

At 01:07 PM 9/22/99 +0100, Folland, Chris wrote:

Dear All

A proxy diagram of temperature change is a clear favourite for the Policy Makers summary. But the current diagram with the tree ring only data somewhat contradicts the multiproxy curve and dilutes the message rather significantly. We want the truth. Mike thinks it lies nearer his result (which seems in accord with what we know about worldwide mountain glaciers and, less clearly, suspect about solar variations). The tree ring results may still suffer from lack of multicentury time scale variance. This is probably the most important issue to resolve in Chapter 2 at present.

Chris

At 04:19 PM 9/22/99 +0100, Keith Briffa wrote:

Hi everyone

Let me say that I don't mind what you put in the policy makers summary if there is a general consensus. However some general discussion would be valuable. First, like Phil, I think that the supposed separation of the tree-ring reconstruction from the others on the grounds that it is not a true "multi-proxy" series is hard to justify. What is true is that these particular tree-ring data best represent SUMMER temperatures mostly at the northern boreal forest regions. By virtue of this, they also definitely share significant variance with Northern Hemisphere land and land and marine ANNUAL temperatures - but at decadal and multidecadal timescales - simply by virtue of the fact that these series correlated with the former at these timescales. The multi proxy series (Mann et al. Jones et al) supposedly represent annual and summer seasons respectively, and both contain large proportions of tree-ring input. The latest tree-ring density curve (i.e. our data that have been processed to retain low frequency information) shows more similarity to the other two series- as do a number of other lower resolution data (Bradley et al, Peck et al., and new Crowley series - see our recent Science piece) whether this represents 'TRUTH' however is a difficult problem. I know Mike thinks his series is the 'best' and he might be right - but he may also be too dismissive of other data and possibly over confident in his (or should I say his use of other's). After all, the early (pre-instrumental) data are much less reliable as indicators of global temperature than is apparent in modern calibrations that include them and when we don't know the precise role of particular proxies in the earlier portions of reconstruction it remains problematic to assign genuine confidence limits at multidecadal and longer timescales. I still contend that multiple regression against the recent very trendy global mean series is potentially dangerous. You could calibrate the proxies to any number of seasons, regardless of their true optimum response. Not for a moment am I saying that the tree-ring, or any other proxy data, are better than Mike's series - indeed I am saying that the various reconstructions are not independent but that they likely contribute more information about reality together than they do alone. I do believe, that it should not be taken as read that Mike's series (or Jones's et al. for that matter) is THE CORRECT ONE. I prefer a Figure that shows a multitude of reconstructions (e.g similar to that in my Science piece). Incidentally, arguing that any particular series is probably better on the basis of what we now know about glaciers or solar output is flaky indeed. Glacier mass balance is driven by the difference mainly in winter accumulation and summer ablation, filtered in a complex non-linear way to give variously lagged tongue advance/retreat. Simple inference on the precedence of modern day snout positions does not translate easily into absolute (or relative) temperature levels now or in the past. Similarly, I don't see that we are able to substantiate the veracity of different temperature reconstructions through reference to Solar forcing theories without making assumptions on the effectiveness of (seasonally specific) long-term insolation changes in different parts of the globe and the contribution of solar forcing to the observed 20th century warming. There is still a potential

problem with non-linear responses in the very recent period of some biological proxies (or perhaps a fertilization through high CO2 or nitrate input) . I know there is pressure to present a nice tidy story as regards 'apparent unprecedented warming in a thousand years or more in the proxy data' but in reality the situation is not quite so simple. We don't have a lot of proxies that come right up to date and those that do (at least a significant number of tree proxies) some unexpected changes in response that do not match the recent warming. I do not think it wise that this issue be ignored in the chapter.

For the record, I do believe that the proxy data do show unusually warm conditions in recent decades. I am not sure that this unusual warming is so clear in the summer responsive data. I believe that the recent warmth was probably matched about 1000 years ago. I do not believe that global mean annual temperatures have simply cooled progressively over thousands of years as Mike appears to and I contend that that there is strong evidence for major changes in climate over the Holocene (not Milankovich) that require explanation and that could represent part of the current or future background variability of our climate. I think the Venice meeting will be a good place to air these issues.

