From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>, Tom Wigley <wigley@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, rbradley@geo.umass.edu
Subject: Re: Soon et al. paper
Date: Tue May 20 16:07:41 2003
Cc: Jerry Meehl <meehl@ucar.edu>, Caspar Ammann <ammann@ucar.edu>, mann@virginia.edu

Mike and Tom and others
My silence to do with the specific issue of the Soon and Baliunas conveys general strong agreement with all the general remarks (and restatement of many in various forms) by Tom Crowley, Mike Mann, Neville Nichols and now Tom Wigley regarding the scientific value of the paper and its obvious methodological flaws.
I have to say that I tended towards the "who cares" camp , in as much as those who are concerned about the science should see through it anyway . I also admit to thinking that some of you seem a little paranoid (especially in the implication that Climate Research is a pro sceptic journal) but I am changing my mind regarding the way the "meaning" of the BS paper is being presented to the wider public - in response to some very poor recent reporting in the British press and several requests from the US that indicate that those of you who work there can not simply rely on the weight of good science eventually showing through as regards the public perception . As Tom W. states , there are uncertainties and "difficulties" with our current knowledge of Hemispheric temperature histories and valid criticisms or shortcomings in much of our work. This is the nature of the beast - and I have been loathe to become embroiled in polarised debates that force too simplistic a presentation of the state of the art or "consensus view". Having read Tom W's and Mike's latest statements I now agree about the need to make some public comment on BS . (I too have given my personal view of the work to David Appell who I assume is writing a balanced view of this paper for Scientific American). I see little need to get involved in a over detailed critic of all the points in the paper , because I am not sure what audience would benefit from it, but the points made by those I listed above could usefully be fashioned into a simple letter to Climate Research, signed by those who wish. This would then go on record as a simple statement of refutation of the method employed and corresponding limitation of the work for informing the "global warming " debate . This could be quickly citable when talking to the media.
The one additional point I would make that seems to have been overlooked in the discussions up to now , is the invalidity of assuming that the existence of a global Medieval Warm period , even if shown to be as warm as the current climate , somehow negates the possibility of enhanced greenhouse warming. The business of constructing a reliable climate history is only one part of establishing the relative roles of natural and anthropogenic forcings, now and in the future. Without reference to the roles of natural forcings in recent and past times , comparisons with other periods are of very limited value anyway.
So I agree with Tom and Mike that something needs to go "on record" . The various papers apparently in production, regardless of their individual emphasis or approaches, will find their way in to the literature and the next IPCC can sift and present their message(s) as it wishes., but in the meantime , why not a simple statement of the shortcomings of the BS paper as they have been listed in these messages and why not in Climate Research?
Keith
At 05:04 PM 5/16/03 -0400, Michael E. Mann wrote:

Tom,
Thanks for your response, which I will maintain as confidential within the small group of the original recipients (other than Ray whom I've included in as well), given the sensitivity of some of the comments made.
Whether or not their comments are ad hominem or potentially libelous is probably immaterial here (some people who have read them think they might be--in certain places, alterior motives are implied on the part of individually named scientists in the discussion of scientific methodologies).
However, the real issue, as you point out, is whether or not their arguments and criticisms are valid. I would argue that very few of them are--I have prepared (and have attached) a draft of replies to some of the specifics in their two papers--this is rough, and I'm working on preparing a refined version of this for use by those who are trying to combat the disinformation that the Baliunas and co. supporters are working at spreading within the beltway, with the full support of industry, and perhaps the administration. By necessity this is brief and focus on the most salient points--a point-by-point rebuttal would take a very long time.
In the meantime, Phil and I, and Ray/Malcolm/Henry D are independently working on review pieces (ours for R.O.G., Ray et al's for Science) that will also correct in more detail some of the most egregious untruths put forward by the Baliunas/Soon pieces (what one colleague of mine aptly chooses to abbreviate as "BS").
The most fundamental criticism, of course, is that the hypothesis, methods, and

assumptions are absolutely nonsensical by construction--as you already pointed out. One could demonstrate that with an example, but then again, why do so when it is self evident that defining an anomaly of either wetter or dryer (what does that leave out?) relative to the 20th century (a comparison which is itself also ill-defined by the authors, since they don't use a uniform 20th century reference period for defining their qualitative anomalies, and discuss proxy records with variable resolution and temporal sampling of the 20th century) was "warmer than the 20th century" is nonsense at the most fundamental level. It defies the most elementary logic, and thus is difficult to reply to other than noting that it is nonsense by its very nature.
Would we be compelled to provide a counterexample to disprove the authors if they had asserted that "1=2"? What they have done isn't that much different...
So its one thing to throw out a bunch of criticisms, very few of which are valid. But to then turn around and present a fundamentally ill-posed, supposed "analysis" which doesn't even attempt to provide a quantitative "alternative" to past studies, to claim to have disproven those past studies, and to supposedly support the non-sequitor conclusion that the "MWP was warmer than the 20th century" is irresponsible, deceptive, dishonest, and a violation of the very essence of the scientific approach in my view.
One or two people can't fight that alone, certainly not with the "artillary" (funding and political organization) that has been lined up on the other side. In my view, it is the responsibility of our entire community to fight this intentional disinformation campaign, which represents an affront to everything we do and believe in. I'm doing everything I can to do so, but I can't do it alone--and if I'm left to, we'll lose this battle,
mike
At 02:18 PM 5/16/2003 -0600, Tom Wigley wrote:

