, and ERSSTv3b and HadSST3 have significantly lower trends than the APDRC Argo record. For the RG2009 Argo record, no significant trend difference can be found for any of the composite temperature series during 2005–2015.

Both ERSSTv4 (*15*) and HadSST3 (*1*) incorporate detailed assessments of fully correlated (parametric) and partially correlated (sampling and measurement) uncertainties into their respective composite SST series. ERSSTv4 assesses these combined "bias" uncertainties via an ensemble of SST reconstructions, incorporating a range of parametric setting combinations, most recently in an expanded 1000-member ensemble (*16*). HadSST3 provides a 100-member ensemble to assess parametric uncertainty but separately treats sampling and measurement uncertainty. We derived a 1000-member ensemble from the HadSST3 ensemble, with each member expanded to 10 members by adding an AR1 time series with SD and autocorrelation scaled to match the missing partially correlated uncertainty. We repeat the buoy-only and CCI IHSST comparisons on each of the realizations masked to common coverage (Fig. 5).

The ERSSTv4 ensemble is not symmetric around the operational "best" estimate, which is based on the most empirically justified

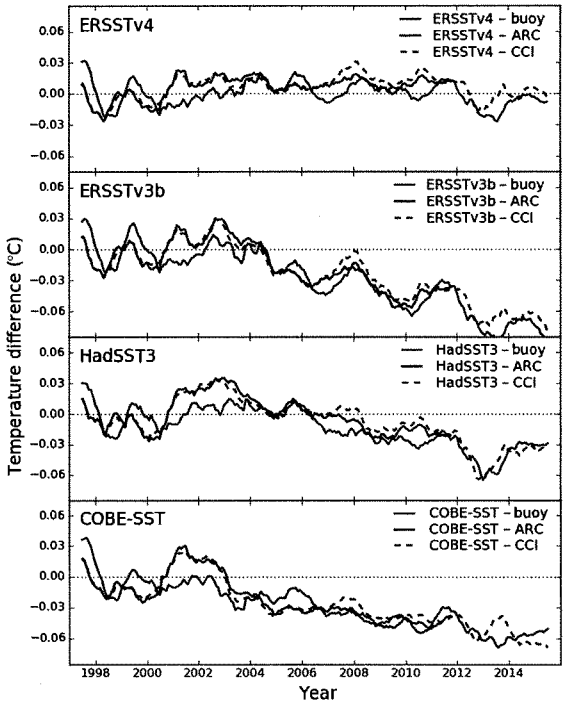


Fig. 2. Twelve-month centered moving average of temperature difference series between composite and buoy-only, CCI, and ARC SST anomalies. Values below 0 indicate that the composite series has a cool bias relative to the IHSST record.

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combination of parameter settings (5); most of the realizations have lower trends, with the lower bound of the ensemble encompassing ERSSTv3b. Only 16 of the 1000 ERSSTv4 realizations have a trend greater than that of the buoy-only IHSST record. The HadSST3 ensemble, in contrast, is largely symmetric around the operational estimate, which is based on the median of the ensemble. All of the 100-member and 1000-member HadSST3 ensemble realizations have lower trends than the buoy-only record. The increased spread of the difference between the HadSST3 ensemble members and CCI, compared to the corresponding differences with the buoy record, may arise from the interaction of the greater regional variability in the difference between HadSST3 and CCI, coupled with the time-varying coverage of HadSST3.

The structural uncertainty in the buoy record, estimated by comparing two subsets of the buoy data, is about 0.05°C in 1997, dropping to 0.027°C for the 2005–2015 period (fig. S4) as the number of observations increases. The structural uncertainties estimated, using Eq. 8 (see Materials and Methods), from an intercomparison of the IHSST records are 0.024°, 0.020°, and 0.012°C for the buoy, Argo-H2008, and CCI records, respectively. The structural uncertainties in the trends over the 2005–2015 period using Eq. 10 are 0.012°, 0.014°, and 0.009°C per decade for the buoy, Argo-H2008, and CCI records, respectively. If the Argo-RG2009 data are used in place of the Argo-H2008 data, the trend uncertainties are 0.014°, 0.020°, and 0.012°C, respectively, representing a small increase in the uncertainties for the buoy and CCI records and a larger increase in the uncertainty for the Argo data.

The trend uncertainties estimated from Eq. 8 are very similar to the uncertainty of 0.013°C per decade estimated from the ERSSTv4 1000-member ensemble. This represents a useful validation of the ERSST

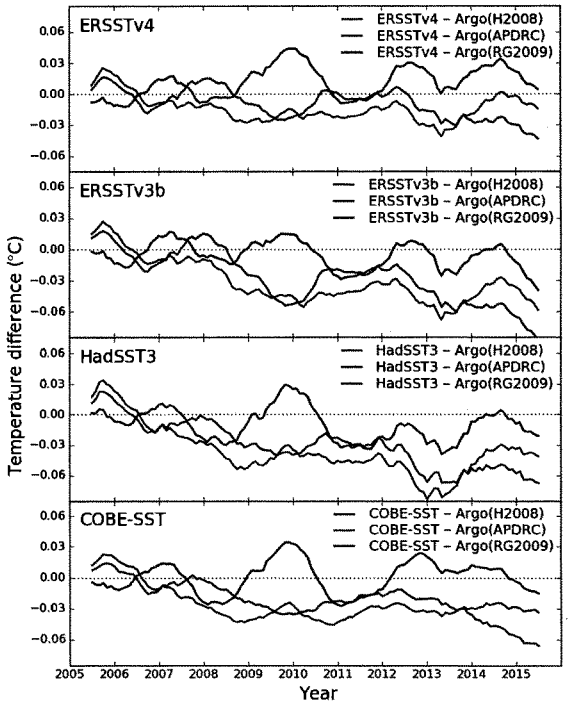


Fig. 3. Twelve-month centered moving average of temperature difference series between composite and Argo near-SST anomalies.

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ensemble, because the methods are independent: The ERSST ensemble relies on a bottom-up estimation of uncertainty from the different uncertainties in the methodology, whereas Eq. 8 yields a top-down estimate based on the differences between independent data sources. The trend uncertainties estimated from Eq. 8 are 10 to 20% of the linear trend uncertainties in the corresponding temperature trends, which include the effect of internal variability. The uncertainties are based on the region of common coverage, and inclusion of poorly sampled regions will increase the structural uncertainty. The limited time span means that uncertainties are somewhat determined by a few outliers in each temperature series; however, the results show that linear trend uncertainty should not be used as an estimate of the structural uncertainty in the trend.

The resulting difference series and trends in the all of the figures will differ modestly on the basis of how spatial coverage is handled. For each IHSST difference series, we restrict coverage for each month to the coverage shared in common between the IHSST series in question and the four composite records. This not only serves to maximize the spatial overlap between the data sets and provide a more accurate global estimate of differences for each individual IHSST but also results in difference series and trends that are not strictly comparable between IHSSTs due to coverage differences. This is particularly pronounced in the 1997–2005 period, when the buoy-only record has less coverage than the more spatially complete ARC and CCI satellite radiometer-based records. Some coverage differences also arise in the 2005–2015 period between Argo-based records and buoy/CCI records, because Argo data are largely unavailable north of 60°N, south of 60°S, or in the Malay Archipelago.

To ensure that our results are robust regardless how spatial coverage is handled, we performed two additional tests to account for both spatial and temporal-spatial consistency across the series. In the first test, we restricted all series examined for the two time periods in question (1997–2015 and 2005–2015) to only 1 × 1 latitude/longitude grid cells containing records from all series examined over those time frames. During the 1997–2015 period, we only looked at grid cells with common coverage across the four composite series, buoys, and CCI, whereas during the 2005–2015 period, we examined only grid cells with common coverage between the composite series, buoys, CCI, and all three Argo-based series. This results in a record that is less spatially complete for any given IHSST-composite series comparison but is strictly comparable between IHSSTs. Difference series and trends for this common coverage approach are shown in figs. S5 to S7. Results are largely comparable to those in the main paper, with a slightly higher trend in CCI difference series during the 1997–2015 period and a lower CCI trend during the 2005–2015 period as the only notable differences.

In the second coverage test, we applied a kriging spatial interpolation approach to the two series (buoys and HadSST3) that contain large gaps in spatial coverage for all months to create fully spatially and temporally complete records (the three Argo series and the other three composite series have their own interpolation provided, whereas satellite records are largely spatially complete apart from high latitudes). We then restricted all series to common coverage over the 1997–2015 and 2005–2015 periods, following the approach of the common coverage test. This introduces some additional uncertainty due to the kriging but ensures that the spatial coverage represented by the difference series and trends does not change from month to month and that all series have nearly complete coverage over the period of overlap. The results for the kriged series are shown in figs. S8 to S10. Here, the cooling bias in ERSSTv3b, COBE-SST, and HadSST3 is more pronounced with

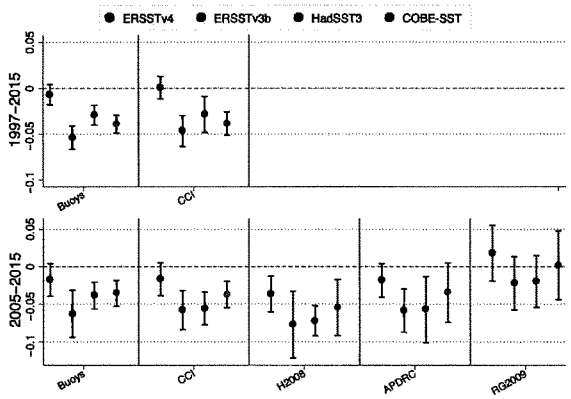


Fig. 4. Trends and 95% confidence intervals (°C per decade) in difference series for each IHSST and composite SST series, masked to common composite SST coverage. Each difference series represents a composite series minus an IHSST series. Confidence intervals for trends are calculated using an ARMA(1, 1) autocorrelation model. Values below 0 indicate that the composite series has a lower trend than the IHSST series over the period examined. The two trend periods examined are January 1997 to December 2015 and January 2005 to December 2015.

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respect to the buoy-only and CCI records, although the overall results are comparable. Interpretation of the Argo records is largely unchanged for any of the spatial coverage approaches examined.

In addition, the collocated buoy and CCI records show a spatial disagreement (not apparent in Figs. 2 and 4) that is only apparent when the CCI coverage is reduced to match the buoy coverage (see figs. S11 and S12). This arises from regional differences between the CCI record and other records, particularly before 2001. CCI shows greater warming than ERSSTv4 in the Southern Ocean but less warming in the northern mid-latitudes. The Southern Ocean is consistently cloud-covered; thus, CCI might be expected to be less accurate in these regions. Winds can also affect skin temperature retrievals

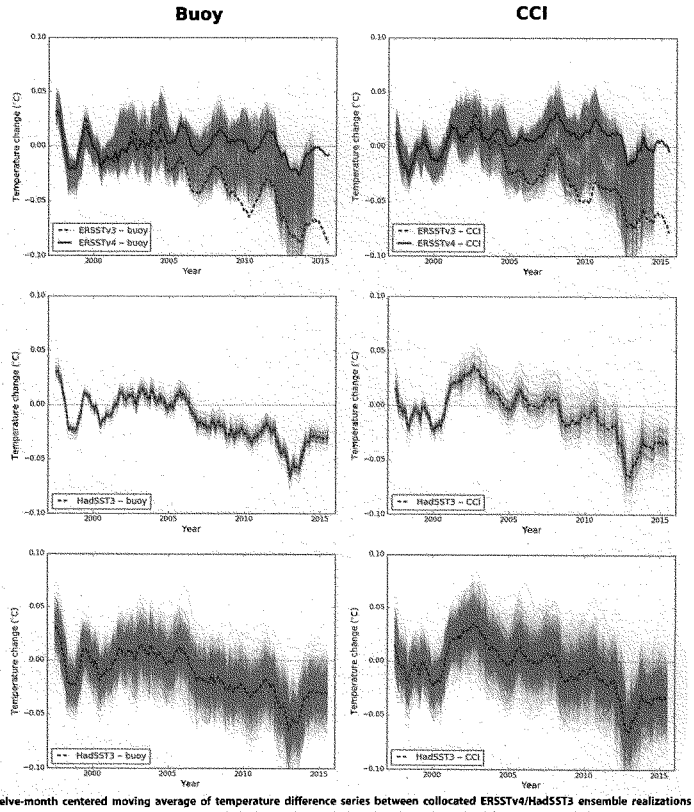


Fig. 5. Twelve-month centered moving average of temperature difference series between collocated ERSSTv4/HadSST3 ensemble realizations and IHSST anomalies. The left column shows the difference series with the buoy-only record. The right column shows the difference series with the CCI record. The top row shows 1000 ERSSTv4 ensemble members, with operational versions of ERSSTv3b and v4 highlighted in black (note that the ERSST ensemble runs only go through 2014). The middle row shows the 100 published HadSST3 ensemble members, with the operational version in black. The bottom row displays the 1000 expanded ensemble members, as discussed in the text.

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relative to those at depth. In situ observations are prevalent in the Northern Hemisphere and so may be more reliable. In the Southern Ocean, in situ observations are sparse and so temperature trends remain uncertain. The regional deviations from the in situ records and their impact on trends mean that comparisons with CCI should be treated with caution.

Coverage biases are also affected by the choice of baseline for geographical map series. The results presented use a 19-year (1997–2015) baseline for both the ERSSTv3b data to which the other series are then matched and the high-resolution climatology used in constructing the buoy record. Changing either of these to a 30-year (1986–2015) baseline has no perceptible effect on the results.

DISCUSSION

Trends in IHSSTs constructed from buoy and satellite data agree with ERSSTv4 over the 1997–2015 period but are significantly higher ($P < 0.01$) than the ERSSTv3b trend, supporting the conclusions of Karl *et al.* (*18*). Both buoys and satellites also suggest a significant ($P < 0.05$) cooling bias in HadSST3 and COBE-SST. Over 2005–2015, four of five IHSST series agree with ERSSTv4 or suggest that it might be slightly cool-biased. By contrast, four of five IHSST series suggest cool biases in both ERSSTv3b and HadSST3, whereas three of five IHSST series suggest a cool bias in COBE-SST. One of the three Argo series (RG2009) is statistically indistinguishable from all four of the composite SST products during the 2005–2015 period.

The difference in IHSST records relative to HadSST3 is particularly noteworthy, because HadSST3 includes explicit buoy-ship offset adjustments comparable to those used by ERSSTv4 and continues ship SST corrections through the present (*1*). The source of the apparent cooling bias in recent years in HadSST3 is unclear, although it is likely related to biases in ship records introduced by the changing composition of shipping fleets and a general decline in the number of available ship-based SST measurements (*4*). When comparing IHSSTs to a ship-only SST record (restricting to common coverage), we have identified a strong cool bias in the ship record, particularly since 2010. Not only are ship temperatures higher than buoy temperatures at the start of the study period (due to an approximately 0.1°C offset), the ship record substantially underestimates the rate of warming over the later part of the period as well (fig. S13). This result is supported by the satellite observations of skin temperature, the buoy measurements in the top meter of the ocean, and Argo observations from three different methodologies over depths spanning 2.5 to 20 m (fig. S14). ERSSTv4 mostly avoids this potential bias in ship records by assigning an increased weight to buoys in recent years (*5*), although the slightly higher trends in buoys, CCI, and two of the three Argo series vis-a-vis ERSSTv4 during 2005–2015 (Fig. 4, bottom) might be driven by some residual ship-related bias.

The difference in trend between ERSSTv3b and ERSSTv4 is smaller than the difference in trend between the buoy and ship records, because ERSSTv3b also incorporates data from buoys but does not account for the offset between the ship and buoy temperatures or assign the buoys more weight than ship-based measurements. HadSST3 falls between the two versions, incorporating an offset adjustment between ships and buoys and some corrections to the ship observations but equally weighting ships and buoys. NMATs (HadNMAT2), which are used as part of the ERSSTv4 homogenization, also appear to show a cool bias comparable to, if not larger than, that of HadSST3 relative to the IHSSTs in the period after 2003 (fig. S15), possibly because of the residual inhomogeneities in NMAT records. Whereas COBE-SST is also significantly cooler in recent years than the buoy-only record and CCI, a new version (COBE-SST2) incorporates buoy adjustments and shows better agreement with the IHSST records but does not extend up to the present and is not yet in operational use in the Japanese Meteorological Agency global land/ocean temperature product (fig. S16) (*27*).

Interpreting the Argo results

The Argo records cover a shorter period (11 years rather than 19), and their results are less clear-cut than the buoy and CCI IHSSTs. The H2008 and APDRC records support ERSSTv4 (and even suggest that it might be a bit too cool), although APDRC results are somewhat sensitive to the choice of start year (fig. S17). RG2009 falls between ERSSTv3b and ERSSTv4 in trend and does not reject either. Similarly, H2008 and APDRC suggest a cool bias in HadSST3 and (to a lesser extent) in COBE-SST over the 2005–2015 period, whereas the results of RG2009 are ambiguous and do not allow any differentiation between composite record trends.

