

Of the more than 1,000 climate change-related emails recently made public from a computer at the University of East Anglia in England, one exchange—from 2003—involved contributions from John Holdren, who is now Director of OSTP and was then a professor in the Kennedy School of Government and the Department of Earth and Planetary Sciences at Harvard University and Director of the independent, nonprofit Woods Hole Research Center in Woods Hole, Mass. That exchange—between Holdren and a skeptical blogger—provides an excellent summary of how a person lacking scientific expertise might best arrive at a conclusion about the conflicting data relating to the science of climate change. The entire exchange appears below.

Date: Thu, 16 Oct 2003 16:43:41 -0400

To: Malcolm Hughes <mhughes@ltr.arizona.edu>, Tim Osborn <t.osborn@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Caspar Ammann <ammann@ucar.edu>, rbradley@geo.umass.edu, tcrowley@duke.edu, omichael@princeton.edu, jto@u.arizona.edu, Scott Rutherford <srutherford@rwu.edu>, p.jones@uea.ac.uk, mann@virginia.edu, Tom Wigley <wigley@ucar.edu>
From: "Michael E. Mann" <mann@virginia.edu>

Subject: Fwd: Correspondence on Harvard Crimson coverage of Soon / Baliunas views on climate

Dear All,

Thought you would be interested in this exchange, which John Holdren of Harvard has been kind enough to pass along...

mike

Date: Thu, 16 Oct 2003 13:53:08 -0400

To: "Michael Mann" <mem6u@virginia.edu>, "Tom Wigley" <wigley@ucar.edu>
From: "John P. Holdren" <john_holdren@harvard.edu>

Subject: Correspondence on Harvard Crimson coverage of Soon / Baliunas views on climate

Michael and Tom --

I'm forwarding for your entertainment an exchange that followed from my being quoted in the Harvard Crimson to the effect that you and your colleagues are right and my "Harvard" colleagues Soon and Baliunas are wrong about what the evidence shows concerning surface temperatures over the past millennium. The cover note to faculty and postdocs in a regular Wednesday breakfast discussion group on environmental science and public policy in Harvard's Department of Earth and Planetary Sciences is more or less self-explanatory.

Best regards,
John

Date: Thu, 16 Oct 2003 11:02:24 -0400

To: schrag@eps.harvard.edu, oconnell@eps.harvard.edu, holland@eps.harvard.edu, pearson@eps.harvard.edu, eli@eps.harvard.edu, ingalls@eps.harvard.edu, mlm@eps.harvard.edu, avan@fas.harvard.edu, moyer@huarp.harvard.edu, poussart@fas.harvard.edu, jshaman@fas.harvard.edu, sivan@fas.harvard.edu,

bec@io.harvard.edu, saleska@fas.harvard.edu
From: "John P. Holdren" <john_holdren@harvard.edu>

Subject: For the EPS Wednesday breakfast group: Correspondence on Harvard Crimson coverage of Soon / Baliunas views on climate
Cc: jeremy_bloxham@harvard.edu, william_clark@harvard.edu, patricia_mclaughlin@harvard.edu,

Colleagues--

I append here an e-mail correspondence I have engaged in over the past few days trying to educate a Soon/Baliunas supporter who originally wrote to me asking how I could think that Soon and Baliunas are wrong and Mann et al. are right (a view attributed to me, correctly, in the Harvard Crimson). This individual apparently runs a web site on which he had been touting the Soon/Baliunas position.

While it is sometimes a mistake to get into these exchanges (because one's interlocutor turns out to be ineducable and/or just looking for a quote to reproduce out of context in an attempt to embarrass you), there was something about this guy's formulations that made me think, at each round, that it might be worth responding. In the end, a couple of colleagues with whom I have shared this exchange already have suggested that its content would be of interest to others, and so I am sending it to our "environmental science and policy breakfast" list for your entertainment and, possibly, future breakfast discussion.