Finally I apologise for this rather self-indulgent ramble, but I thought I may as well voice these points to you . I too would be happy to go through the recent draft of the chapter when it becomes available.

cheers to all
Keith

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From: "Michael E. Mann" <mann@xxxxxxxxxxx>
To: Keith Briffa <k.briffa@xxxxxxxxxxx>, "Folland, Chris" <ckfolland@xxxxxxxxxxx>, 'Phil Jones' <p.jones@xxxxxxxxxxx>
Subject: RE: IPCC revisions
Date: Wed, 22 Sep 1999 12:35:24 -0400
Cc: tkarl@xxxxxxxxxxx, mann@xxxxxxxxxxx

Thanks for your response Keith,

For all:

Walked into this hornet's nest this morning! Keith and Phil have both raised some very good points. And I should point out that Chris, through no fault of his own, but probably through ME not conveying my thoughts very clearly to the others, definitely overstates any singular confidence I have in my own (Mann et al) series. I believe strongly that the strength in our discussion will be the fact that certain key features of past climate estimates are robust among a number of quasi-independent and truly independent estimates, each of which is not without its own limitations and potential biases. And I certainly don't want to abuse my lead authorship by advocating my own work.

I am perfectly amenable to keeping Keith's series in the plot, and can ask Ian Macadam (Chris?) to add it to the plot he has been preparing (nobody liked my own color/plotting conventions so I've given up doing this myself). The key thing is making sure the series are vertically aligned in a reasonable

way. I had been using the entire 20th century, but in the case of Keith's, we need to align the first half of the 20th century w/ the corresponding mean values of the other series, due to the late 20th century decline.

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

So, if we show Keith's series in this plot, we have to comment that "something else" is responsible for the discrepancies in this case. Perhaps Keith can help us out a bit by explaining the processing that went into the series and the potential factors that might lead to it being "warmer" than the Jones et al and Mann et al series?? We would need to put in a few words in this regard. Otherwise, the skeptics have an field day casting doubt on our ability to understand the factors that influence these estimates and, thus, can undermine faith in the paleoestimates. I don't think that doubt is scientifically justified, and I'd hate to be the one to have to give it fodder!

The recent Crowley and Lowery multiproxy estimate is an important additional piece of information which I have indeed incorporated into the revised draft. Tom actually estimates the same mean warming since the 17th century in his reconstruction, that we estimate in ours, so it is an added piece of information that Phil and I are probably in the ballpark (Tom has used a somewhat independent set of high and low-resolution proxy data and a very basic compositing methodology, similar to Bradley and Jones, so there is some independent new information in this estimate.

One other key result with respect to our own work is from a paper in the press in "Earth Interactions". An unofficial version is available here:

http://www.ngdc.noaa.gov/paleo/ei/ei_cover.html

The key point we emphasize in this paper is that the low-frequency variability in our hemispheric temperature reconstruction is basically the same if we don't use any dendroclimatic indicators at all (though we certainly resolve less variance, can't get a skillful reconstruction as far back, and there are notable discrepancies at the decadal and interannual timescales). I believe I need to add a sentence to the current discussion on this point, since there is an unsubstantiated knee-jerk belief that our low-frequency variability is suppressed by the use of tree ring data.

We have shown that this is not the case: (see here:

http://www.ngdc.noaa.gov/paleo/ei/ei_datarev.html

and specifically, the plot and discussion here:

http://www.ngdc.noaa.gov/paleo/ei/ei_nodendro.html

Ironically, you'll note that there is more low-frequency variability when the tree ring data *are* used, then when only other proxy and historical/instrumental data are used!

SO I think we're in the position to say/resolve somewhat more than, frankly, than Keith does, about the temperature history of the past millennium. And the issues I've spelled out all have to be dealt with in the chapter.

One last point: We will (like it or not) have SUBSTANTIAL opportunity/requirement to revise much of this discussion after review, so we don't have to resolve everything now. Just the big picture and the important details...

I'm sure we can can up with an arrangement that is amenable to all, and I'm looking forward to hearing back from Keith, Phil, and Chris in particular about the above, so we can quickly move towards finalizing a first draft.

Looking forward to hearing back w/ comments,

mike

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