The brevity of the Argo records and their divergence from other records limit the weight that can be placed on them. If the faster warming H2008 and APDRC records are accurate, then all of the IHSSTs (buoys, satellites, and Argo floats) are in basic agreement in rejecting the slower warming ERSSTv3b record. However, if the slower warming RG2009 record is correct, then this would imply either that the buoy and CCI IHSSTs are too warm during 2005–2015 or that there may be a variation in temperature trend with depth: The skin record and the top meter show faster warming, whereas the deeper ship and Argo records show slower warming. Different observational platforms sample sea "surface" (or near-surface) temperature at different depths in the mixed layer, with satellites, buoys, ships, and Argo floats observing the temperature at increasing depths. If H2008 or APDRC records are more accurate, it seems unlikely that depth plays a role in the differences between temperature trends, because the slower warming ship record is bracketed in depth by the satellite/buoy records and the Argo records. This would also suggest that measurement depth does not explain any part of the slower warming found in the ship record. However, if the RG2009 record is correct, it may suggest that the slower warming ship record arises from a combination of both depth and the bias in the ship record (because the ship record exhibits less warming than even RG2009, as shown in fig. S14).

Argo instruments have temperature profiles at depths throughout the mixed layer (and below), with the shallowest observations in any of the Argo products in the range of 2.5 to 7.5 m. Although the Argo records show no discernable reduction in trends between depths of 5, 10, and 20 m (fig. S18), they cannot exclude a difference with the top meter measured by the buoys. If there is a significant difference in temperature trend between the top meter and the remainder of the mixed layer, this would present a problem in the construction of a homogeneous SST product from the combination of ship and buoy records. Similarly, most of the Coupled Model Intercomparison Project Phase 5 (CMIP5) climate models have a top layer spanning 0 to 10 m and so may not resolve the top meter of the ocean. This could present a challenge both in testing for the depth effect in models and in comparing the models to observations. However, because two of the three Argo-based records analyzed show no significant difference with buoy and CCI surface records and the Argo series is short, any conclusions about depth-related effects appear to be premature.

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Concluding remarks

Adjustments to correct for inhomogeneities in SSTs in recent years have a large impact on the resulting decadal-scale global temperature trends. Assessing the effectiveness of these adjustments is critical to improving our understanding of the structure of modern climate changes and the extent to which trends in recent periods may have been anomalous with respect to longer-term warming. Using independent IHSST series, we find that NOAA's new ERSSTv4 effectively corrects a significant cooling bias present in ERSSTv3b during the past two decades without introducing any detectable residual trend bias. We also conclude that two other widely used composite SST series, HadSST3 and COBE-SST, likely suffer from spurious cooling biases present in ship-based records in recent years.

Some uncertainty remains, particularly in Argo-based near-SST reconstructions. Two of the three Argo reconstructions examined agree well globally with the buoy and radiometer-based IHSSTs, whereas the third does not allow for any effective differentiation between composite SST series. Similarly, although CCI and ARC-SST radiometer-based estimates agree quite well globally with the buoy-only record, there are significant zonal differences. The time period considered is relatively short, with most of the divergence between composite SST records occurring after 2003, and sufficient Argo data are only available after 2005. Nonetheless, SST time series from drifting buoys, satellite radiometers, and two of the three Argo series strongly suggest a cool bias present in ERSSTv3b, HadSST3, and COBE-SST. Overall, these results suggest that the new ERSSTv4 record represents the most accurate composite estimate of global SST trends during the past two decades and thus support the finding (*14*) that previously reported rates of surface warming in recent years have been underestimated.

MATERIALS AND METHODS

We compared composite SST records including ERSSTv3b, ERSSTv4, HadSST3, and COBE-SST to three separate IHSST records constructed from ICOADS-reporting buoys, near-surface measurements from Argo floats, and radiometer-based satellite SST records. We obtained existing spatially gridded fields for each SST series (and created novel ones in the case of buoy-only and ship-only records) and converted each to standardized 1° latitude by 1° longitude uniform grid (hereafter 1° × 1° grid).

Temperature averaging in the presence of varying geographical coverage requires that all of the temperature series be aligned on a common baseline. It is common practice to apply an offset to each cell and month of the year to bring the mean of that cell and month to 0 over a 30-year baseline period; however, this is impractical for the short buoy record. Fixing the baseline for an incomplete record is problematic in the case where the months for which observations are present are unusually hot or cold; however, the problem may be addressed by aligning the data to a more complete record containing the same weather signal. The spatially complete ERSSTv3b record was therefore aligned to 0 during the 1997–2015 period, and then the other data sets are aligned to the normalized ERSSTv3b map series. This method is a conservative choice in attempting to detect a bias in the ERSSTv3b record, as it may bias the compared series slightly toward it.

Data series were carefully aligned to ensure accurate intercomparisons of SST series. The process was as follows: Optimum Interpolation SST (OISST) was used to construct high-resolution daily climatology on the baseline period (1997–2015)—yielding 365 fields, one for each day (leap days are also treated). The buoy series was then calculated using this high-resolution daily climatology, yielding 228 monthly fields (19 years × 12 months). ERSSTv3b was also aligned to the 1997–2015 baseline. All of the composite series and IHSSTs (including the buoy series) were then aligned to the baselined ERSSTv3b on the basis of whatever months are available for each grid cell. These were then masked to common coverage and plotted in Fig. 1. This made use of the spatial completeness of ERSSTv3b to avoid artifacts due to baselining temporally incomplete cells on an incomplete baseline period; we used ERSSTv3b for this purpose to avoid biasing our results toward ERSSTv4. Pairwise difference map series were calculated between the aligned maps. The study was restricted to the 1997–2015 period, with the start date determined by buoy coverage and a data break in the ATSR-based SST data. Details of how each data set was obtained and processed are provided below.

ERSST, HadSST3, and COBE-SST

Both ERSSTv3b (*10*) and ERSSTv4 (*11*) were produced on a 2° × 2° grid, with sea ice cells recorded as −1.8°C. The ice cells were set to missing, and then the data were expanded to a 1° × 1° grid, repeating each value from the original grid to the four corresponding cells in the finer grid. HadSST3 (*1*) was produced on a 5° × 5° grid with no values for sea ice cells and was expanded to the 1° × 1° grid by repeating each value from the original grid to the 25 corresponding cells in the finer grid. COBE-SST (*7*) and COBE-SST2 (*27*) were distributed as a 1 × 1 gridded product; cells with sea ice were recorded as −1.8°C, similar to ERSST, and were set to missing. Because both HadSST3 and ERSSTv4 included ensembles of realizations with different parameterizations, for the main analysis in the paper (for example, Figs. 1 to 4), the operational version of each series was used. This is the ensemble median in the case of HadSST3, whereas ERSSTv4 provides a preferred realization.

Different approaches were used in the construction of the gridded SST products. In the HadSST3 record, observations only contributed to the grid cell and month in which they occurred, leading to some cells for which no temperature estimate was available. In the COBE-SST records, optimal interpolation was used in both space and time to create a spatially complete field from the available data. The ERSST and COBE-SST2 data sets combined a low-resolution reconstruction with the fitting of empirical orthogonal teleconnections to the observations to produce a spatially complete field, in which local temperatures could be inferred from distant observations (up to a specified distance) through teleconnections. All the records included data from ICOADS (albeit some from different releases of the database); however, in addition to differences in the processing methods, ERSSTv4 attached an increased weight to buoy observations on the basis of their lower estimated uncertainty.

Because some of the composite SST series included interpolation of observations into proximate grid cells with missing data, all composite SST series were restricted to grid cells common to the HadSST3, ERSSTv4, and COBE-SST data sets for any given month. Because HadSST3 included no explicit interpolation (apart from that implicit in its use of relatively large 5° × 5° grid cells), this should remove any differences between series due to interpolation. Failing to account for interpolation could lead to difficulty in cross-comparison of difference series between IHSST and different composite SST records.

Buoys

The buoy data were obtained from the ICOADS Release 2.5 data (*4*). Drifting buoys were selected by the World Meteorological Organization

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(WMO) buoy identifier and the presence of a value in the SST field (thus excluding Argo buoys with WMO identifiers). Moored buoys were excluded from the analysis because of an offset in temperatures between drifting and moored buoys (perhaps due to measurement depth; see fig. S19), which would introduce a bias as the proportion of moored and drifting buoys changes over the period of interest. A large majority of measurements in recent years come from drifting rather than moored buoys, and the use of drifting buoys only has no major impact on the results. The temperature field was determined by averaging buoy observations over the span of a month for each cell in a global grid. The grid consisted of cells of equal area, with equatorial cells spanning a range of 5° in both longitude and latitude. At higher latitudes, the longitudinal width of a cell in degrees was increased by calculating the area of the latitude band, dividing by the area of a 5 × 5 cell at the equator, and using that many cells in the latitude band to maintain a constant area.

The data were processed 1 month at a time. For each buoy, data were divided into days. The (typically hourly) temperature, latitude, and longitude data for that day were averaged. Buoys that showed temperature variations with an SD exceeding 1°C or positional variation with an SD exceeding 0.5° of latitude or longitude during a single day were excluded for the whole month: This can occur if a buoy is beached or picked up by a ship. The temperature was then converted to an anomaly using climatology calculated from OISST version 2 (OISSTv2) (*28*) for that day of the year and for the corresponding latitude and longitude on a finer 0.5° grid. This mitigated the biasing effects of temperature observations at the beginning or end of a month or the northern or southern edges of a 5° latitude band. The daily mean temperature anomaly for the buoy was then added to a list for the corresponding grid cell. After all buoy records were processed, all temperature anomalies for a given cell were averaged to produce a final anomaly value for that cell.

This method for constructing the buoy-only temperature record was chosen for simplicity, with the aim of reducing the possibility of methodological artifacts, such as infilling distorting the result: A consequence of this is that the resulting temperature reconstruction is limited to regions where observations are available. However, simplicity does not in itself preclude bias: An overly simple method might, for example, fail to detect some faulty observations. This possibility will be addressed through internal consistency checks on the buoy data.

Another possible source of bias is miscalibration of the temperature sensor, leading to systematically lower or higher readings. Normally, these would contribute noise rather than a bias in the trends as the miscalibrated buoy moves into more or less sampled regions and so receives a different weight in the temperature calculation. However, if new buoys are introduced, which are systematically different in calibration relative to older buoys, a bias in the trends could result. There was no sign of such a bias in the comparisons between different IHSSTs, and the cross-validated uncertainties were lowest for the recent period where the composite records show most difference.

Additional interbuoy comparisons were performed to address this possibility. For each grid cell and month where at least three buoys contributed observations, a bias estimate was calculated from the difference between the mean anomaly for the buoy and the mean of the anomalies for all the remaining buoys in that cell. All the bias estimates for a buoy were collected, and buoys for which the magnitude of the mean bias or SD of the bias estimates exceeded 1°C were eliminated, reducing the total number of buoys by about 10%. In a further test, the temperature record was recalculated, applying the resulting bias adjustment to the readings from each buoy in turn.

Four versions of the buoy record were prepared to evaluate the potential impact of buoy biases, as follows:

(1) Using all of the data, omitting the test for daily variability.

(2) Filtering on the basis of daily variability only (the default per-buoy filter, described at the start of this section).

(3) Filtering on the basis of daily variability and interbuoy variability (that is, the additional filter described in the previous paragraph).

(4) Filtering on the basis of monthly and interbuoy variability and application of the bias correction [as in (3) but then recalculating the buoy record after applying a correction to each buoy on the basis of its mean difference with passing buoys].

The resulting temperature series are shown in fig. S20, along with the differences of the other methods from the default method. The largest difference arose from using all of the data without filtering for daily variability. Interbuoy variability and bias correction made a smaller difference. The differences between the methods were small compared to the differences between the composite records. The default method using a per-buoy filter showed the lowest trend during 1997–2015 and was therefore a conservative choice.

The buoy coverage was limited, particularly in the 1990s, and comparisons to other data sets may have been affected by coverage bias. To produce an unbiased comparison to other data sets, all the data sets were expanded onto a 1° × 1° grid. Comparisons were made using only the cells for which the data sets being compared had values. The area-weighted mean temperature was then calculated for each record using the common coverage cells. The percentage of global ocean covered by buoy measurements varies from around 40% in the mid-1990s to around 70% in recent years.

Ships

The ship record was constructed in the same way as the buoy record, with one exception: Many ships only report once per day, and from 2007, some ship identifiers were masked for security reasons (although this has been improved in Release 3 of ICOADS). The test to detect excessive motion or variation within a single day was therefore omitted. The only quality control applied to the ship record therefore arose from the calculation of the global mean of the SST field, which excluded observations that fell in land areas. The ship observations were subject to significant quality issues, and the limited quality control implemented in this record therefore provided no more than a general indication of the presence and scale of any bias in the ship record.

Argo floats

Three different gridded Argo data provided online by the International Pacific Research Center APDRC (*23*), the Japan Agency for Marine-Earth Science and Technology (H2008) (*24*, *25*), and Roemmich and Gilson (RG2009) (*26*) were used. These data were produced on a monthly 1° × 1° grid and were smoothed and infilled by the data provider using a variational analysis technique to provide global coverage over all cells unaffected by seasonal sea ice. Sea surface height was used as part of the interpolation process in APDRC, whereas cells containing sea ice were represented by missing data. The data did not require regridding and were aligned to the ERSSTv3b data, as described previously.

The RG2009 Argo product had temperature values at 2.5, 10, and 20 dbar and deeper levels; the H2008 product had temperatures at 10, 20, and 30 dbar and deeper levels, and the APDRC product had

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temperature values at 0, 5, and 10 m and deeper levels. We used the 5-m level for the APDRC product, the 10-dbar (10 m) level for the H2008 product, and the 2.5-dbar level for the RG2009 product (which represented measurements ranging from 2.5 to 7.5 dbar with a mean level of 5 dbar/m) to provide the most comparable and highest available depths; estimated 0-m temperatures from APDRC were not used because they resulted from interpolation (because no Argo floats sampled sea skin temperatures).

Throughout the paper, we refer to the record derived from Argo floats as "near-SST," because the highest level of the ocean measured by most Argo floats is approximately 5 m below the surface (*26*). However, with the exception of satellite radiometer-based estimates, all of the instruments used in this analysis recorded ocean temperatures at depths between 0 and 20 m. For example, ships tend to measure temperatures through ERI valves at depths of 7 to 11 m for large ships and 1 to 3 m for small ships (*3*). Moored buoys typically measure SSTs at a depth of 3 m, whereas drifting buoys measure SSTs at around 0.5 m. Recent work (*29*) found no long-term difference in warming rates between depths of 0 to 4 m and depths of 4 to 9 m in a CMIP5 model; similarly, we have establisbed that our results are robust when using the next deeper level of each Argo data set (fig. S19). The different depths sampled by the different observational systems provide a basis to assess whether depth plays a role in the rate of recent warming.

Argo data have been used to create SST analogs in the past; for example, Roemmich and Gilson (*30*) compared ARGO "near"-SST to NOAA's OISSTv1, whereas Roemmich *et al.*(*31*) compared a 5-m Argo-based SST record to OISSTv2. Here, we performed a similar analysis using the Argo-based fields provided by RG2009, APDRC, and H2008.

Satellites

The ATSR instruments provided infrared images of Earth, from which skin temperatures may be derived. ATSR data were incorporated into two gridded data sets, the ATSR ARC (*21*) spanning the 1996–2012 period and the experimental National Center for Earth Observation/European Space Agency SST CCI Analysis L3S version EXP-1.2 (ESA-CCI or CCI) (*22*), which also incorporates data from the AVHRR and spans the period from 1996 to the present (end of 2015). Coverage between 60°S and 60°N was largely complete (except for a few cells each month in the ATSR record, which were affected by cloud, typically in the Southern Ocean or North Atlantic). Both the ATSR-only (through mid-2012) and ATSR + AVHRR (through present time) CCI data were analyzed, and the CCI data were used in the paper because they extend to the present (and differences between the two were minor during the period of overlap, as shown in Fig. 6).

Spatial coverage

The main figures in the paper were generated by limiting difference series to common spatial coverage between the four composite SST series and the IHSST in question. For example, a difference series between ERSSTv4 and the buoy-only record would show the difference for all grid cells for each month, where all four composite SST series and the buoy-only record had data available. The requirement that all four composite series share the same coverage is intended to remove the effects of interpolation on the results, because all largely rely on the same ICOADS data.

Two additional tests described in the Discussion were undertaken to ensure that the results were robust to choices of how coverage was handled. In the first test, the analysis was carried out for the two periods of interest (1997–2015 and 2005–2015), restricting the analysis to only grid cells, where all series available for tbose periods had coverage. During the 1997–2015 period, this means that only 1 × 1 latitude/longitude grid cells (where the four composite series, the buoy-only record, and the CCI record all had coverage for any given month) were used. During 2005–2015, grid cells required coverage by the four composites, buoys, CCI, and all three Argo records to be used.

In the second test, we created fully spatially and temporally complete fields to control for both difference in coverage for any given time period and changes in coverage over time. Infilling was performed on the gridded data using the original grid sampling for that record: For the buoy record, this was on the 550-km equal area grid, and for the HadSST3, this was on the 5° × 5° grid. The resulting infilled field was then copied onto a 1° × 1° grid as before. Infilling was performed using the method of kriging (*32*), by which the values at unobserved locations were inferred from the observed values. Each observation was weighted on the basis of distance from the target location using a variogram, relating the expected variance between two grid cells to the distance between them, which was determined from grid cells for which observations were available, fitted with an exponential model controlled by a single range parameter, which was the e-folding distance of the variance. The kriging calculation also used the covariance between locations where observations were present to estimate the amount of independent information in each observation. The buoy record showed longer range autocorrelation than the HadSST3 data, with respective e-folding distances of 1400 and 900 km, suggesting that the buoy record showed more spatial autocorrelation.