The items in the correspondence are arranged below in chronological order, so that it can be read straight through, top to bottom.

Best,
John

At 09:43 PM 9/12/2003 -0400, you wrote:

Dr. Holdren:

In a recent Crimson story on the work of Soon and Baliunas, who have written for my website [1]www.techcentralstation.com, you are quoted as saying:

My impression is that the critics are right. It s unfortunate that so much attention is paid to a flawed analysis, but that s what happens when something happens to support the political climate in Washington.

Do you feel the same way about the work of Mann et. al.? If not why not?

Best,
Nick
Nick Schulz
Editor
TCS
1-800-619-5258

From: John P. Holdren [[2]mailto:john_holdren@harvard.edu]
Sent: Monday, October 13, 2003 11:06 AM
To: Nick Schulz

Subject: Harvard Crimson coverage of Soon / Baliunas controversy

Dear Nick Schultz --

I am sorry for the long delay in this response to your note of September 12. I have been swamped with other commitments.

As you no doubt have anticipated, I do not put Mann et al. in the same category with Soon and Baliunas.

If you seriously want to know "Why not?", here are three ways one might arrive at what I regard as the right conclusion:

(1) For those with the background and patience to penetrate the scientific arguments, the conclusion that Mann et al. are right and Soon and Baliunas are wrong follows from reading carefully the relevant Soon / Baliunas paper and the Mann et al. response to it:

W. Soon and S. Baliunas, "Proxy climatic and environmental changes of the past 1000 years", *Climate Research*, vol. 23, pp 89ff, 2003.

M. Mann, C. Amman, R. Bradley, K. Briffa, P. Jones, T. Osborn, T. Crowley, M. Hughes, M. Oppenheimer, J. Overpeck, S. Rutherford, K. Trenberth, and T. Wigley, "On past temperatures and anomalous late-20th century warmth", *EOS*, vol 84, no. 27, pp 256ff, 8 July 2003.

This is the approach I took. Soon and Baliunas are demolished in this comparison.

(2) Those lacking the background and/or patience to penetrate the two papers, and seriously wanting to know who is more likely to be right, have the option of asking somebody who does possess these characteristics -- preferably somebody outside the handful of ideologically committed and/or oil-industry-linked professional climate-change skeptics -- to evaluate the controversy for them. Better yet, one could poll a number of such people. They can easily be found by checking the web pages of earth sciences, atmospheric sciences, and environmental sciences departments at any number of major universities.

(3) The least satisfactory approach, for those not qualified for (1) and lacking the time or initiative for (2), would be to learn what one can about the qualifications (including publications records) and reputations, in the field in question, of the authors on the two sides. Doing this would reveal that Soon and Baliunas are, essentially, amateurs in the interpretation of historical and paleoclimatological records of climate change, while the Mann et al. authors include several of the most published and most distinguished people in the world in this field. Such an investigation would also reveal that Dr. Baliunas' reputation in this field suffered considerable damage a few years back, when she put her name on an incompetent critique of mainstream climate science that was never published anywhere respectable but was circulated by the tens of thousands, in a format mimicking that of a reprint from the Proceedings of the National Academy of Sciences, in pursuit of signatures on a petition claiming that the mainstream findings were wrong.

Of course, the third approach is the least satisfactory because it can be dangerous to assume that the more distinguished people are always right. Occasionally, it turns out that the opposite is true. That is one of several good reasons that it pays to try to penetrate the arguments, if one can, or to poll others who have tried to do so. But in cases where one is not able or willing to do either of these things -- and where one is able to discover that the imbalance of experience and reputation on the two sides of the issue is as lopsided as here -- one ought at least to recognize that the odds strongly favor the proposition that the more experienced and reputable people are right. If one were a policy maker, to bet the public welfare on the long odds of the opposite being true would be foolhardy.