Dear folks,
I have just read the Soon et al. paper in E&E. Here are some comments, and a request.
Mike said in an email that he thought the paper contained possibly 'legally actionable' ad hominem attacks on him and others. I do not agree that there are ad hominem attacks. There are numerous criticisms, usually justified (although not all the justifications are valid). I did not notice any intemperate language.
While many of the criticisms are invalid, and some are irrelevant, there are a number that seem to me to be quite valid. Probably, most of these can be rebutted, and perhaps some of these are already covered in the literature. In my view, however, there a small number of points that are valid criticisms.
[Off the record, the most telling criticisms apply to Tom Crowley's work -- which I do not hold in very high regard.]
The real issue that the press (to a limited extent) and the politicians (to a greater extent) have taken up is the conclusions of the paper's original research.
First, Soon et al. come down clearly in favor of the existence of a MWE and a LIA. I think many of us would agree that there was a global-scale cool period that can be identified with a LIA. The MWE is more equivocal. There are real problems in identifying both of these 'events' with certainty due to (1) data coverage, (2) uncertainty in transfer functions, and (3) the noise of internally generated variability on the century time scale. [My paper on the latter point is continually ignored by the paleo community, but it is still valid.]
So, we would probably say: there was a LIA; but the case for *or against* a MWE is not proven. There is no strong diagreement with Soon et al. here.
The main disagreements are with the methods used by Soon et al. to draw their LIA/MWE conclusion, and their conclusion re the anomalousness/uniqueness of the 20th century (a conclusion that is based on the same methods).
So what is their method? I need to read the paper again carefully to check on this, but it seems that they say the MWE [LIA] was warm [cold] if at a particular site there is a 50+ year period that was warm, wet, dry [cold, dry, wet] somewhere in the interval 800-1300 [1300-1900], where warm/cold, wet, dry are defined relative to the 20th century.
The problems with this are .....
(1) Natural internally generated variability alone virtually guarantees that these criteria will be met at every site.
(2) As Nev Nicholls pointed out, almost any period would be identified as a MWE or LIA by these criteria -- and, as a corollary, their MWE period could equally well have been identified as a LIA (or vice versa)
(3) If the identified warm blips in their MWE were are different times for different locations (as they are) then there would be no global-mean signal.
(4) The reason for including precip 'data' at all (let alone both wet and dry periods in both the MWE and LIA) is never stated -- and cannot be justified. [I suspect that if they found a wet period in the MWE, for example, they would search for a dry period in

the LIA -- allowing both in both the MWE and LIA seems too stupid to be true.]
(5) For the uniqueness of the 20th century, item (1) also applies.
So, their methods are silly. They seem also to have ignored the fact that what we are searching is a signal in global-mean temperature.
The issue now is what to do about this. I do not think it is enough to bury criticisms of this work in other papers. The people who have noticed the Soon et al paper, or have had it pointed out to them, will never see or become aware of such rebuttals/responses. Furthermore, I do not think that a direct response will give the work credibility. It is already 'credible' since it is in the peer reviewed literature (and E&E, by the way, is peer reviewed). A response that says this paper is a load of crap for the following reasons is *not* going to give the original work credibility -- just the opposite.
How then does one comprehensively and concisely demolish this work? There are two issues here. The first is the point by point response to their criticisms of the literature. To do this would be tedious, but straightforward. There will be at least some residual criticisms that must be accepted as valid, and this must be admitted. Cross-referencing to other review papers would be legitimate here.
The second is to demolish the method. I have done this qualitatively (following Nev mainly) above, but this is not enough. What is needed is a counter example that uses the method of reductio ad absurdem. This would be clear and would be appropriate since it avoids us having to point out in words that their methods are absurd. I have some ideas how to do this, but I will let you think about it more before going further.
You will see from this email that I am urging you to produce a response. I am happy to join you in this, and perhaps a few others could add their weight too. I am copying this to Jerry since he has to give some congressional testimony next week and questions about the Soon et al work are definitely going to be raised. I am also copying this to Caspar, since the last millenium runs that he is doing with paleo-CSM are relevant.
Best wishes,
Tom.

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