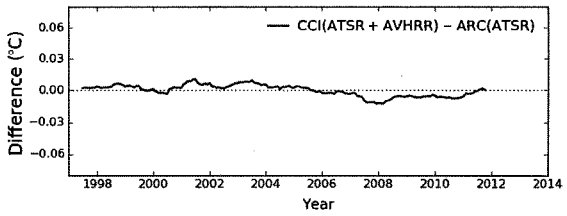


Fig. 6. Twelve-month centered moving average of differences between CCI ATSR + AVHRR and ATSR-only ARC SST records during the period of overlap. The earlier IHSST ARC shows small differences to the newer combined version; however, the differences are minor compared to the differences relative to the composite SST records.

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Infilled temperature observations will therefore be a weighted combination of the nearest observations if there are observations within a small multiple of the e-folding distance. Locations that are very distant from any observation will tend toward an optimal estimate of the global mean of the temperature field.

Uncertainty estimation

SST reconstructions include uncertainties due to limitations of both the data and the methods. Differences between reconstructions may arise because of random errors in the data or errors introduced during processing or because of uncorrected biases in the observational data. Identification of a bias requires that the difference between reconstructions must be shown to be larger than can be accounted for by random errors alone. To that end, we now examined different methods for the determination of the uncertainty in a reconstruction. Two approaches were used. First, collocated temperature difference series were used to estimate the significance of the differences. Second, a method was outlined for the use of independent temperature series to directly estimate the structural uncertainty in each series.

Significance of the temperature difference series trends.

To assess the significance of differences in trends between temperature series, we first calculated the difference temperature series from the difference map series to eliminate differences in coverage. The trend in the difference series was then compared to the uncertainty in that trend estimated using an appropriate autoregression model and used to determine whether the trend difference was significantly different from 0.

The trend in the difference series is identical to the difference in the trends between the two series, assuming that both map series are reduced to common coverage. However, calculation of the trend in the difference series offers a benefit when determining the uncertainty in that trend (*33*). If the trend difference is calculated from the trends of the individual series, the uncertainty in the trend difference requires the determination of the covariance between the model residuals. The respective residuals contain common internal variability and so are strongly correlated; therefore, the covariance term is positive. Omission of the covariance term leads to the uncertainty in the trend difference being markedly overestimated. With the covariance term included, estimates of the uncertainty in the trend difference from either the difference series, or from the two individual series, give identical results.

The difference series linear trends were estimated with ordinary least squares (OLS), with SE correction to account for serial correlation of the residuals (*34*–*36*). The general approach is to estimate the effective sample length (and, thus, the effective degrees of freedom) from an estimate of the positive autocorrelation of the residuals

$$n_e = n_t \Big/ \left(1 + 2\sum_{j=1}^{n-1} \varrho_j\right) \tag{1}$$

where n_t is the original series length, n_e is the effective sample length, and ϱ_j is the autocorrelation at lag j of an autoregressive (AR) or ARMA noise model estimated from the OLS residuals. An ARMA(1, 1) model was used for all gridded and global difference series (for example, ERSSTv4-buoys). The ARMA model coefficients were estimated with maximum likelihood for global series and Yule-Walker (moments) for gridded series trends. An ARMA(1, 1) series X_t, with white noise series ϵ_t satisfies

$$X_t = \phi X_{t-1} + \epsilon_t + \theta\epsilon_{t-1} \tag{2}$$

Then, the autocorrelation function (ACF) of an ARMA(1, 1) series is given by

$$\begin{aligned} \varrho_0 &= 1 \\ \varrho_1 &= (\phi + \theta)(1 + \phi\theta)/(1 + 2\phi\theta + \theta^2) \\ \varrho_j &= \varrho_1\phi^{j-1},\ j \geq 2 \end{aligned} \tag{3}$$

where ϕ and θ are the respective AR and MA coefficients.

Because the assessed trends cover only 11 to 19 years (132 to 228 months), a bias correction was also applied to the global difference series trends to account for the underestimate of autocorrelation in these short series (*35*, *37*). The original Tjøstheim and Paulsen correction for the AR(1) estimated coefficient ϕ is given by

$$\phi_{bc} = \phi + (1 + 4\phi)/n_t \tag{4}$$

The bias correction of ARMA(1, 1) estimated ACF coefficients ϱ_1, and ϕ generalizes (*7*) by also accounting for the positive difference between ϕ and ϱ_1. Note that the AR(1) bias correction in Eq. 4 then becomes a special case where $\theta = 0$ and $\varrho_1 = \phi$ [AR(1) is employed in the few cases where this results in more conservative uncertainties].

$$\begin{aligned} \phi_{bc} &= \phi + \big(1 + 4(2\phi - \varrho_1)\big)/n_t \\ \varrho_{1bc} &= \varrho_1 + \big(1 + 4(2\varphi - \varrho_1)\big)/n_t \end{aligned} \tag{5}$$

The ARMA coefficient estimates ϕ_{bc} and ϱ_{1bc} can then be substituted into the appropriate specific form of Eq. 1. The ARMA(1, 1) formulation in Eq. 3 can then be simplified as (*36*)

$$n_e = n_t \Big/ \left(1 + 2\sum_{j=1}^{n-1} \varrho_{1bc}\phi_{bc}^{j-1}\right) \approx n_t/\big(1 + 2\varrho_{1bc}/(1 - \phi_{bc})\big) \tag{6}$$

IHSST uncertainty estimation.

The methods presented so far allowed us to estimate the significance of the differences between temperature series. However, the ability to estimate the uncertainty in each individual IHSST series would also be useful. Two methods will be used, the first based on the internal consistency of the buoy data and the second based on the intercomparison of the IHSST temperature data sets.

The uncertainty in the buoy data may be estimated by dividing the buoys into two random subsets and calculating gridded temperature data from each subset of the data. Global temperature series were then calculated from the collocated values from each map series. A 120-month moving root mean square difference between the two temperature series provides an estimate of the uncertainty in the global temperature for the region of common coverage (after scaling by $1/\sqrt{2}$). This uncertainty estimate includes the effects of random measurement errors, as well as a sampling error that increases with decreasing coverage; however, it does not include coverage uncertainty or systematic biases affecting all of the buoys.

In the second approach, an estimate of the uncertainties in each of the IHSST series is obtained from the difference temperature series for the overlap period 2005–2011. The uncertainty in the difference series

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between the buoy and Argo data arises from the sum of the variances of the two series, assuming that the series are independent

$$\sigma^2_{\text{buoy-Argo}} = \sigma^2_{\text{buoy}} + \sigma^2_{\text{Argo}} \quad (7)$$

and assuming similar expressions for the remaining two series, where σ^2 is the squared uncertainty in the given temperature series. The squared uncertainty in the difference temperature may be estimated from the variance of the difference series, adjusting the number of degrees of freedom to account for the removal of the annual cycle from the difference series.

The uncertainty in a given series may then be estimated using equations of the following form

$$\sigma^2_{\text{buoy}} = {}^1/_2\left(\sigma^2_{\text{buoy-Argo}} + \sigma^2_{\text{buoy-CCI}} - \sigma^2_{\text{Argo-CCI}}\right) \quad (8)$$

The resulting uncertainty estimates include the effects of random measurement errors and any biases in the independent data sources, which are not correlated across the data sources; however, as before, they do not include coverage bias. This is similar to the approach outlined in O'Carroll *et al.* (*38*).

The uncertainty in the trend in an IHSST series may be estimated from the uncertainty in the monthly temperatures obtained from Eq. 8 using the equation

$$\sigma^2_\beta = \frac{\nu\sigma^2}{\sum_i (t_i - t)^2} \quad (9)$$

where σ^2_β is the variance of the trend, σ is the SD of the time series values, t_i is the date of the ith value in fractional years, and ν is the number of months of data per effective degree of freedom (*36*). Note that this differs from the ordinary equation for the uncertainty in a trend in the use of the SD of the time series in place of the SD of the residuals—this is because the difference in trends between a pair of series also contributes to the uncertainty. For the trend of a set of contiguous monthly values, this simplifies to

$$\sigma^2_\beta = \frac{\nu\sigma^2}{\Delta t^3} \quad (10)$$

where Δt is the length of the period in years. ν is about 2 for the buoy series or about 8 for the smoother Argo or CCI series.

SUPPLEMENTARY MATERIALS

Supplementary material for this article is available at http://advances.sciencemag.org/cgi/content/full/3/1/e1601207/DC1

fig. S1. Trend maps on the 2005–2015 period for all of the composite records, and for the buoy, Argo, and CCI records.

fig. S2. Comparison of ERSSTv3b and ERSSTv4 with three different Argo-based near-SST records, using the same spatial restrictions as in Fig. 1, but with ERSSTv4 aligned to 1997–2001 (inclusive), with all other series aligned onto ERSSTv4 using the 2005–2007 period because of the limited time span with Argo data.

fig. S3. Trends and 95% confidence intervals (°C per decade) for the 1997–2012 period for buoy, ARC, and CCI IHSSTs and each composite SST series, masked to common composite SST coverage.

fig. S4. Cross-validated uncertainties for the buoy record, whether with no climatology or with daily climatologies derived from the OISSTv2 daily reanalysis data.

fig. S5. Twelve-month centered moving average of temperature difference series between composite and buoy-only, CCI, and ARC SST anomalies restricted to common coverage across all series shown (four composites, buoys, and ARC/CCI).

fig. S6. Twelve-month centered moving average of temperature difference series between composite and Argo near-SST anomalies restricted to common coverage across all series with records from 2005 to 2015 (four composites, three Argos, buoy-only, and CCI).

fig. S7. Trends and 95% confidence intervals (°C per decade) in difference series for each IHSST and composite SST series, masked to common coverage for all series available.

fig. S8. Twelve-month centered moving average of temperature difference series between composite and buoy-only, CCI, and ARC SST anomalies, with the buoy and HadSST3 series kriged and all series reduced to common coverage to ensure consistent complete spatial and temporal coverage.

fig. S9. Twelve-month centered moving average of temperature difference series between composite and Argo near-SST anomalies with the buoy and HadSST3 series kriged and all series reduced to common coverage to ensure consistent complete spatial and temporal coverage.

fig. S10. Trends and 95% confidence intervals (°C per decade) in difference series for each IHSST and composite SST series, with the buoy and HadSST3 series kriged and all series reduced to common coverage to ensure consistent complete spatial and temporal coverage.

fig. S11. Trend difference maps from January 1997 to December 2015 for the difference between ERSSTv4 and CCI.

fig. S12. Differences between ERSSTv4 and CCI by latitude zone.

fig. S13. Buoy-only and ship-only temperature anomalies from January 1997 to December 2015, with no matching of coverage.

fig. S14. Difference between ship-only record and the three Argo series using a 12-month centered moving average.

fig. S15. Comparison of COBE-SST and COBE-SST2 to the IHSSTs using a 12-month centered moving average.

fig. S16. Comparison of HadSST3 and HadNMAT2 to the IHSSTs using a 12-month centered moving average.

fig. S17. Trends in differences for ERSSTv4 records versus IHSST records, with common coverage from 1997 (buoys and CCI only as dashed lines) and common coverage from 2005 (buoys, CCI, and Argos as solid lines).

fig. S18. Differences between Argo series at minimum reported depth, and differences within each Argo series as minimum reported, 20- and 50-m depths.

fig. S19. Comparison of buoy records composed of all buoys (drifting + moored) and only drifting buoys.

fig. S20. Comparison of drifting buoy-based IHSST records for different quality control and homogenization choices.

fig. S21. Twelve-month centered moving average of differences between IHSST series from January 1997 to December 2015 when reduced to common coverage for each separate pairing.

fig. S22. Trend difference maps during 2005–2015 for the composite records versus Buoy, CCI, and Argo (H2008).

fig. S23. Trends in differences for composite versus buoy (solid lines) and CCI (dashed lines) IHSST records with common coverage.

fig. S24. Number of observations over time by instrument type in the ICOADS (version 2.5) database.

fig. S25. Similar to fig. S24, but showing the percentage of ICOADS observations in each year from each instrument type.

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Debunking the climate hiatus

Bala Rajaratnam[1,2,3] · Joseph Romano[3,4] · Michael Tsiang[1] · Noah S. Diffenbaugh[1,2]

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Abstract The reported "hiatus" in the warming of the global climate system during this century has been the subject of intense scientific and public debate, with implications ranging from scientific understanding of the global climate sensitivity to the rate in which greenhouse gas emissions would need to be curbed in order to meet the United Nations global warming target. A number of scientific hypotheses have been put forward to explain the hiatus, including both physical climate processes and data artifacts. However, despite the intense focus on the hiatus in both the scientific and public arenas, rigorous statistical assessment of the uniqueness of the recent temperature time-series within the context of the long-term record has been limited. We apply a rigorous, comprehensive statistical analysis of global temperature data that goes beyond simple linear models to account for temporal dependence and selection effects. We use this framework to test whether the recent period has demonstrated i) a hiatus in the trend in global temperatures, ii) a temperature trend that is statistically distinct from trends prior to the hiatus period, iii) a "stalling" of the global mean temperature, and iv) a change in the distribution of the year-to-year temperature increases.

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✉ Bala Rajaratnam
brajarat@stanford.edu

[1] Department of Earth System Science, Stanford University, 473 Via Ortega, Rm 140, Stanford, CA 94305-4216, USA

[2] Woods Institute for the Environment, Stanford University, 473 Via Ortega, Stanford, CA 94305-4216, USA

[3] Department of Statistics, Stanford University, Sequoia Hall, Stanford, CA 94305-4065 USA

[4] Department of Economics, Stanford University, 579 Serra Mall, Stanford, CA 94305-6072, USA

We find compelling evidence that recent claims of a "hiatus" in global warming lack sound scientific basis. Our analysis reveals that there is no hiatus in the increase in the global mean temperature, no statistically significant difference in trends, no stalling of the global mean temperature, and no change in year-to-year temperature increases.

1 Introduction, Motivation and Approach

The international debate on the "hiatus" in the warming of the global climate system over the last 15 years has intensified (e.g., Meehl et al. (2011), IPCC (2013), Otto et al. (2013), Fyfe et al. (2013), Kosaka and Xie (2013), Santer et al. (2014), Trenberth and Fasullo (2013), Smith (2013), Guemas et al. (2013), Chen and Tung (2014), Boykoff (2014), Hawkins et al. (2014), England et al. (2014), Karl et al. (2015), Cowtan et al. (2015)). The implications of the purported hiatus (also referred to as a "pause" or "slowdown") are far reaching. First, contradictory scientific conclusions have emerged regarding the relationship between climate change and anthropogenic global warming, especially during a period of heightened carbon emissions (Kosaka and Xie 2013). Second, the discrepancy between climate model projections and observations appear to point to an overestimation of climate sensitivity to anthropogenic forcings (Otto et al. 2013; Fyfe et al. 2013).

The perceived hiatus has led to a myriad of resources being expended on trying to better understand the geophysical mechanisms that lead to a possible hiatus (including, among others, volcanic activity (Santer et al. 2014), Pacific Ocean variability (Kosaka and Xie 2013; Trenberth and Fasullo 2013), and increased ocean heat uptake (Smith 2013; Guemas et al. 2013; Chen and Tung 2014)), as well as spurious artifacts of the global climate observing system (Durack et al. 2014; Cowtan and Way 2014; Karl et al. 2015; Cowtan et al. 2015). The purported hiatus has therefore inspired valuable scientific insight into the processes that regulate decadal-scale variations of the climate system. However, the perception of a hiatus has important repercussions for public decision making, as the implications that global warming has paused or slowed down (Boykoff 2014; Hawkins et al. 2014), and that climate models have overestimated the rate of warming (e.g., Fyfe et al. (2013)), both influence the perceived level of mitigation action that is needed to obtain particular policy targets (Otto et al. 2013).

Fundamental to any work on the hiatus is to ascertain whether there is sufficient empirical evidence in support of its existence. Surprisingly, to our knowledge, a rigorous statistical analysis has not been undertaken, at least not one which incorporates temporal dependencies without making strong assumptions about the underlying process. Without empirical evidence in support of the hiatus claims, any further conclusions stemming from the assumption should be called into question.

As a part of our investigation to better understand the hiatus, we develop a comprehensive scientific framework that is intended to systematically test hypotheses that have been implied in statements claiming a hiatus in global warming. We first identify a typology of the scientific assertions that have been put forward, including i) that there has been a hiatus in the trend in global warming, ii) that there is a difference in trends before and during the hiatus, iii) that there has been a hiatus in the change in mean global temperature, and iv) that there is a difference in warming before and during the hiatus (when accounting for possibly non-linear increases without explicit reference to a linear trend). (See Supplementary Section 3.4 for more detail on the typology.) We next connect these scientific claims with four classes of distinct testable statistical hypotheses, with each hypothesis focusing

on different aspects of the underlying (unknown) temperature process. We then identify and develop appropriate statistical tools in order to test each of these hypotheses in a principled manner, and under progressively less restrictive - and therefore more generally applicable-modeling assumptions, thereby allowing for a deeper understanding of the nuances of the global temperature time series. In particular, we attempt to properly account for temporal dependence, we use less restrictive resampling methods to assess statistical significance, and we employ a flexible nonparametric modeling approach. By applying these progressively more general techniques in a cascading approach, we are able to test the extent to which invalid statistical assumptions can lead to erroneous scientific conclusions.