Sincerely,
John Holdren

PS: I have provided this response to your query as a personal communication, not as fodder for selective excerpting on your web site or elsewhere. If you do decide that you would like to propagate my views on this matter more widely, I ask that you convey my response in its entirety.

At 11:16 AM 10/13/2003 -0400, you wrote:

I have the patience but, by your definition certainly, not the background, so I suppose it is not surprising I came to a different conclusion. I guess my problem concerns what lawyers call the burden of proof. The burden weighs heavily much more heavily, given the claims on Mann et.al. than it does on Soon/Baliunas. Would you agree?

Falsifiability for the claims of Mann et. al. requires but a few examples, does it not? Soon/Baliunas make claims that have no such burden. Isn't that correct?

Best,
Nick

From: John P. Holdren [[3]mailto:john_holdren@harvard.edu]

Sent: Tuesday, October 14, 2003 5:54 PM

To: Nick Schulz

Subject: RE: Harvard Crimson coverage of Soon / Baliunas controversy

Nick--

Yes, I can see how it might seem that, in principle, those who are arguing for a strong and sweeping proposition (such as that "the current period is the warmest in the last 1000 years") must meet a heavy burden of proof, and that, because even one convincing counter-example shoots the proposition down, the burden that must be borne by the critics is somehow lighter. But, in practice, burden of proof is an evolving thing -- it evolves as the amount of evidence relevant to a particular proposition grows.

To choose an extreme example, consider the first and second laws of thermodynamics. Both of these are "empirical" laws. Our confidence in them is based entirely on observation; neither one can be "proven" from more fundamental laws. Both are very sweeping. The first law says that energy is conserved in all physical processes. The second law says that entropy increases in all physical processes. So, is the burden of proof heavier on somebody who asserts that these laws are correct, or on somebody who claims to have found an exception to one or both of them? Clearly, in this case, the burden is heavier on somebody who asserts an exception. This is in part because the two laws have survived every such challenge in the past. No exception to either has ever been documented. Every alleged exception has turned out to be traceable to a mistake of some kind. This burden on those claiming to have found an exception is so strong that the US Patent Office takes the position, which has been upheld in court, that any patent application for an invention that violates either law can be rejected summarily, without any further analysis of the details.

Of course, I am not asserting that the claim we are now in the warmest period in a millennium is in the same league with the laws of thermodynamics. I used the latter only to illustrate the key point that where the burden is heaviest depends on the state of prior evidence and analysis on the point in question -- not simply on whether a proposition is sweeping or narrow.

In the case actually at hand, Mann et al. are careful in the nature of their claim. They write along the lines of "A number of reconstructions of large-scale temperature changes support the conclusion" that the current period is the warmest in the last millennium. And they write that the claims of Baliunas et al. are "inconsistent with the preponderance of scientific evidence". They are not saying that no shred of evidence to the contrary has ever been produced, but rather that analysis of the available evidence as a whole tends to support their conclusion.

This is often the case in science. That is, there are often "outlier" data points or apparent contradictions that are not yet adequately explained, but still are not given much weight by most of the scientists working on a particular issue if a strong preponderance of evidence points the other way. This is because the scientists judge it to be more probable that the outlier data point or apparent contradiction will ultimately turn out to be explainable as a mistake, or otherwise explainable in a way that is consistent with the preponderance of evidence, than that it will turn out that the preponderance of evidence is wrong or is being misinterpreted. Indeed, apparent contradictions with a preponderance of evidence are FAR more often due to measurement error or analysis error than to real contradiction with what the preponderance indicates.

A key point, then, is that somebody with a PhD claiming to have identified a counterexample does not establish that those offering a general proposition have failed in their burden of proof. The counterexample itself must pass muster as both valid in itself and sufficient, in the generality of its implications, to invalidate the proposition.