Our analysis is first undertaken using the NASA-GISS global mean land-ocean temperature index. It is subsequently also repeated on the NOAA and HadCRUT4 datasets for comparison purposes (see Supplemental Tables 1 and 2). The analysis is also undertaken on the recently released ERSSTv4 (Karl et al. (2015)) datasets (see Supplemental Tables 11 and 12). Plots of the NASA-GISS raw and smoothed global mean land-ocean temperature index from 1880 to 2013, with the base period 1951–1980, are given in in Fig. 1 (top). As there is a clear underlying trend, a moving average is superimposed on the time series. A statistical analysis of the serial correlation in the residuals after fitting a regression line is also given in Fig. 1 (top). The autocorrelation in the temperature time series is non-negligible.

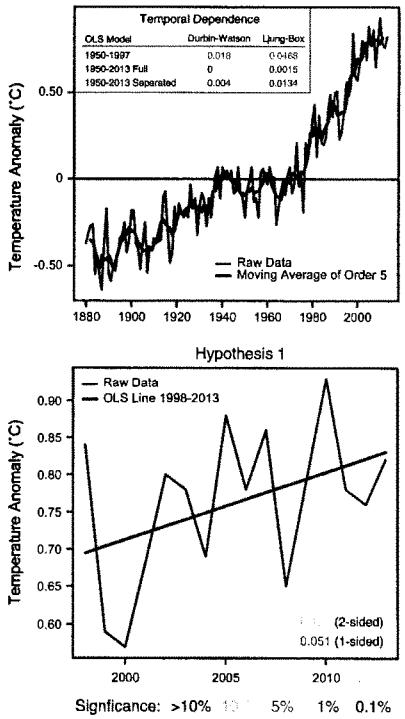


Fig. 1 *Top panel* global mean land-ocean temperature index from 1880 to 2013, with base period 1951–1980 and moving average superimposed. The table provides Durbin-Watson and Ljung-Box p-values for the residuals from three OLS fits between 1950–2013. The Ljung-Box test here considers residual autocorrelation in the first 20 lags. The 1950–2013 Full OLS model fits a single regression line to all observations from 1950 to 2013. The 1950–2013 Separated model fits a separate regression line to the 1950–1997 and 1998–2013 periods. *Bottom panel* plot of the global mean land-ocean temperature index, from 1998 to 2013, with the ordinary least squares regression line superimposed

The presence of autocorrelation motivates the need to use less naive statistical methods to understand the evolution of temperature over time (see also Supplemental Section 2.2).

2 Methods

Datasets The datasets of global surface temperature anomalies used in our analysis come from three sources: the NASA Goddard Institute for Space Studies (GISS) Surface Temperature Analysis (GISTEMP) Data, the NOAA National Climatic Data Center (NCDC) data, and the HadCRUT4 data, produced from the Met Office Hadley Centre in collaboration with the University of East Anglia Climatic Research Unit (CRU). Each source combines monthly land and sea surface temperature measurements into spatial grids that are then averaged into a single global temperature series. Temperature anomalies are computed from a baseline period, which differs by dataset. The differences in the three datasets largely come from the adjustment/infilling methods for sparse temporal/spatial coverage (Hansen et al. 2010; Morice et al. 2012). (See the Supplemental Section for more details). Note that given the global mean temperature data that is available, the main goal of our analysis is to understand the possible mischaracterizing of hiatus claims as compared to understanding the source of observational errors of the temperature process.

Temporal dependence and uncertainty quantification The global temperature record exhibits temporal correlation. Standard statistical methods tend to ignore this important feature, which in turn can lead to incorrect statistical modeling assumptions and incorrect statistical significance, which can in turn lead to erroneous scientific conclusions. For the purposes of uncertainty quantification when testing each of the four statistical hypotheses, we either model the temporal dependence in the global temperature time series explicitly through a parametric autoregressive model, or account for it through the nonparametric circular block bootstrap, stationary block bootstrap, or subsampling. (See the Supplemental Section for more details.)

Statistical hypothesis testing The various scientific assertions regarding the global warming hiatus are collected into four groups and then formulated as four testable statistical hypotheses. These four hypotheses are specified rigorously, in a principled statistical framework, and are given in Supplemental Sections 3.1, 3.2, 3.3 and 3.4. The Wald test is used to test slope parameters in the linear regression model in Hypotheses I and II leading to Normal or t-distribution based p-values. Moreover, p-values based on the bootstrap and subsampling are also calculated as alternatives to the Wald test whenever appropriate. When comparing two distributions, the Kolmogorov-Smirnov test is used, together with the bootstrap or subsampling, to account for temporal dependence. (See the Supplemental Section for more details.)

Observational uncertainties It is important to recognize that the temperature data that is used in our analysis are estimates of an unobserved process and is thus subject to observational errors and the implied uncertainties. Observational uncertainties could arise due to various factors, including instrumental error, changes in the observing network configuration and observing technology, and also due to uncertainties in adjustments made to the data. The HadCRUT4 dataset allows an analysis that incorporates observational uncertainties. The single time-series used for the analysis of the HadCRUT4 data is actually derived from

multiple time series which are constructed in order to reflect observational uncertainties. This analysis is provided in Supplemental Section 4.

3 Results

3.1 Hypothesis I: hiatus in temperature trend during 1998–2013

A basic assertion regarding the hiatus is that the steady increase in global surface temperature around a linear positive trend has stopped, or "paused" (Guemas et al. 2013). This sentiment is reflected in statements that "Despite a sustained production of anthropogenic greenhouse gases, the Earth's mean near-surface temperature paused its rise during the 2000–2010 period" (Guemas et al. 2013), and that "climate skeptics have seized on the temperature *trends* as evidence that global warming has ground to a halt" (Tollefson 2014). These scientific claims can be turned into a precise statistical null hypothesis: the slope in the regression line of global temperature on time is zero during the hiatus period.

We use three methods with increasing levels of generality to test the above hypothesis. Specific details of the methodology are provided in Supplementary Section 3.1. First, beginning with the 1998–2013 period we fit a standard regression to the response variable global temperature on time during 1998–2013, with errors assumed to be independently and identically distributed (see Fig. 1 for the fit). A two-sided hypothesis test yields a p-value of 0.102 (a one-sided test yields a p-value of 0.051). Thus, the claim of a zero warming trend during the hiatus period cannot be rejected at the 5 % significance level. The second method fits a linear regression with autocorrelated errors that follow a parametric autoregressive model with lag 1. This model aims to directly address the year-to-year temporal dependency present in the global temperature record. Estimating the autoregression and regression parameters using the method of Cochrane and Orcutt (1949), a p-value of 0.075 is obtained for the regression slope coefficient by the bootstrap method (with one-sided p-value less than 5 %). Taking temporal dependence into account, there is now more evidence against the null hypothesis of a climate hiatus. The third method is completely nonparametric, and instead of using the parametric AR(1) approach to model the temporal dependency, a block bootstrap is used which allows for quite general forms of temporal dependence, and yields a two-sided p-value of 0.019. There is now compelling evidence to reject the claim of no warming trend during the 1998–2013 period at the 5 % significance level (and even at the 1 % level for a one-sided test). Moreover, the p-values corresponding to starting years 1999 and 2000 are 0.005 and 0.017 respectively, yielding even lower p-values - and stronger evidence against a hiatus - than when using a starting year of 1998. The sensitivity analysis highlights the fact that choosing the year 1998 had a priori favored the hiatus claim. Moreover, assuming the hiatus as the null makes it harder to conclude otherwise. Regardless, the assertion of a climate hiatus is nevertheless rejected at the 5 % level. We therefore conclude that there is "overwhelming evidence" against the claim that there has been no trend in global surface temperature over the past $\approx$ 15 years.

Note also that, in applying progressively more general statistical techniques, the scientific conclusions have progressively strengthened from "not significant," to "significant at the 10 % level," and then to "significant at the 5 % level." It is therefore clear that naive statistical approaches can possibly lead to erroneous scientific conclusions. Methods that rely upon a strong modeling assumption of no temporal dependence, or that of a specific

form, are less reliable than methods that capture dependence without assuming structural knowledge of the type of dependence.

3.2 Hypothesis II: difference in temperature trends

Otto et al. (2013) state that: "the rate of mean global warming has been lower over the past decade than previously." This statement encompasses a second interpretation of the purported hiatus: that the hiatus represents a "slowdown" of global warming (Chen and Tung 2014), in which the rate of warming is less during the hiatus compared with the warming prior to the hiatus (Chen and Tung 2014; Otto et al. 2013; Smith 2013). This claim can be formulated as a testable statistical hypothesis, where the null hypothesis is that the regression slope before the hiatus period minus the regression slope during the hiatus period is zero or negative, versus the alternative hypothesis that this difference is positive.

We employ three different methods with increasing levels of statistical sophistication to test this hypothesis. Specific details of the methodology are provided in Supplementary Section 3.2. First, a standard regression of global temperature on time is fitted to both the 1998–2013 hiatus period and the period 1950–1997, with errors assumed to be independently and identically distributed (see Fig. 2 top left panel). The first method yields a p-value of 0.210. Thus, there is no evidence of a difference in warming trends even at the 10 % significance level. The second method accounts for the temporal dependency in the global temperature record by using a block bootstrap approach, yielding a p-value of 0.323. The evidence for a difference in trends is further weakened when temporal dependency is accounted for. The third approach uses the method of subsampling (Politis et al. 1999; Rajaratnam et al. 2014) to determine how the current 16-year trend during 1998–2013 compares against all the previous 16-year trends observed between 1950 and 1997. A p-value of 0.3939 is obtained and evidence for the hiatus is further weakened. From the plots in Fig. 2 (bottom panel), observe that during the 1950–1997 period, there are several 16-year periods with both higher and lower linear trends. Therefore the observed trend during 1998–2013 does not appear to be anomalous in a historical context.

See Fig. 2 (top right panel) for a summary of results of hypothesis II. Varying the cut-off year from 1998 to either 1999 or 2000 yields p-values of 0.214 and 0.348, respectively, for the bootstrap method. Even after properly accounting for temporal dependence, and undertaking a sensitivity analysis, there is no compelling evidence to suggest that the slopes are significantly different. We therefore conclude that the rate of warming over the past $\approx$ 15 years is not appreciably different from the rate of warming prior to the recent period.

3.3 Hypothesis III: hiatus in the mean global temperature

Some claims have simply asserted that the annual *mean* global temperature has remained constant since 1998 (versus slowing of the *trend* in global warming). For example, Kosaka and Xie (2013) state that "Despite the continued increase in atmospheric greenhouse gas concentrations, the annual-mean global temperature has not risen in the twenty-first century", while Tollefson (2014) states that "Average global temperatures hit a record high in 1998 – and then the *warming stalled*." This claim can also be precisely formulated as a testable statistical hypothesis. The statistical model can be written as $x_t = \mu_t + \varepsilon_t$, where t denotes time (in years), x_t is the 1998–2013 global mean temperature anomalies series, μ_t is the mean parameter and ε_t is the random noise component(with $\mathbb{E}(\varepsilon_t) = 0, \mathbb{V}\text{ar}(\varepsilon_t) =$

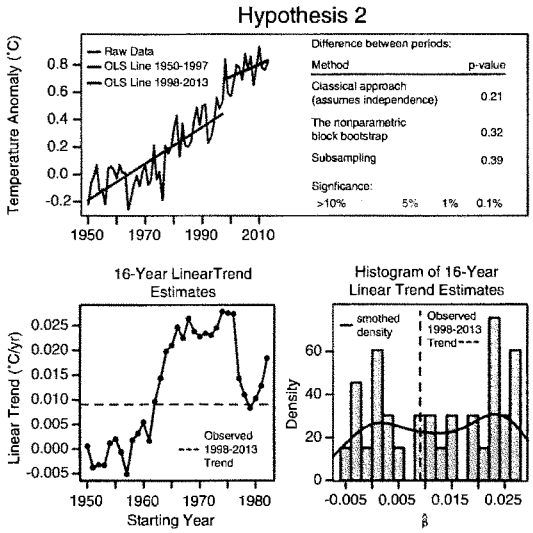
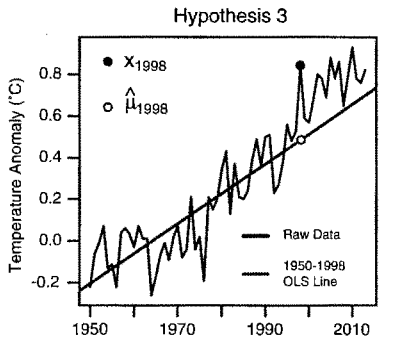


Fig. 2 *Top panel* (*left*) plot of the global mean land-ocean temperature index, from 1950 to 2013, with the base period of 1951–1980. The regression fits for the two time periods (1950–1997 and 1998–2013) are superimposed. *Top panel* (*right*) summary table of results for Hypothesis II *Bottom panel* (*left*) time series plot of 16-year observed trends. *Bottom panel* (*right*) histogram of 16-year observed trends

σ^2). The corresponding null hypothesis and alternative are given as $H_0 : \mathbb{E}(x_{1998}) = \mathbb{E}(x_{1998+t})$ for $t = 1, 2, \cdots, 15$ versus $H_A : \mathbb{E}(x_{1998}) \neq \mathbb{E}(x_{1998+t})$.

Specific details of the methodology are provided in Supplementary Section 3.3. Hypothesis III is tested in four different ways. There are two options for determining the value of $\mathbb{E}[x_{1998}] = \mu_{1998}$: to directly use the observed 1998 temperature record x_{1998} as a substitute for μ_{1998}, or to alternatively estimate μ_{1998} from the regression line from the period 1950–1997. Figure 3 (top panel) illustrates this concept. As the two approaches for specifying μ_{1998} yield fixed values, the inherent variability therein can be explicitly accounted for by using the bootstrap. Doing so propagates the variability in a rigorous manner. The table in Fig. 3 (bottom panel) summarizes the results of testing hypothesis III.

For Method A, when x_{1998} is used as a substitute for μ_{1998}, the statistical test concludes that the mean has decreased during the hiatus, and thus strongly favors the hiatus claim. However, since this one single observed value is not a consistent estimate of μ_{1998}, the conclusion is not reliable. In Method B when μ_{1998} is estimated from the 1950–1997 regression line, the null hypothesis is rejected in the opposite direction, suggesting that the mean temperature has actually increased during the hiatus period. Thus, the selection effect from choosing 1998 as the reference cut-off year has a tremendous impact on the statistical conclusion. Method C, which specifically incorporates the variability inherent in estimating μ_{1998} as x_{1998} leads to a different conclusion than in Method A. In particular, as soon as the variability in estimating μ_{1998} to be x_{1998} is incorporated, one can no longer reject the null hypothesis that the mean has remained constant - even when the high value x_{1998} is used. Method D uses a value for μ_{1998} which is estimated from the 1950–1997 regression and



Hypothesis 3 with 1998 as start of "hiatus" period:

$E(x_{1998})$	Variability of x_{1998}	Result	Remark
x_{1998}	Assume fixed	Reject H_0	decrease in mean
$\hat{\mu}_{1998}$	Assume fixed	Reject H_0	increase in mean
x_{1998}	Simulate by bootstrap	Retain H_0	no change in mean
$\hat{\mu}_{1998}$	Simulate by bootstrap	Reject H_0	increase in mean

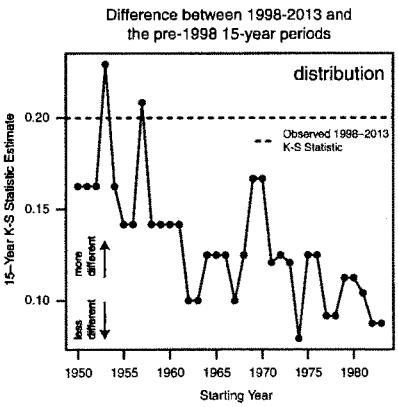
Fig. 3 *Top panel* figure illustrating how the mean μ_{1998} can be estimated. *Bottom panel* summary table of results for Hypothesis III with 1998 as start of hiatus period

also incorporates the variability of this estimate. Here the assertion that the mean is either zero, or has decreased, is rejected.

Given the results of this nuanced analysis, we conclude that claims that the global mean temperature has not changed in recent decades are not supported by evidence. In addition, our nuanced analysis gives much needed rigor to the claim that using 1998 as a reference year amounts to "cherry picking" (Leber 2014; Stover 2014), see also Supplemental Section for detailed discussions). The results are further validated when the analysis is repeated with 1999 and 2000 as the starts of the hiatus period (see Supplemental Section 3.3). Note furthermore that since 2014 was the warmest year on record Karl et al. (2015), ignoring 2014 in our analysis can be viewed as being even more conservative, similar to using 1998 as the starting point.

3.4 Hypothesis IV: difference in year-to-year temperature changes

It is also instructive to extend the analysis above without relying on a linear model to understand trends or means. One such approach is to assess whether the distribution of year-to-year temperature *changes* is markedly different between the hiatus period and the prior periods. Such analysis is inherently less reliant on a statistical model of temperature on time, and hence makes fewer assumptions. The scientific assertion here is that year-to-year changes in global mean temperature during 1998–2013 are different from those during 1950–1997. Under the null hypothesis, these year-to-year changes are assumed to come from a common underlying distribution, though we do not assume that the observations of differences are independent. This framework also allows for testing of specific features of the distribution, including changes in the mean, median and variance. The empirical distribution of annual changes in the global temperature can be constructed by taking first differences: the global mean temperature during a given year is subtracted from the global mean temperature in the previous year. The first differences during 1998–2013 give rise to a 15-year times series of temperature changes. Differences in distribution (using the



Hypothesis 4 using bootstrap and subsampling:

Test	Bootstrap p-value	Subsampling p-value
Difference in distribution	0.295	
Difference in mean	0.362	0.118
Difference in median		< 0.029
Difference in variance	0.496	0.265
Difference in log variance	0.483	—

Signficance: >10% 5% 1% 0.1%

Fig. 4 *Top panel* time series plot of 15-year observed KS differences. *Bottom panel* summary table of results for Hypothesis IV using bootstrap and subsampling

Kolmogorov-Smirnov (K-S) statistic), in means, medians and variances are tested using the block bootstrap and subsampling, thus taking temporal dependency fully into account. Specific details of the methodology are provided in Supplementary Section 3.4.

The results of this analysis are given in Fig. 4. Using either bootstrap or subsampling there is no evidence at the 5 % significance level to suggest that the distribution of changes during the hiatus period is different from the previous period 1950–1997. The same applies to the mean and variance of the distributions, The difference in medians is not statistically significant at the 5 % level using the block bootstrap approach, but is significant when using subsampling. However this difference in medians completely disappears when the starting year of the hiatus is changed to either 1999 or 2000, hence the result is not robust (see Table S8 in Supplemental Section 3.4). Given these results, we conclude that the distribution of annual changes in global temperature has not been different in the past 15 years than earlier in the global temperature record.

3.5 Re-analyzing recently-updated global temperature observations

We have also implemented our methodology on the recently released ERSSTv4 dataset to compare our results to the results obtained in a recent paper by Karl et al. (2015). Unlike the study by Karl et al. (2015), we do not indirectly impose Gaussianity on the temperature data (in the most general approach that we propose for each hypothesis). We also do not impose an autoregressive structure for modeling the temporal dependence. Instead we account for the temporal dependency more flexibly and non-parametrically using the circular block bootstrap and related methods. The increased sophistication allows one to have more confidence in the results' general validity as our approach makes fewer assumptions.

The end result is also compelling. First, the results in Karl et al. (2015) show a positive slope during the hiatus period (Hypothesis I) only at the 10 % significance level. Our analysis shows however that removing the arbitrary and parametric autoregressive structure on the residuals and using the block bootstrap yields significance at the 0.1 % level. The p-value stemming from our approach is less than 0.0005. The implication of the much stronger conclusion is that the warming trend observed during 1998–2014[1] arising from a model of no warming is less than 1 in 2000 (as compared to less than 1 in 20 from Karl et al. (2015)). Thus the conclusion is made stronger by a factor of 100 using the methodology we have developed.

Now consider hypothesis II which compares the warming trend during the hiatus period to that in the previous period (1950–1997). Karl et al. (2015) assert that the analysis on the corrected NOAA global temperature shows that the 90 % confidence interval for the trend in the hiatus period encompasses that of the previous period. Note that this confidence interval is based on the period 1998–2012 and is thus calculated on only 15 years of data. Since the theoretical justification of such confidence intervals is valid for large sample sizes, it is not clear how reliable the conclusion really is. On the other hand, our subsampling methodology for comparing the trends in the two periods is applicable even when the sample size in the hiatus period is small. In particular, the validity of the subsampling approach here does not rely on asymptotic arguments (i.e., increasing sample sizes) during the hiatus period. Details of the analysis are given in Tables S11 and S12 in Supplementary Section 6.

Recall that the analysis by Karl et al. (2015) requires the use of the corrected NOAA dataset to reject the claim of a hiatus. We note that our analysis rejects the hiatus claim even when using the older NOAA temperature dataset (that is, even without correcting for the data biases). The use of methodology with far fewer restrictive assumptions appears to be more robust to errors in the data. This may not be unexpected since biases in the data tend to violate basic parametric assumptions, whereas the less restrictive techniques, such as the ones we develop, can handle a variety of data generating mechanisms simply by their very non-parametric nature.

Note that, by and large, the conclusions reached by Karl et al. (2015) and our conclusions agree. However, it is important to mention that an approach based on stringent or unrealistic assumptions which agrees with our conclusions for this dataset may fail to do so on another dataset.

3.6 Summary

We summarize the overall results from all four hypothesis tests I, II, III and IV in Tables 5 and 6 in Supplementary Section 4. These two tables also analyze the sensitivity of the results to two important factors: first when the cut-off year is changed from 1998 to either 1999 or 2000; and second when the NOAA or HadCRUT4 datasets are used instead of the NASA-GISS dataset. As there are four hypotheses being tested, using a battery of rigorous test procedures, the number of hypothesis being tested are numerous. Hence the issue of multiple hypothesis testing surfaces. In particular, a certain number of these hypotheses are expected to be falsely rejected by chance alone, casting further doubt on any of the hiatus claims.

[1] Though we consider the hiatus period as 1998–2013 elsewhere in the analysis, we consider the hiatus period as 1998–2014 here in order to compare directly with Karl et al. (2015)

Our rigorous statistical framework yields strong evidence against the presence of a global warming hiatus. Accounting for temporal dependence and selection effects rejects - with overwhelming evidence - the hypothesis that there has been no trend in global surface temperature over the past ≈15 years. This analysis also highlights the potential for improper statistical assumptions to yield improper scientific conclusions. Our statistical framework also clearly rejects the hypothesis that the trend in global surface temperature has been smaller over the recent ≈ 15 year period than over the prior period. Further, our framework also rejects the hypothesis that there has been no change in global mean surface temperature over the recent ≈15 years, and the hypothesis that the distribution of annual changes in global surface temperature has been different in the past ≈15 years than earlier in the record. Taken together, these results clearly reject the presence of a hiatus, pause, or slowdown in global warming. In rejecting all four hiatus hypotheses, our results instead demonstrate that the evolution of global surface temperature over the past 1–2 decades is not abnormal or unexpected within the context of the long-term record of variability and change.

Without empirical evidence in support of the hiatus claims, the assumption that there has been a hiatus/pause/slow-down in global warming should be called into question. That being said, recent work investigating the geophysical causes of the recent temperature time series have provided valuable insights into the processes that create decadal-scale variability in global temperature within a long-term trend of global warming. Moreover, it is also useful that errors in data aggregation have been corrected in the recent work of Karl et al. (2015).

Author Contributions B.R. and N.D. conceived the initial scope of the study; B.R. and J.R. conceived details of the overall investigation; B.R. designed the scientific framework. B.R. and J.R. translated the scientific hypothesis into statistical hypothesis and designed the statistical framework; M.T. analyzed the data and implemented the analytical tools; B.R. and J.R. developed and supervised the statistical analyses; B.R. synthesized the paper. All authors discussed the results and commented on the draft. B.R. was partially funded by the US National Science Foundation under grants DMS-CMG 1025465, AGS-1003823, DMS-1106642, DMS-CAREER-1352656 and the US Air Force Office of Scientific Research grant award FA9550-13-1-0043. J.R. was partially funded by the US National Science Foundation under grants DMS-1307973 and DMS-1007732. N.S.D. was partially funded by NSF-CAREER-0955283.

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THE "PAUSE" IN GLOBAL WARMING

Turning a Routine Fluctuation into a Problem for Science

BY STEPHAN LEWANDOWSKY, JAMES S. RISBEY, AND NAOMI ORESKES



Many indicators confirm that Earth continues to warm from greenhouse gases (Abraham et al. 2013; Balmaseda et al. 2013; Durack et al. 2014). Notwithstanding, climate contrarians have been claiming for nearly a decade that global warming has "stopped" (Carter 2006). Boykoff (2014) showed how, over time, those repeated contrarian claims entered the discourse in the media and among policy makers and politicians. In consequence, climate change has frequently been framed around the presumed fact that global warming—measured by global mean surface temperature (GMST)—has "stalled," "stopped," "paused," or entered a "hiatus." Evidence for the widespread adoption of this frame is provided by a *Google Trends* analysis (conducted on 21 October 2014), which reveals that the search term "global warming stopped" has been used nearly continuously since February 2008, with distinct spikes ahead of the climate meetings in Copenhagen, Denmark (December 2009), and Doha, Qatar (November 2012).



This frame has also found explicit uptake in the peer-reviewed literature, with two special issues of *Nature* journals devoted to the "pause" or "hiatus" in early 2014, and a total of more than 40 articles having appeared in print on the pause by 2014. Moreover, the Intergovernmental Panel on Climate Change (IPCC), which reflects the scientific consensus on climate change, adopted the term hiatus in its Fifth Assessment Report, and even gave it a definition "as the reduction in GMST trend during 1998–2012 as compared to the trend during 1951–2012" (IPCC 2013, Box TS.3).

Is there a meaningful pause or hiatus in global warming? If not, what has caused the scientific community to devote such intense activity to analyzing something that does not exist? This article presents

evidence that there has been no meaningful pause in global warming and offers an account of why this notion has become so widespread in the scientific community.

There has been ongoing analysis and commentary arguing against the existence of a statistically meaningful pause for several years (e.g., Foster and Rahmstorf 2011). Two analyses of the GMST time series have failed to find any statistical evidence for a slowdown (Foster and Abraham 2015), or a distinct changepoint in the rate of warming (Cahill et al. 2015). There have also been questions about biases in some datasets used to identify a potential pause (Cowtan and Way 2014; Karl et al. 2015). Most recently, a bias-corrected release of the National Oceanic and Atmospheric Administration's (NOAA) National Centers for Environmental Information (NCEI) dataset (Karl et al. 2015) assessed the rate of warming during the hiatus period identified by the Intergovernmental Panel on Climate Change (IPCC; 1998–2012) to differ little from the longer-term trend considered by the IPCC for comparison (1951–2012). Although those bias corrections were unavailable at the time when the pause gained entry into the literature, we show below that our conclusions do not depend on those corrections.

Accordingly, there are other indications of longstanding disquiet with the presumed pause. For example, the IPCC's use of the term hiatus (without scare quotes) came under critical scrutiny during review of the Fifth Assessment Report. In a high-priority comment on the Summary for Policy Makers, the German government noted that the term hiatus was strongly misleading and recommended against its use.[1] Although the German delegation's suggestion was not adopted, it points to a fundamental problem surrounding the pause: what exactly is meant by a pause or hiatus?

[1] The full comment reads as follows: "the underlying report and the TS label the recent reduction in surface warming as 'hiatus'. The web site http://thesaurus.com gives as definition of this expression 'pause, interruption', www.merriam-webster.com gives '1a: a break in or as if in a material object, 2a: an interruption in time or continuity; break; especially: a period when something (as a program or activity) is suspended or interrupted.' All these definitions do not appropriately describe the recent temperature evolution as there is not a pause or interruption, but a decrease in the warming trend, and the first decade of this century has been the warmest since preindustrial times, see Figure SPM1. (a), lower figure. Hence, the expression 'hiatus' is strongly misleading and should not be used throughout the report" (www.climatechange2013.org/images/report/WGIAR5_FGD_FinalDraftSPMComments.pdf).

WHAT IS A PAUSE? By definition, a "pause" involves the interruption or suspension of a process. The presence of a pause or hiatus in global warming would thus mean what contrarians say it means (e.g., Carter 2006), namely, that warming had stopped, at least for a time. Determining whether warming has stopped is nontrivial because greenhouse-driven global warming is expressed on multidecadal and longer time scales (i.e., 30 yr and longer), whereas on shorter time scales (10–20 yr) the rate of warming speeds up and slows down relative to the longer-term average trend (IPCC 1996; Risbey 2015). At one point or another, there may therefore be periods of limited duration during which surface temperatures do not increase significantly.

In this article, we consider the period since 1970 to provide a representation of the "longer term" rate of greenhouse warming that is characteristic of the modern period. The choice of period marking the longer-term trend is necessarily somewhat arbitrary. The year 1970 has been statistically identified as an approximate marker of an upsurge in the rate of global warming on multidecadal time scales (Cahill et al. 2015). This longer-term trend (1970–2014) has been estimated at 0.17 K decade^{-1} (Cowtan and Way 2014; Karl et al. 2015) or 0.16 K decade^{-1} [National Aeronautics and Space Administration (NASA) Goddard Institute for Space Studies Surface (GISS) Temperature Analysis (GISTEMP; Hansen et al. 2010) and the Met Office's Hadley Centre/Climatic Research Unit, version 4 (HadCRUT4; Morice et al. 2012)].

By contrast, we refer to decadal scale (10–20 yr) variations in the rate of warming about a longer-term trend as *fluctuations*. Here, we focus on 15-yr trends to reflect the duration of the presumed hiatus employed by the IPCC (2013, Box TS.3). Those fluctuations may be driven by internal variability (ocean circulation and its coupling to the atmosphere), or they may involve variations in external forcings of the climate system (such as solar irradiance and aerosol concentrations), or both. These fluctuations are "routine" in the sense that they occur commonly and are caused by different combinations of the same set of processes.

A given fluctuation is defined by a start year and an end year, and its magnitude is highly dependent on the choice of start and end years. For example, the decadal rate of warming during the 15 yr centered around 2005 was 0.11 K; for the 15 yr centered on 1999 it was 0.31 K—a nearly threefold difference resulting from a shift of the temporal window by 6 yr [data from Cowtan and Way (2014)]. Fluctuations can therefore display warming rates that are greater than or less than the greenhouse-driven longer-term

trend. There may also be short-term periods of cooling embedded within a longer-term warming trend (Easterling and Wehner 2009).

Any claims of a pause or hiatus in the recent rate of warming must therefore be assessed against the overall pattern of fluctuations in the temperature record. A claim to find a pause or hiatus is a different assertion than a fluctuation and implies that the fluctuation is extraordinary in a particular way: the meaning of the terms pause and hiatus implies that the normal fluctuations in warming rate have been surpassed such that warming has stopped. We next show that no such stoppage has occurred.

GLOBAL WARMING CONTINUES: THE STATISTICAL EVIDENCE. Detailed analyses of temperature trends have been reported previously (Easterling and Wehner 2009; Santer et al. 2011; Karl et al. 2015). Here, we generalize and update those results. The top panel in Fig. 1 shows all possible 15-yr trends in GMST for the period 1970–2014 (i.e., 1970–84, 1971–85, and so on; $N = 31$) for four different datasets. It is clear that the short-term trend sometimes falls above the longer-term trend (indicated by the gray band) and sometimes below it. It is also clear that warming has continued throughout the 45 yr as none of the trends are zero (dashed horizontal line).

The linear trend in GMST (established by ordinary least squares on annual global means) is statistically significant for the last 15-yr period (ending in 2014) for three of the four available datasets: GISS (trend, $b = 0.08$ K decade^{-1}; test statistic, $t = 2.20$; level of significance, $p < 0.05$), the dataset of Cowtan and Way ($b = 0.10$ K decade^{-1}, $t = 2.41$, $p < 0.05$), and the most recent NOAA dataset by Karl et al. ($b = 0.11$ K decade^{-1}, $t = 3.25$, $p < 0.007$). Only HadCRUT4, which does not cover parts of the Arctic where warming is known to be most rapid, fails to yield a significant trend for this 15-yr period ($b = 0.07$ K decade^{-1}, $t = 1.70$, $p > 0.10$). When a further year is included in the analysis, HadCRUT4, too, yields a significant trend ($b = 0.09$ K decade^{-1}, $t = 2.48$, $p < 0.03$).

Although the most recent 15-yr trend is significant for most datasets, there have been six occasions since 1970 when a 15-yr trend would have failed to reach significance (using GISS); namely, in the years 1986, 1993, 1994, 1995, 2011, and 2012. At all those times, the preceding 15 yr failed to show significant warming. And at all those times, the inclusion of further years renders the trend significant. The claim that global warming uniquely "stopped" during any recent 15-yr period is therefore not sustainable. Conversely, any argument about a pause, hiatus, or stoppage could have been made with equal justification (or lack thereof) repeatedly during the past 45 yr.

Nor does the most recent fluctuation constitute a uniquely large deviation from the longer-term trend.