In the case at hand, it is not even a matter of an "outlier" point or other seeming contradiction that has not yet been explained. Mann et al. have explained in detail why the supposed contrary evidence offered by Baliunas et al. does NOT constitute a counterexample. To those with some knowledge and experience in studies of this kind, the refutation by Mann et al is completely convincing.

Sincerely,
John Holdren

At 08:08 AM 10/15/2003 -0400, you wrote:

Dr. Holdren:

Thank you for your thoughtful reply. I genuinely appreciate you taking the time. You are quite right about the laws of thermodynamics. And you are quite right that Mann et al is not in the same league as those laws and that's not to take anything from their basic research.

You write to those with knowledge and experience in studies of this kind, the refutation by Mann et al is completely convincing. Since I do not have what you would consider the requisite knowledge or experience, I can't speak to that. I've read the Mann papers and the Baliunas Soon paper and the Mann rebuttal and find Mann's claims based on his research extravagant and beyond what he can legitimately claim to know. That said, I'm willing to believe it is because I don't have the tools necessary to understand.

But if you will indulge a lay person with some knowledge of the matter, perhaps you could clear up a thing or two.

Part of the confusion over Mann et al it seems to me has to do not with the research itself but with the extravagance of the claims they make based on their research. And yet you write: Mann et al. are careful in the nature of their claim. They write along the lines of A number of reconstructions of large-scale temperature changes support the conclusion that the current period is the warmest in the last millennium. And they write that the claims of Baliunas et al. are inconsistent with the preponderance of scientific evidence .

That makes it seem as if Mann s not claiming anything particularly extraordinary based on his research.

But Mann claimed in the NYTimes in 1998 that in their Nature study from that year Our conclusion was that the warming of the past few decades appears to be closely tied to emission of greenhouse gases by humans and not any of the natural factors." Does that seem to be careful in the nature of a claim? Respected scientists like Tom Quigley responded at the time by saying "I think there's a limit to how far you can ever go." As for using proxy data to detect a man-made greenhouse effect, he said, "I don't think we're ever going to get to the point where we're going to be totally convincing." These are two scientists who would agree on the preponderance of evidence and yet they make different claims about what that preponderance means. There are lots of respected climatologists who would say Mann has insufficient scientific basis to make that claim. Would you agree? The Soon Baliunas research is relevant to that element of the debate what the preponderance of evidence enables us to claim within reason. To that end, I don't think claims of Soon Baliunas are inconsistent with the preponderance of scientific evidence.

I'll close by saying I'm willing to admit that, as someone lacking a PhD, I could be punching above my weight. But I will ask you a different but related question How much hope is there for reaching reasonable public policy decisions that affect the lives of millions if the science upon which those decisions must be made is said to be by definition beyond the reach of those people?

All best,
Nick

Date: Thu, 16 Oct 2003 08:46:23 -0400
To: "Nick Schulz" <nschulz@techcentralstation.com>
From: "John P. Holdren" <john_holdren@harvard.edu>

Subject: RE: Harvard Crimson coverage of Soon / Baliunas controversy

Nick--

You ask good questions. I believe the thoughtfulness of your questions and the progress I believe we are making in this interchange contain the seeds of the answer to your final question, which, if I may paraphrase just a bit, is whether there's any hope of reaching reasonable public-policy decisions when the details of the science germane to those decisions are impenetrable to most citizens.

This is a hard problem. Certainly the difficulty is not restricted to climate science and policy, but applies also to nuclear-weapon science and policy, nuclear-energy science and policy, genetic science and policy, and much more. But I don't think the difficulties are insurmountable. That's why I'm in the business I'm in, which is

teaching about and working on the intersection of science and technology with policy.

Most citizens cannot penetrate the details of what is known about the how the climate works (and, of course, what is known even by the most knowledgeable climate scientists about this is not everything one would like to know, and is subject to modification by new data, new insights, new forms of analysis). Neither would most citizens be able to understand how a hydrogen bomb works (even if the details were not secret), or what factors will determine the leak rates of radioactive nuclides from radioactive-waste repositories, or what stem-cell research does and promises to be able to do.