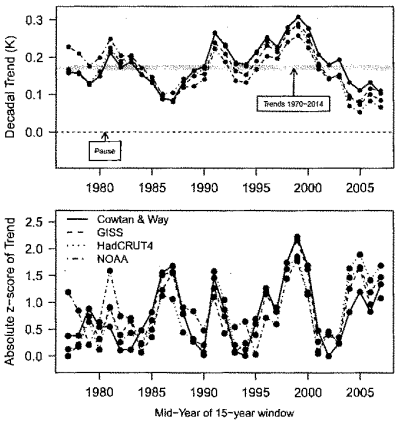


FIG. 1. Summary of all possible 15-yr trends in GMST between 1970 and 2014 inclusive. (top) The trend (K decade^{-1}) for the 15-yr window centered on the plotted year for four datasets: NASA's GISS (Hansen et al. 2010; http://data.giss.nasa.gov/gistemp/**, accessed 17 Jan 2015), the Met Office's HadCRUT4 (Morice et al. 2012;** www.metoffice.gov.uk/hadobs/hadcrut4/data/current/time_series/HadCRUT.4.3.0.0.annual_ns_avg.txt**, accessed 2 Feb 2015), the coverage-bias-corrected version of HadCRUT4 reported by Cowtan and Way (2014) (**http://www-users.york.ac.uk/~kdc3/papers/coverage2013/series.html**, accessed 2 Feb 2015), and the latest NOAA dataset (Karl et al. 2015;** www.ncdc.noaa.gov/cag/time-series/global/globe/land_ocean/ytd/12/1880-2014.csv**, accessed 12 Aug 2015). The GISS dataset is based on sea surface temperature data [Extended Reconstructed SST version 3b (ERSSTv3b)]. The decadal temperature increase is greater than zero (dashed horizontal line) in all datasets at all times. The gray horizontal band represents the average of the trends between 1970 and 2014 across the four datasets. The longer-term trend is represented as a band to capture some of the uncertainty from dataset to dataset, but also to indicate that this is an inherently imprecise quantity because it varies with the exact period that is chosen to represent a longer-term trend. (bottom) The same data as in the top panel, but 15-yr trends are converted into absolute z scores, by expressing each observed trend as the absolute difference in standard deviation units from the mean of all trends since 1970. Originally positive z scores (representing greater than average warming) are plotted in red, and originally negative z scores are shown in blue.**

This is shown in the bottom panel of Fig. 1, which plots the same 15-yr trends but converted into absolute z scores. The advantage of z scores is that they reexpress each data point as a deviation from the overall mean of a sample in units of standard deviation, thereby providing an indication of the extremity of the observations. To compute z scores, the mean of all possible trends was first subtracted from each individual trend, and each such difference was in turn divided by the standard deviation of all trends. To permit a comparison of decelerating ($z < 0$) and accelerating ($z > 0$) fluctuations, the z scores were converted to absolute values for plotting. For clarity, z scores that were originally negative are plotted in blue in Fig. 1, and those that were originally positive are shown in red.

For a pause to be distinctive, it must deviate below the longer-term trend more than previous periods deviated *above* the longer-term trend; otherwise, it can be considered to be just a fluctuation like others observed in the past. The bottom panel in Fig. 1 shows that this criterion for distinctiveness is not met: for all datasets bar HadCRUT4, the pause is less anomalous than the accelerated period of warming that took place during the 15 yr spanning 1999 (i.e., 1992–2006). That is, the absolute magnitudes of the z scores associated with the recent deceleration (whichever recent year is picked as the point on which the pause is centered) are consistently smaller—sometimes by a considerable margin—than those for the 1999 acceleration. Only for HadCRUT4, and only for the 15-yr period centered on 2005, are the z scores for the pause and the maximum warming virtually indistinguishable (1.86 vs −1.90).

Taken together, the statistical evidence presented here and elsewhere (Cahill et al. 2015; Foster and Abraham 2015) shows that the pause period is comparable in statistical terms with other recent fluctuations. Any exceedance of the z score of the pause period compared to other fluctuations, if it exists, is marginal and depends on the details of which dataset is used and precisely what time window is used to assess the pause. The pause is not unusual or extraordinary relative to other fluctuations and it does not stand out in any meaningful statistical sense.

Note that these conclusions are not dependent on the choice of baselines used to represent longer-term greenhouse warming. For example, a longer baseline such as the IPCC's 1951–2012 period yields a lower longer-term trend, thus rendering any fluctuations with slower rates of warming even less unusual. Our conclusions are also qualitatively unaffected by the modeling of autocorrelations and by the choice of window size for the short-term trend.

We next show that experts fail to detect evidence for a pause in a blind test.

GLOBAL WARMING CONTINUES: THE BLIND EXPERT TEST. The forecasting of time series data is central not only to climatology, but also to economics, finance, and allied disciplines. Forecasting techniques have therefore attracted considerable research attention, and the last 25 years have seen a striking reevaluation of the role of human judgment in forecasting. Whereas human judgment used to be given little if any credence in forecasting, today it is "recognised as an indispensable component of forecasting" (Lawrence et al. 2006, p. 493).

People are known to be able to learn smooth functions with considerable precision (DeLosh et al. 1997; Lewandowsky et al. 2002). People are also able to extract information from noisy data presented in graphical form (Lewandowsky and Spence 1989). In forecasting studies, participants across a broad range of expertise are now generally thought to perform well (Harvey and Bolger 1996; Harvey et al. 1997; Du and Budescu 2007), and domain experts outperform statistical models in some circumstances (Forrest et al. 2005), although this is becoming increasingly less common in weather forecasting (Baars and Mass 2005).

Here, we are interested in human forecasting not because people's predictions might constitute a viable alternative to the projections of climate models, but because forecasting judgments reveal people's perceptions of the trend in a dataset. People's extrapolations of visually presented temperature data can therefore reveal whether people believe that global warming has stopped.

To assess the claim that global warming has indeed stopped, Lewandowsky (2011) presented naïve participants with a graph of the historical temperature record, which either identified the data as global temperatures or as a fictitious share price. Figure 2 shows the results of Lewandowsky (2011) for the condition in which the data were identified as global temperatures. Respondents clearly did not perceive a pause or hiatus in the GMST data,[2] as revealed by the fact that their extrapolations (large squared plotting symbols) had a statistically significant positive slope. Extrapolations did not differ notably from a condition (not shown in the figure) in which the stimulus data were presented as fictitious share prices. In the eyes of naïve observers, therefore, global warming has not stopped but is

[2] It must be noted that at the time of the study, the time series ended in 2009. However, at that time the idea of a pause had already been established in contrarian discourse.

set to continue. People's extrapolations were, however, conservative, falling consistently below the linear extrapolation of the long-term trend. The tendency to underestimate a long-term trend is a well-established phenomenon in judgmental forecasting known as trend damping (Harvey and Bolger 1996). This observation merits further exploration because it raises the possibility that people are overly sensitive to any slowing in warming.

This possibility was explored in a blind test involving professional economists, who were asked specifically to comment on the presence of a pause or hiatus in GMST. The sample of economists (N = 25) was tested online and was recruited by a survey firm (Qualtrics.com). All experts held at least a master's degree or a Ph.D. in economics or an allied discipline, with all but four experts reporting five or more years of professional experience. Participants were shown the GMST data through 2010, but presented as "world agricultural output" (see Fig. 3). The graph was accompanied by the following statement that experts had to evaluate in light of the plotted data: "A prominent Australian critic of conventional economics, Mr. X., publicly stated in 2006, that 'There IS a problem with the growth in world agricultural output—it stopped in 1998.' A few months ago, Mr. X. reiterated that '...there's no trend, 2010 is not significantly more productive in any way than 1998.' " This statement is an exact translation, into the economic terms of world agricultural output, of a series of public statements about the putative pause or stoppage of global warming (Carter 2006, 2011).

The experts responded to six test items, which are shown in Table 1.[3] Table 1 also shows the responses of the experts on a six-point scale that ranged from "strongly disagree" (1) to "strongly agree" (6). Any mean response above 3.5 therefore represents agreement, and any mean response below 3.5 indicates disagreement, respectively, with the test item (there was no "neutral" response category). It is clear that the experts disagreed with the invocation of a pause: experts rejected the idea that the data confirm the statement and instead find that the data contradict the statement. The experts also found the statement to be misleading and ill-informed. The experts were divided on whether or not the statement is fraudulent, although nearly ⅔ of them endorsed that possibility as well. The experts were also divided on whether the statement might be compatible with the data in a "narrow sense."

[3] The experiment involved additional statements by contrarians, pertaining to other climate variables, such as glaciers and Arctic ice, that are not relevant to the present article and are not reported here.

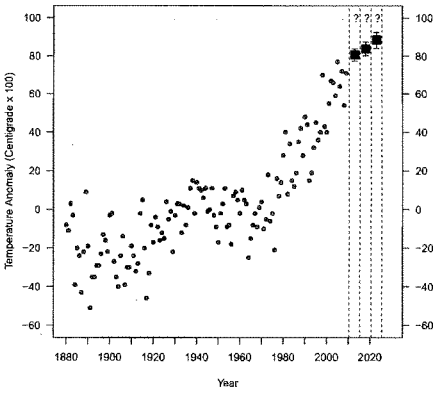


FIG. 2. Stimuli and data from an experiment by Lewandowsky (2011). Gray circles show actual global mean land surface air temperature anomalies from 1880 to 2009. Extrapolations of the trend by the respondents are represented by large red squares. When the graph was presented as a stimulus, the three questions marks (?) at the top identified the three columns in which participants marked their predictions. Temperature data are from the GISTEMP (http://data.giss.nasa.gov/gistemp/, accessed 4 Feb 2010; see also Hansen et al. 2010).

These results from our experiment are consistent with an earlier informal study conducted by the Associated Press with a small sample of statisticians who were blind to the data source (Borenstein 2009). Those experts, too, saw no evidence for a decline in the temperature trend and instead decried the cherry-picking of observations on which that claim was based.

In summary, in two blind tests, experts and novice observers alike consider the evidence of continued global warming to be clear. By contrast, statements endorsing the pause were identified by experts in forecasting and time series analysis to be misleading and at odds with the data.

WHERE DID THE PAUSE COME FROM? Our preceding analyses show that the entrenchment of the pause concept in the literature is incommensurate with the lack of evidence supporting it, and that it does not pass a blind expert test. Despite that, large segments of the climate science community, including the IPCC (2013, Box TS.3), have adopted the notion of a pause or hiatus in global warming.

This is not to say that interpretations of the pause are entirely uniform. A few articles addressing the pause question its existence. For example, Seneviratne

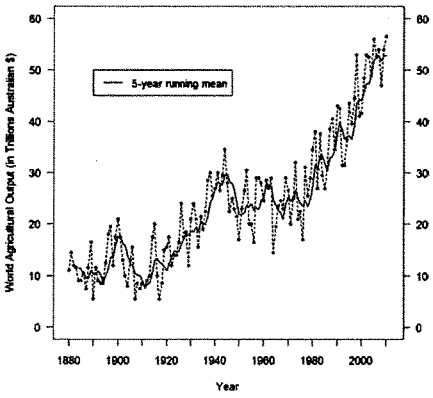


FIG. 3. Stimulus data shown to expert economists in a blind test of contrarian statements invoking the pause. See text for details. Data are actually global land–sea surface temperature anomalies from the GISTEMP dataset (http://data.giss.nasa.gov/gistemp/, accessed 3 Mar 2011; see also Hansen et al. 2010).

et al. (2014) call the term misleading and conclude that "not only is there no pause in the evolution of the warmest daily extremes over land but...they have continued unabated over the observational record" (p. 163). Risbey et al. (2014) show that recent fluctuations are not unusual and do not constitute meaningful evidence against climate model projections. Santer et al. (2014) refer to the pause or hiatus in quotation marks (i.e., scare quotes), thereby implying skepticism or disagreement with the phrase. However, the majority of the more than 40 articles on the pause that we know of start from the premise that the pause is meaningful, and present it as a significant development requiring explanation. Moreover, some researchers (albeit a minority) have taken the pause to imply that the climate system may be less sensitive to greenhouse gas emissions than previously thought (Otto et al. 2013; Curry 2014). But any use of the term—except in a clearly refutative context—is problematic because it reinforces, both in scientific and public debate, the belief that there has been a statistically meaningful cessation of warming when there has not.

How did this occur? We have shown in detail elsewhere (Lewandowsky et al. 2015) that there are several psychological and cognitive reasons why climate scientists may have been susceptible to the meme of a pause that demonstrably originated in contrarian discourse on the Internet and in the media (Boykoff 2014). Here, we suggest that a contrarian meme can find entry into the scientific community simply by exploiting scientists' commitment to explanation and to responding to intellectual challenges. Scientists generally strive to emphasize factual information and deemphasize value judgments. Indeed, "disinterestedness" has long been identified as one of the core norms of science (Merton 1942).

In a world in which contrarian claims in the media and other public arenas are overrepresented (Boykoff and Boykoff 2004; Elsasser and Dunlap 2013; Boykoff 2013), scientists may feel the need to respond to these claims. This may occur informally, as when friends, neighbors, or family members ask questions about contrarian claims they encountered online, or formally, when journalists, editors, or policy makers seek answers to contrarian talking points. If these encounters involve loaded questions, such as "What about the 'pause' in warming?," then climate scientists may inadvertently accept the biasing terms in which those questions are framed.

Frames are rhetorical and communicative structures that select and highlight certain aspects of a perceived reality over others (Dirikx and Gelders 2010). Because frames are rarely made explicit—for example, few people know that the use of the term "climate change" rather than "global warming" was advocated by Republican strategist Frank Luntz (Mooney 2005; Lakoff 2010)—frames can shape in a hidden manner the way in which people discuss an issue (de Boer et al. 2010). Would voters be more likely to support a price on carbon if it were framed as an "additional tax burden," "insurance premium for your grandchildren's well-being," or "putting a fair price on the true cost of oil and gas?" Even simple choices of wording, such as "tax" versus "offset" can have large effects on people's endorsement of policy options (Hardisty et al. 2010).

Simply by being exposed to the pause meme for over a decade, and by explaining short-term fluctuations from a longer-term trend in the terms posed to them, scientists have accepted a contrarian frame, and this acceptance may in turn have subtly changed scientists' way of thinking (Lewandowsky et al. 2015).

To illustrate, we provide citations from some recent articles on the pause in Table 2. None of those articles questioned the fundamental fact that Earth is warming from greenhouse gas emissions, and some authors even underscored the likelihood of future warming, for example by suggesting that the "present hiatus will be short-lived" with "rapid warming set to

Table 1. Test items and responses given by expert economists to contrarian statements endorsing the pause that were evaluated in light of the data.

Test item	Agreement[a]	Mean[b]	t[c]	p[d]
The data confirm the claim made by Mr. X.	0.36	2.84	−2.72	<0.02
The data contradict the claim made by Mr. X.	0.68	4.12	2.58	<0.02
The claim made about the data by Mr. X is misleading.	0.76	4.28	3.67	<0.002
The claim made about the data by Mr. X is ill-informed.	0.76	4.04	2.38	<0.03
If incompetence is ruled out, the claim made about the data by Mr. X is fraudulent.	0.64	3.84	1.49	n.s.
The statement by Mr. X is compatible with the data in a narrow sense, but the data do not support the implication of his statement, which is that world agricultural output is no longer growing.	0.52	3.60	0.34	n.s.

[a] Proportion of experts out of 25 who agreed (rating > 3) with the test item.

[b] Mean response on the six-point scale. Any value > 3.5 represents agreement.

[c] Single-sample t statistic ($df = 24$) comparing the mean response to the null hypothesis that the mean is equal to 3.5 (neutrality on the six-point scale).

[d] The p value of the t test in the previous column: n.s. means nonsignificant.

resume" once the present decadal variation comes to an end (England et al. 2014, p. 225). Nonetheless, the majority of articles accepted the framing of a pause and sought to explain its cause. Furthermore, the citations in Table 2—typically from the opening paragraph of an article—show that authors often framed the article by juxtaposing the continuing increase of atmospheric CO_2 levels with the presumed lack of warming on a decadal scale as though this presented a notable scientific problem at odds with expectations from greenhouse theory.

The statements in Table 2—and similar but often tacit implications of many other articles—are at variance with long-established knowledge that multidecadal natural variations in climate are superimposed on a longer-term CO_2 warming trend. These variations demonstrate that whereas CO_2 may increase year after year, surface temperature need not. More than 20 years ago, the IPCC's Second Assessment Report pointed to the importance of decadal and longer time-scale variability (IPCC 1996, 329–330), as did a U.S. National Research Council report (Martinson 1995). The IPCC summary for policy makers in the 1995 report cautioned that future decadal-scale changes would include considerable natural variability despite the longer-term warming.

If this knowledge had been foremost on scientists' minds, rather than the contrarian pause meme, the framing of many recent research articles arguably would have been different. Instead of opening an article with "Despite ongoing increases in atmospheric greenhouse gases, the Earth's global average surface air temperature has remained more or less steady since 2001," we suggest that scientists might have adopted a more appropriate framing such as "It has long been known that the longer-term greenhouse warming trend is punctuated with decadal and longer fluctuations. In this article we show that the most recent fluctuation during which warming fell below the longer-term trend was due to..."

THE MERITS OF RESEARCH ON THE PAUSE. The body of work on fluctuations in warming rate has clearly contributed to our understanding of decadal variations in climate. For example, studies have shown that the negative radiative forcing from stratospheric loadings of volcanic aerosol has increased in recent years and is larger than previously thought (Solomon et al. 2011; Neely et al. 2013; Ridley et al. 2014; Santer et al. 2014). Research has also highlighted processes whereby the ocean can vary the rate at which heat is taken up from the surface (Kosaka and Xie 2013; England et al. 2014).

Research on decadal fluctuations has also highlighted differences in expectations between climate projections that tend to average out decadal variations and the actual transient response of the climate system (Schneider and Thompson 1981) that includes such variation. Research has shown that differences in expectation between averages of projections and the actual transient response are related to model-versus-observed differences in the phasing of internal variability (Meehl and Teng 2014; Risbey et al. 2014), systematic errors in some of the external forcings used

TABLE 2. Representative quotations from peer-reviewed articles that frame the pause or hiatus as a problem for climate science.

Quotation	Source
"Reconstructions of global mean surface temperature [Hansen et al. 2010; Morice et al. 2012] show rising values after the 1960s but a slowing of the warming in the 2000s, even though atmospheric greenhouse gas concentrations continued to increase. This hiatus in warming may have been exaggerated by sampling errors [Cowtan and Way 2014], but a significant slowdown is evident."	(Drijfhout et al. 2014, p. 7868)
"Despite ongoing increases in atmospheric greenhouse gases, the Earth's global average surface air temperature has remained more or less steady since 2001."	(England et al. 2014, p. 222)
"The warming of the climate system is unequivocal as evidenced by an increase in global temperatures by 0.8°C over the past century. However, the attribution of the observed warming to human activities remains less clear, particularly because of the apparent slow-down in warming since the late 1990s."	(Estrada et al. 2013, p. 1050)
"Despite a sustained production of anthropogenic greenhouse gases, the Earth's mean near-surface temperature paused its rise during the 2000–2010 period."	(Guemas et al. 2013, p. 649)
"Given the widely noted increase in the warming effects of rising greenhouse gas concentrations, it has been unclear why global surface temperatures did not rise between 1998 and 2008."	(Kaufmann et al. 2011, p. 11,790)
"Despite the continued increase in atmospheric greenhouse gas concentrations, the annual-mean global temperature has not risen in the twenty-first century, challenging the prevailing view that anthropogenic forcing causes climate warming."	(Kosaka and Xie 2013, p. 403)
"Despite continued growth in atmospheric levels of greenhouse gases, global mean surface and tropospheric temperatures have shown slower warming since 1998 than previously."	(Santer et al. 2014, p. 185)
"Despite increasing radiative forcing, the observed globally averaged annual mean surface temperature (Tmean) has only increased very slowly since the late 1990s (e.g., IPCC AR5 2013)."	(Sillmann et al. 2014, p. 1)

in Coupled Model Intercomparison Project phase 5 (CMIP5) simulations (Fyfe et al. 2013; Schmidt et al. 2014), incomplete coverage and quality of observations (Karl et al. 2015), and use of incommensurate measures between models and observations (Cowtan et al. 2015).

In addition, the statistical properties of many different examples of decelerating fluctuations are very similar in observations and in models (Risbey et al. 2014; England et al. 2015; Marotzke and Forster 2015). Other research has highlighted that there will be similar fluctuations (in both directions; faster as well as slower warming) in the future, a point about which policy makers perhaps need to be reminded (Easterling and Wehner 2009; Hawkins et al. 2014; England et al. 2015).

Research on the pause has thus ultimately reaffirmed the overall reliability of climate models for projecting temperature trends. However, by accepting the framing of a recent fluctuation as a pause or hiatus, that research has, ironically and unwittingly, entrenched the notion of a pause (with all the connotations of that term) in the literature as well as in the public's mind.

PUTTING THE PAUSE TO FUTURE "PAUSES." To avoid misframing in the future does not mean that scientists should necessarily avoid an issue simply because it has gathered public prominence or is being used by contrarians. Scientists have previously responded to contrarian memes with success, for example by showing that appeals to the sun or galactic cosmic rays fail to explain global warming (Benestad 2013; Sloan and Wolfendale 2013). Concerning the recent fluctuation, we have shown that its framing as a pause or hiatus that constitutes a problem for greenhouse warming is incorrect, because it is not meaningfully different from other fluctuations in warming rate. If the fluctuation were instead framed as an instance of decadal variation, then scientists would be able to put the pause to misleading contrarian claims that global warming has stopped.

It bears remembering that the point of contrarian memes is to "keep the controversy alive" (Oreskes and Conway 2010). Accepting contrarian linguistic frames helps maintain the fiction that the science is still too uncertain to form a reliable basis for public policy. Moreover, it should be noted that the remaining uncertainties often provide a greater, rather than lesser, impetus for mitigation (Lewandowsky et al. 2014a,b).

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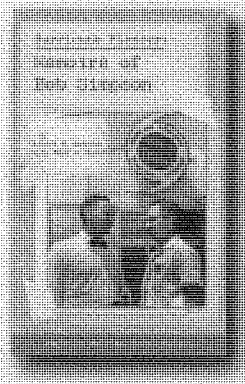
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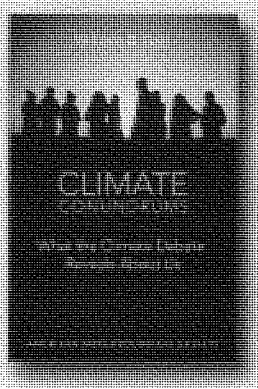
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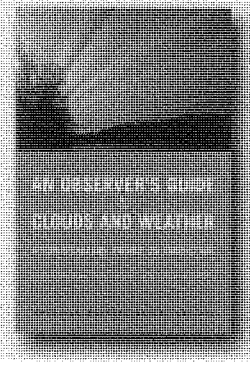


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William R. Cotton

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THE HILL

Drowning by EPA overreach

BY WILL COGGIN - 06/17/15 05:30 PM EDT 4

The Environmental Protection Agency (EPA) recently found itself in hot water. *The New York Times* revealed the agency colluded with environmentalist groups in a campaign to manufacture public comments in favor of a new rule that expands its own power. The agency's actions and the shenanigans of its environmentalist supporters shed light on how a bad rule can flow through the regulatory process.

The Waters of the United States (WOTUS) rule extends the reach of EPA to regulate ponds, ditches and even large puddles under the Clean Water Act (CWA). That's bad news for farmers, ranchers, small businesses or anyone else who wants to use land under CWA jurisdiction: It costs an average of $270,000 to obtain the special permit required to do so, according to the National Federation of Independent Businesses.

The downsides are clear, and the EPA's judgment was murky even before the rule. Last year, the agency threatened to fine a Wyoming man $75,000 a day for building a pond on his own property without a permit.
Almost immediately after its proposal, the rule prompted a wide opposition urging the EPA to "ditch the rule," from small businesses, farmers and ranchers, energy producers and others.

The EPA needed support for its water grab. While the EPA failed to consult with those harmed by the WOTUS rule, documents obtained by *The New York Times* show the EPA worked with environmentalist groups including the Sierra

Club and National Resources Defense Council to manufacture public comments in its favor.

EPA Administrator Gina McCarthy later testified at a Senate subcommittee hearing that 87 percent of the approximately 1 million public comments her agency received were supportive. By omitting mention of the efforts (or money spent) to solicit the comments, McCarthy attempted to make it look like there was a spontaneous groundswell of support for her rule.

And that wasn't the only subterfuge behind the EPA's power grab.

A number of left-wing groups camouflaged as sportsmen-friendly organizations, including the Theodore Roosevelt Conservation Partnership (TRCP), Backcountry Hunters and Anglers (BHA) and Trout Unlimited, were also helping the EPA to foist the water rule onto an unsuspecting public.

In July 2014, TRCP called for "broad public involvement," setting the table for the EPA's campaign to gather public comment in support. This despite the fact that the organization's support had already been touted by the EPA in an effort to make it look like a broad coalition was in favor.

These groups claim to represent sportsmen's interests—giving the rule seemingly conservative support—but they are tangled in a web of money from left-wing foundations with anti-gun and anti-agriculture agendas. BHA gets most of its donations from three environmental groups, according to tax records, while TRCP gets its money from a handful of Big Labor and Big Green groups. Trout Unlimited, meanwhile, has taken tens of millions from fringe environmental groups.

A bipartisan bill to send the rule back to the EPA's drawing board has already passed the House and a similar measure introduced by Sen. John Barrasso (R-Wyo.) has gained cosponsors from both sides of the aisle in the Senate. In response BHA labeled the congressional effort "un-American."

Meanwhile, attorneys general in three states have said that state challenges to the rule are likely.

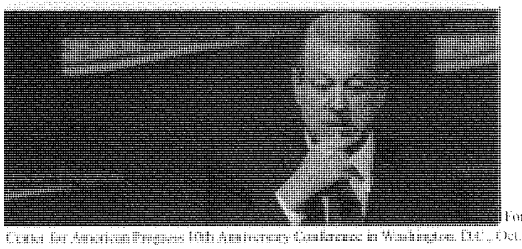
And the EPA may have violated federal law that prohibits using appropriated funds for lobbying in creating the comments. Sen. Pat Roberts (R-Kan.) called it "a political grassroots lobbying campaign with environmental

groups to manipulate the process and disregard legitimate concerns from rural America."

The EPA's brazenness in ramming through a rule with camouflaged and concocted support is concerning even for a town where there's no shortage of dirty tricks. Congress would be wise to wash away the water rule before it does lasting damage.

Coggin is the director of research at the Environmental Policy Alliance, a project of the non-profit Center for Organizational Research and Education. CORE is supported by a wide variety of businesses and foundations, including those in the hospitality, agriculture, and energy industries.

THE DAILY CALLER



Former Vice President Al Gore speaks during the Center for American Progress 10th Anniversary Conference in Washington, D.C., Oct. 24, 2013. (JIM WATSON/AFP/Getty Images)

Former UN Lead Author: Global Warming Caused By 'Natural Variations' In Climate

MICHAEL BASTASCH

Global temperature change observed over the last hundred years or so is well within the natural variability of the last 8,000 years, according to a new paper by a former Intergovernmental Panel On Climate Change (IPCC) lead author.

Dr. Philip Lloyd, a South Africa-based physicist and climate researcher, examined ice core-based temperature data going back 8,000 years to gain perspective on the magnitude of global temperature changes over the 20th Century.

What Lloyd found was that the standard deviation of the temperature over the last 8,000 years was about 0.98 degrees Celsius– higher than the 0.85 degrees climate scientists say the world has warmed over the last century.

"This suggests that while some portion of the temperature change observed in the 20th century was probably caused by greenhouse gases, there is a strong likelihood that the major portion was due to natural variations," Lloyd wrote in his study.

The United Nations' IPCC claims there's been 0.85 degrees Celsius of warming since the late 1800s, and concludes that most of this warming is due to human activities– mainly, the burning of fossil fuels and changes in land use. The IPCC says that "more than half of the observed increase in global average surface temperature from 1951 to 2010" have been caused by human activity.

If Lloyd's results hold, the IPCC may have to revise how much warming it attributes to mankind. In any case, the IPCC's estimate of man-made and natural warming (0.85 degrees) is still below

the standard deviation for the last 8,000, according to Lloyd's results. This means that warming is not very significant within the context of the Earth's recent climate history.

Lloyd arrived at his conclusion after the "differences in temperatures between all records which are approximately a century apart were determined, after any trends in the data had been removed." Lloyd noted the "differences were close to normally distributed."

But Lloyd's study hits at a larger debate within climate science: how much warming is attributable to mankind or nature. Clearly, Lloyd and the IPCC he once contributed to now represent different ends of the spectrum.

"The key challenge in understanding climate change is to assess the natural climate variability," Dr. Judith Curry, a climate scientist at Georgia Tech, told The Daily Caller News Foundation in April.

At the time, Ronald Bailey, a science write for Reason magazine, wrote that there has still not been enough observed warming to meet the IPCC's standard of "enhanced warming" — that is, warming above natural levels.

In his article, Bailey noted that there has not been enough temperature rise since the IPCC set its benchmark for "enhanced warming" in 1990. Curry noted that there was a big jump in temperature between 1993 and 1998, but that was basically because of the latter year's El Niño.

"The magnitude of natural climate variability over the past 1000 years and even the past 100 years is hotly debated," Curry added. "Personally, I think the role of natural climate variability has been substantially underestimated in our interpretation of recent climate change."

But not all scientists agree with Bailey's article, and some argue that signs of human influence on the Earth's climate were evident in the 1970s. Indeed, by 1995 the IPCC stated that the "balance of evidence suggests a discernible human influence on global climate." The international body has only made stronger statement on man's climatic influence ever since.

"I would not pin anything on what was said by IPCC in 1990," Dr. Kevin Trenberth, a climate scientist with the National Center for Atmospheric Research, told TheDCNF in April. "In the reports since then there have been thorough evaluations of past IPCC projections and whether they were out of line."

Human influence on the climate may have been observable in the 1970s, but scientists have had trouble explaining why satellite data shows that average global temperatures have been virtually flat for more than 18 years. Satellites measure the troposphere — the lowest few miles of the atmosphere — in contrast, to surface temperature measurements, which most climate bodies rely on for estimates of global average temperature average.

But even surface temperature data showed a hiatus in warming for about 15 years or so. Scientists have offered up dozens of explanations for why global temperatures have been flat since the late 1990s. The most prominent explanation is that oceans have been absorbing most of

the "heat" from increased greenhouse gas emissions, meaning surface temperatures show less warming than they otherwise would.

"What is evident now is that the signal of global warming emerged from the noise of natural variability about the mid 1970s," Trenberth added. "There are fluctuations in global mean temperatures: from year to year with El Niños, etc., and from decade to decade, so that trends reflecting global warming need to be taken over at least 20 years."

Guest essay by Philip Lloyd,

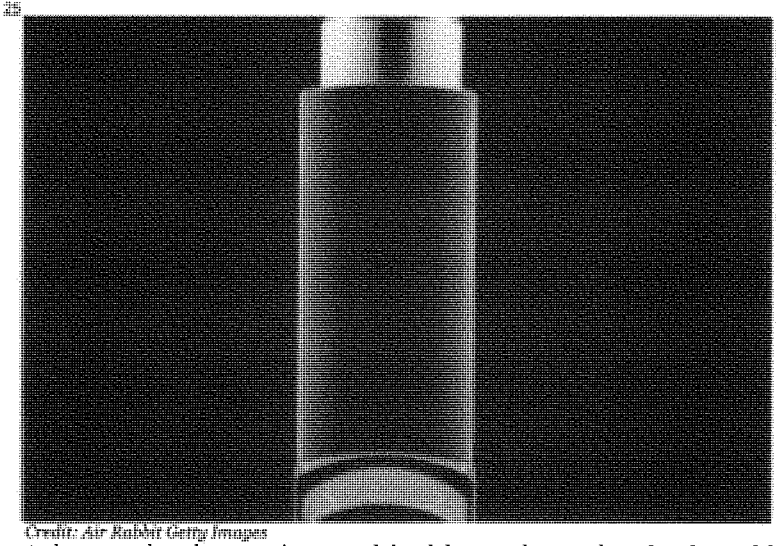
The raw data that is fed to NASA in order to develop the global temperature series is subjected to "homogenization" to ensure that it does not suffer from such things as the changes in the method of measuring the mean temperature, or changes in readings because of changes in location. However, while the process is supposed to be supported by metadata – i.e. the homogenizers are supposed to provide the basis for any modification of the raw data., For example, the raw data for my home city, Cape Town, goes back to 1880:, clip_image002, http://data.giss.nasa.gov/tmp/gistemp/STATIONS/tmp_141688160000_0_0/station.txt, The warmest years were in the 1930's, as they were in many other parts of the globe. There was then a fairly steep decline into the 1970's before the temperature recovered to today's levels, close to the hottest years of the 1930's., In NASA's hands, the data pre-1909 was discarded; the 1910 to 1939 data was adjusted downwards by 1.1deg C; the 1940 to 1959 data was adjusted downwards by about 0.8 deg C on average; the 1969 to 1995 data was adjusted upwards by about 0.2 deg C, with the end result that GISS Ver 2 was:-, clip_image004, Being curious, I asked for the metadata. Eventually I got a single line, most of which was obvious, latitude, longitude, height above mean sea level, followed by four or five alphanumerics. This was no basis for the "adjustments" to the raw data., Which should I believe? The raw data showed a marked drop from the 1940's to the 1970's, which echoed similar drops elsewhere. Time magazine covers showed the 1970's were indeed cold., The raw data is probably accurate. The homogenized data is certainly not. It is difficult to avoid the conclusion that "homogenization" means "revise the story line" and "anthropogenic global warming" really means "humans changed the figures".,

Prof Philip Lloyd, Energy Institute, CPUT, SARETEC, Sacks Circle, Bellville

Sent from my iPad

Researchers once blamed a cleaner world. Now they are not so sure

- By Veronique Greenwood on April 1, 2011
- 25



Credit: Air Rabbit Getty Images

Asthma rates have been surging around the globe over the past three decades, and for a long time researchers thought they had a good idea of what might be fueling the increase: the world we live in is just a little too clean. According to this notion—known as the hygiene hypothesis—exposure in early childhood to infectious agents programs the immune system to mount differing highly effective defenses against disease-causing viruses, bacteria and parasites. Better sanitary conditions deprive the immune system of this training, so that for reasons that are still unclear, the body pounces on harmless particles—such as dust and ragweed—as if they were deadly threats. The resulting allergic reaction leads to the classic signs of asthma: chronic inflammation or swelling of the airways and acute spasms of those passageways.