But, as Amory Lovins once said in addressing the question of whether the public deserved and could play a meaningful role in debates about nuclear-weapon policy, even though most citizens would never understand the details of how nuclear weapons work or are made, "You don't have to be a chicken to know what to do with an egg." In other words, for many (but not all) policy purposes, the details that are impenetrable do not matter.

There CAN be aspects of the details that do matter for public policy, of course. In those cases, it is the function and the responsibility of scientists who work across the science-and-policy boundary to communicate the policy implications of these details in ways that citizens and policy makers can understand. And I believe it is the function and responsibility of citizens and policy makers to develop, with the help of scientists and technologists, a sufficient appreciation of how to reach judgments about plausibility and credibility of communications about the science and technology relevant to policy choices so that the citizens and policy makers are NOT disenfranchised in policy decisions where science and technology are germane.

How this is best to be done is a more complicated subject than I am prepared to try to explicate fully here. (Alas, I have already spent more time on this interchange than I could really afford from other current commitments.) Suffice it to say, for now, that improving the situation involves increasing at least somewhat, over time, the scientific literacy of our citizens, including especially in relation to how science works, how to distinguish an extravagant from a reasonable claim, how to think about probabilities of who is wrong and who is right in a given scientific dispute (including the question of burden of proof as you and I have been discussing it here), how consulting and polling experts can illuminate issues even for those who don't understand everything that the experts say, and why bodies like the National Academy of Sciences and the Intergovernmental Panel on Climate Change deserve more credibility on the question of where mainstream scientific opinion lies than the National Petroleum Council, the Sierra Club, or the editorial page of the Wall Street Journal.

Regarding extravagant claims, you continue to argue that Mann et al. have been guilty of this, but the formulation of theirs that you offer as evidence is not evidence of this at all. You quote them from the NYT in 1998, referring to a study Mann and co-authors published in that year, as saying

"Our conclusion was that the warming of the past few decades appears to be closely tied to emission of greenhouse gases by humans and not any of the natural factors."

and you ask "Does that seem to be careful in the nature of a claim?" My answer is: Yes, absolutely, their formulation is careful and appropriate. Please note that they did NOT say "Global warming is closely tied to emission of greenhouse gases by humans and not any of the natural factors." They said that THEIR CONCLUSION (from a particular, specified study, published in NATURE) was that the warming of THE PAST FEW DECADES (that is, a particular, specified part of the historical record) APPEARS (from

the evidence adduced in the specified study) to be closely tied... This is a carefully specified, multiply bounded statement, which accurately reflects what they looked at and what they found. And it is appropriately contingent --"APPEARS to be closely tied" -- allowing for the possibility that further analysis or new data could later lead to a different perspective on what appears to be true.

With respect, it does not require a PhD in science to notice the appropriate boundedness and contingency in the Mann et al. formulation. It only requires an open mind, a careful reading, and a degree of understanding of the character of scientific claims and the wording appropriate to convey them that is accessible to any thoughtful citizen. That is why I'm an optimist.

You go on to quote the respected scientist "Tom Quigley" as holding a contrary view to that expressed by Mann. But please note that: (1) I don't know of any Tom Quigley working in this field, so I suspect you mean to refer to the prominent climatologist Tom Wigley; (2) the statements you attribute to "Quiqley" do not directly contradict the careful statement of Mann (that is, it is entirely consistent for Mann to say that his study found that recent warming appears to be tied to human emissions and for Wigley to say that that there are limits to how far one can go with this sort of analysis, without either one being wrong); and (3) Tom Wigley is one of the CO-AUTHORS of the resounding Mann et al. refutation of Soon and Baliunas (see attached PDF file).

I hope you have found my responses to be of some value. I now must get on with other things.

Best,
John Holdren

JOHN P. HOLDREN

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