Or so the thinking went. Although a lot of data support the hygiene hypothesis for allergies, the same cannot be said for asthma. Contrary to expectations, asthma rates have skyrocketed in urban areas in the U.S. that are not particularly clean. Moreover, the big increase in asthma rates in developed countries did not kick off until the 1980s—well after general sanitary conditions in the richer parts of the world had improved. And some studies are beginning to show that far from protecting children from asthma, respiratory infections in early childhood may actually be a risk factor for it.

The collapse of the hygiene hypothesis as a general explanation for the startling jump in asthma rates has led physicians and scientists to a new realization: asthma is a much more complex condition than anyone had truly appreciated. Indeed, it may not be even

be a single disease. Studies now suggest that only half of asthma cases have an allergic component.

The prevention and treatment implications are significant. If, for instance, it is true that allergy is not a fundamental cause of asthma in many people, then an alternative mix of treatments may be more effective for those individuals. To root out asthma's cause (or causes) and properly treat the burgeoning number of people who are affected—300 million globally at last count—scientists will have to come to grips with the biology of its various forms.

Balancing Act

The hygiene hypothesis was first described in 1989 by David P. Strachan, a British epidemiologist who was studying hay fever. The more children in a family, he noticed, the lower the rates of hay fever and eczema, an allergic skin condition. Children in large families tend to swap colds and other infections more often than children with fewer siblings. Could it be that increased exposure to pathogens from their many siblings was protecting children from large families against allergies?

That same year Erika von Mutius, an epidemiologist at Munich University, was looking into the effect of air pollution on asthma in what was then East and West Germany. Children from dirtier East Germany, she was shocked to find, had dramatically less asthma than their West German counterparts living in cleaner, more modern circumstances. The East German children, unlike their Western counterparts, had spent more time in day care and thus had likely been exposed to many more viruses and bacteria. "That was astonishing," she recalls, and led to "a major shift" in thinking.

These findings sparked intense debate among scientists. What is it about unhygienic living that might protect against asthma? One of the more popular explanations in the following decades entailed a balance between the immune cells that are involved in the body's reaction to most viruses and bacteria and those that are involved in the reaction to most parasites and allergens. These two groups of cells produce chemicals that inhibit each other. Early-childhood exposure to bacteria and viruses would cause the infection-related cells to become active, keeping the allergy- and parasite-related cells in check. Without that interplay, the allergy-related cells would later become overreactive, starting an allergic chain reaction that became chronic and ended in constricted airways, asthmatic spasms and labored breathing.

[break]

Inconvenient Facts

There was only one problem. As more data came in, they failed to tell the same story as the hygiene hypothesis. Children in Latin America with high rates of supposedly protective infection have even higher rates of asthma than children in western Europe. Inner-city children in Chicago and New York have quite high rates of asthma, despite unhygienic living. And the rates of asthma varied among countries with very similar histories of cleanliness—indicating that there was more to it than tidiness. For example, by 2004 Sweden's asthma cases had increased to 10 percent, according to one international study, while the number of cases in the U.K. had soared to 20 percent.

In addition, research showed that the relation between asthma and allergy is not at all straightforward. Some cases of asthma are indeed triggered by allergies, although the consensus among researchers over the past decade is that the connection is probably not as clear-cut as the hygiene hypothesis would suggest. Still other layers of immune regulation must be involved. Maria Yazdanbakhsh, a parasitologist at Leiden University

in the Netherlands, has shown that people infected with parasitic worms have very high levels of the allergy-related immune cells but very low rates of asthma, disproving a direct connection between allergy and asthma in these cases at least.

What is more, a landmark review of asthma studies in 1999 by Neil Pearce, now at the London School of Hygiene and Tropical Medicine, demonstrated that at least half of asthma cases in the general population have no connection to allergic reactions at all. These could never be explained by the hygiene hypothesis.

In fact, the same factors that the hygiene hypothesis suggests protect people from developing allergic asthma may cause them to develop nonallergic asthma. "We think that dirt protects against allergic asthma, as foretold by the hygiene hypothesis, but increases the risk of having a nonallergic form," says Laura Rodrigues of the London School of Hygiene and Tropical Medicine, who studies asthma in Latin America. Pollutants in the air can irritate the airways and cause inflammation that leads to constricted breathing. Childhood colds, which the hygiene hypothesis suggested might help prevent development of asthma, can actually be a risk factor for asthma, especially if severe, says James E. Gern, a pediatrician who studies colds and asthma at the University of Wisconsin–Madison. "Early-life infections are an indicator of asthma risk rather than protective in any way," he says.

Besides the hygiene hypothesis, what can explain the increase in asthma rates? Other suggested causes include a rise in sedentary lifestyle, which could affect lung strength, and the rise in obesity, which increases inflammation throughout the body. A reworking of the hygiene hypothesis that focuses on changes in the normal nondisease-causing bacteria that live inside and on the body (in the intestines or the airways or on the skin) has promise. Studies by von Mutius and others have shown that children who live on farms where cows or pigs are raised and where they drink raw milk almost never have asthma, allergic or otherwise. Presumably because the children drank unpasteurized milk and handled livestock, they have different strains of normal bacteria in their airways that are somehow more protective than those found in city kids.

But the short answer to the question of why asthma has increased, according to Pearce, von Mutius, Rodrigues and many others, is, "We don't know." Pearce, in particular, wonders whether modernization in general or westernization in particular may play a role. "There is something about westernization that means people's immune systems function in a different way," he says. "But we don't know what the mechanism is."

[break]

Getting at the true underlying cause of the climb will require better ways of distinguishing among various possible types of asthma. Major asthma research networks supported by the National Institutes of Health have begun recording the details of thousands of individuals' symptoms and treatments. As the results are gathered and analyzed, researchers hope to identify clusters of asthma cases that have different causes and respond to different treatments. The hope is that "if you come in with these characteristics in asthma, we can anticipate what the prognosis is going to be and what the most effective treatment for you is going to be," says William W. Busse of the University of Wisconsin School of Medicine and Public Health, who is part of one such network.

It will take years to understand fully whether microbial exposure, lifestyle changes or the obesity epidemic is more important in explaining the continuing increase in asthma rates. But one thing is clear: the hygiene hypothesis was just the beginning.

CLIMATE DEPOT

UN Scientists Who Have Turned on the UN IPCC & Man-Made Climate Fears — A Climate Depot Flashback Report

Warming fears are the "worst scientific scandal in the history...When people come to know what the truth is, they will feel deceived by science and scientists." - *UN IPCC Japanese Scientist Dr. Kiminori Itoh, an award-winning PhD environmental physical chemist.*

By: Marc Morano - Climate DepotAugust 21, 2013 9:34 PM

Here is a very small sampling of what current and former UN scientists have to say about the UN's climate claims and its scientific methods.

Warming fears are the "worst scientific scandal in the history...When people come to know what the truth is, they will feel deceived by science and scientists." – ***UN IPCC Japanese Scientist Dr. Kiminori Itoh, an award-winning PhD environmental physical chemist.***

"The IPCC has actually become a closed circuit; it doesn't listen to others. It doesn't have open minds... I am really amazed that the Nobel Peace Prize has been given on scientifically incorrect conclusions by people who are not geologists." – ***Indian geologist Dr. Arun D. Ahluwalia at Punjab University and a board member of the UN-supported International Year of the Planet.***

"Temperature measurements show that the [climate model-predicted mid-troposphere] hot zone is non-existent. This is more than sufficient to invalidate global climate models and projections made with them!"- ***UN IPCC Scientist Dr. Steven M. Japar, a PhD atmospheric chemist who was part of Intergovernmental Panel on Climate Change's (IPCC) Second (1995) and Third (2001) Assessment Reports, and has authored 83 peer-reviewed publications and in the areas of climate change, atmospheric chemistry, air pollutions and vehicle emissions.***

UN IPCC Scientist Kenneth P. Green Declares 'A Death Spiral for Climate Alarmism' – September 30, 2009 – 'We can expect climate crisis industry to grow increasingly shrill, and increasingly hostile toward anyone who questions their authority' – ***Dr. Kenneth Green was a Working Group 1 expert reviewer for the United Nations' Intergovernmental Panel on Climate Change (IPCC) in 2001***

'The whole climate change issue is about to fall apart — Heads will roll!' -***South African UN Scientist Dr. Will Alexander, April 12, 2009 – Professor Alexander, is Emeritus of the Department of Civil and Biosystems Engineering at the University of Pretoria in South Africa, and a former member of the United Nations Scientific and Technical Committee on Natural Disasters.***

"I was at the table with three Europeans, and we were having lunch. And they were talking about their role as lead authors. And they were talking about how they were trying to make the report so dramatic that the United States would just have to sign that Kyoto Protocol," Christy told CNN on May 2, 2007. – ***Alabama State Climatologist Dr. John Christy of the University of Alabama in Huntsville, served as a UN IPCC lead author in 2001 for the 3rd assessment report and detailed how he personally witnessed UN scientists attempting to distort the science for political purposes.***

"Gore prompted me to start delving into the science again and I quickly found myself solidly in the skeptic camp…Climate models can at best be useful for explaining climate changes after the fact." – ***Meteorologist Hajo Smit of Holland, who reversed his belief in man-made warming to become a skeptic, is a former member of the Dutch UN IPCC committee.***

"The quantity of CO2 we produce is insignificant in terms of the natural circulation between air, water and soil… I am doing a detailed assessment of the UN IPCC reports and the Summaries for Policy Makers, identifying the way in which the Summaries have distorted the science." – ***South African Nuclear Physicist and Chemical Engineer Dr. Philip Lloyd, a UN IPCC co-coordinating lead author who has authored over 150 refereed publications.***

"The claims of the IPCC are dangerous unscientific nonsense" – declared ***IPCC reviewer and climate researcher Dr Vincent Gray, of New Zealand in 2007. Gray was an expert reviewer on every single draft of the IPCC reports going back to 1990, author of more than 100 scientific publications.*** (LINK) & (LINK)

"After reading [UN IPCC chairman] Pachauri's asinine comment [comparing skeptics to] Flat Earthers, it's hard to remain quiet." – ***Climate statistician Dr. William M. Briggs, who specializes in the statistics of forecast evaluation, serves on the American Meteorological Society's Probability and Statistics Committee and is an Associate Editor of Monthly Weather Review.***

UN IPCC Lead Author Tom Tripp Dissents on man-made warming: 'We're not scientifically there yet' – July 16, 2009

The UN IPCC's Kevin Trenberth's claim that the UN IPCC is an "very open" also needs examining. The IPCC summary for policymakers is used to scare politicians and goad the public into action. The UN is all about politics.

UN special climate envoy Dr. Gro Harlem Brundtland declared "it's completely immoral, even, to question" the UN's alleged global warming "consensus," according to a May 10, 2007 article. Sounds scientific, doesn't it?

Dr. John Brignell, a UK Emeritus Engineering Professor at the University of Southampton who held the Chair in Industrial Instrumentation at Southampton, accused the UN of "censorship" on July 23, 2008.

"Here was a purely political body posing as a scientific institution. Through the power of patronage it rapidly attracted acolytes. Peer review soon rapidly evolved from the old style refereeing to a much more sinister imposition of The Censorship. As Wegman demonstrated, new circles of like-minded propagandists formed, acting as judge and jury for each other. Above all, they acted in concert to keep out alien and hostile opinion. 'Peer review' developed into a mantra that was picked up by political activists who clearly had no idea of the procedures of science or its learned societies. It became an imprimatur of political acceptability, whose absence was equivalent to placement on the proscribed list," Brignell wrote.

Research by Australian climate data analyst John McLean revealed that the IPCC's peer-review process for the Summary for Policymakers leaves much to be desired. (LINK) (LINK) (LINK) & (LINK) McLean's research revealed that the UN IPCC peer-review process is "an illusion." McLean's study found that very few scientists are actively involved in the UN's peer-review process. The report contained devastating revelations to the central IPCC assertion that 'it is very highly likely that greenhouse gas forcing has been the dominant cause of the observed global warming over the last 50 years." The analysis by McLean states: "The IPCC leads us to believe that this statement is very much supported by the majority of reviewers. The reality is that there is surprisingly little explicit support for this key notion. Among the 23 independent reviewers just 4 explicitly endorsed the chapter with its hypothesis, and one other endorsed only a specific section. Moreover, only 62 of the IPCC's 308 reviewers commented on this chapter at all." Repeating: Only four UN scientists in the IPCC peer-review process explicitly endorsed the key chapter blaming mankind for warming the past 50 years, according to this recent analysis.

Here is a small sampling of what current and former UN scientists have to say about the UN IPCC's "very open" process.

(Below are excerpts from various U.S. Senate reports which Climate Depot's Morano authored during his years at the U.S. Senate's Environment and Public Works Committee.)

One former UN IPCC scientist bluntly told the Senate Environment and Public Works (EPW) committee how the UN IPCC Summary for Policymakers "distorted" the scientists work. "I have found examples of a Summary saying precisely the opposite of what the scientists said," explained **South African Nuclear Physicist and Chemical Engineer Dr. Philip Lloyd, a UN IPCC co-coordinating lead author who has authored over 150 refereed publications**.

In an August 13, 2007 letter, **UN IPCC Scientist Dr. Madhav Khandekar, a retired Environment Canada** scientist, lashed out at those who "seem to naively believe that the climate change science espoused in the [UN's] Intergovernmental Panel on Climate Change (IPCC) documents represents 'scientific consensus.'" Khandekar continued: "Nothing could be further than the truth! As one of the invited expert reviewers for the 2007 IPCC documents, I have pointed out the flawed review process used by the IPCC scientists in one of my letters. I have also pointed out in my letter that an increasing number of scientists are now questioning the hypothesis of Greenhouse gas induced warming of the earth's surface and suggesting a stronger impact of solar variability and large-scale atmospheric circulation patterns on the observed temperature increase than

previously believed." "Unfortunately, the IPCC climate change documents do not provide an objective assessment of the earth's temperature trends and associated climate change," Khandekar concluded.

Paul Reiter, a malaria expert formerly of the Centers for Disease Control and Prevention, participated in a past UN IPCC process and now calls the concept of consensus on global warming a "sham." Reiter, a professor of entomology and tropical disease with the Pasteur Institute in Paris, had to threaten legal action to have his name removed from the IPCC. "That is how they make it seem that all the top scientists are agreed," he said on March 5, 2007. "It's not true," he added.

Hurricane expert Christopher W. Landsea of NOAA's National Hurricane Center, was both an author a reviewer for the IPCC's 2nd Assessment Report in 1995 and the 3rd Assessment Report in 2001, but resigned from the 4th Assessment Report after charging the UN with playing politics with Hurricane science. Landsea wrote a January 17, 2005 public letter detailing his experience with the UN: "I am withdrawing [from the UN] because I have come to view the part of the IPCC to which my expertise is relevant as having become politicized. In addition, when I have raised my concerns to the IPCC leadership, their response was simply to dismiss my concerns." "I personally cannot in good faith continue to contribute to a process that I view as both being motivated by pre-conceived agendas and being scientifically unsound," Landsea added.

In addition, a Greenpeace activist co-authored a key economic report in 2007. Left unreported by most of the media was the fact that Bill Hare, an advisor to Greenpeace, was a lead co- author of a key economic report in the IPCC's 4th Assessment. Not surprisingly, the Greenpeace co-authored report predicted a gloomy future for our planet unless we follow the UN's policy prescriptions.

The UN IPCC's own guidelines explicitly state that the scientific reports have to be "change[d]" to "ensure consistency with" the politically motivated Summary for Policymakers.

In addition, the IPCC more closely resembles a political party's convention platform battle – not a scientific process. During an IPCC Summary for Policymakers process, political delegates and international bureaucrats squabble over the specific wording of a phrase or assertion.

Steve McIntyre of Climate Audit, one of the individuals responsible for debunking the infamous "Hockey Stick" temperature graph, slammed the IPCC Summary for Policymaker's process on January 24, 2007.

McIntyre wrote: "So the purpose of the three-month delay between the publication of the (IPCC) Summary for Policy-Makers and the release of the actual WG1 (Working Group 1) is to enable them to make any 'necessary' adjustments to the technical report to match the policy summary. Unbelievable. Can you imagine what securities commissions would say if business promoters issued a big promotion and then the promoters made the 'necessary' adjustments to the qualifying reports and financial statements so that they matched the promotion. Words fail me."

Former Colorado State Climatologist Dr. Roger Pielke Sr. also detailed the corruption of the UN IPCC process on September 1, 2007: "The same individuals who are doing primary research in the role of humans on the climate system are then permitted to lead the [IPCC] assessment! There should be an outcry on this

obvious conflict of interest, but to date either few recognize this conflict, or see that since the recommendations of the IPCC fit their policy and political agenda, they chose to ignore this conflict. In either case, scientific rigor has been sacrificed and poor policy and political decisions will inevitably follow," Pielke explained. He added: "We need recognition among the scientific community, the media, and policymakers that the IPCC process is obviously a real conflict of interest, and this has resulted in a significantly flawed report."

Andrei Kapitsa, a Russian geographer and Antarctic ice core researcher: "The Kyoto theorists have put the cart before the horse. It is global warming that triggers higher levels of carbon dioxide in the atmosphere, not the other way round...A large number of critical documents submitted at the 1995 U.N. conference in Madrid vanished without a trace. As a result, the discussion was one-sided and heavily biased, and the U.N. declared global warming to be a scientific fact."

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