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Subject: No Subject  
Date: Tue, 13 Apr 1999 15:05:02 -0400 (EDT)  
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Dear Keith

(Tim, please get this to Keith by FAX or other means, if he is unlikely to have received this at his own email while traveling).

It's a good piece overall. As you might suspect, I do have several comments. Ray and Malcolm may send along a few of their own. Malcolm in particular may want to comment on some of your points regarding dendroclimatic series and our ITRDB PC#1 series which figures so prominently in our millennial reconstruction.

1) page 2, top paragraph:

It's is very misleading to make it sound as if we are strictly reconstructing northern hemisphere mean temperature, and then say "4 of the records are actually from the southern hemisphere locations". This is misleading for a number of reasons. First of all, if one is going after true northern hemisphere areally-weighted mean temperature 0-90 degrees (as we are), then the southern hemisphere tropics are actually more relevant than the high-latitudes of the Northern Hemisphere. Careful diagnostics of surface temperature covariances by Alexey Kaplan, Mark Cane and others have shown this clearly to be true. BUT more than that, we are reconstructing the full 20th century surface temperature domain shown in Figure 1 of our '98 Nature paper. This is a GLOBAL domain, albeit sparse outside the southern hemisphere tropics/subtropics, particularly the southern oceans, for obvious reasons. The proxy network roughly overlaps the spatial domain of surface temperature we are reconstructing (ie, compare Nature '98 figure 1a and figure 1b). We choose to diagnose from this spatial domain the northern hemisphere mean only because that is the hemisphere for which we can meaningfully talk about a true hemispheric mean. But both the predictor and predictand have a global distribution. Without going on and on, I think its clear why your comments here are a bit unfair in how they represent why we use southern hemisphere data. This is probably the most important point that needs to be revised here.

2) page 2, 2nd paragraph

A minor point, but an important one: It is incorrect to say the our uncertainties are based only on "a consideration ...goodness of fit...over the calibration period"! This is not correct. A key point is that the verification period (1854-1901) diagnostics (though based on a somewhat sparser distribution of gridpoint data from which NH mean temp can be estimated) give very nearly identical diagnostics in terms of unresolved reconstructed NH mean temp variance. So our uncertainties are based both on 20th century calibration and independent confirmation from 19th century data. PLEASE MAKE SURE this is clear.

On the bigger point being made here, I agree w/ you in principle, and this is a point that Phil has raised too: what we \*DONT\* take into account (though I challenge anyone to really ever be able to take this into account!) is the unknown potential bias due to degradation from diminishing quality of the underlying proxy data back in time. However, on some of the specific points in that regard, it is very likely not a significant concern in our reconstructions. We closely examined the spectra of the underlying proxy data to insure that those upon which our reconstruction ultimately relies have the amount of millennial scale trend/variability that would be expected for a climatic series for at least the null hypothesis of red noise.

Malcolm independently examined the tree ring chronologies underlying our ITRDB PC #1 to verify that the standardization was appropriately conservative for a millennial-scale reconstruction. Furthermore, Malcolm verified that the ITRDB PC #1 is made up of heavily replicated chronologies as far back as we use them. So neither of the points you raise appear to be all that relevant to our reconstruction.

With regard to this point, I have some issues with your Figure that accompanies the piece. It is quite ironic given your comments about the potential impacts of standardization on the long-timescale variations. For our millennial reconstruction we have verified as carefully as has ever been verified, that the millennial scale trend is likely to be meaningful. I don't think you have done so for the 2000 year-long trend in the series you show, and if you have not verified that it is likely to have retained 2000 year long trends, it is VERY misleading to show this series along with the others. I don't believe that it is likely to accurately represent the 2000 year long trend in Northern Hemisphere mean temperature, as you imply by showing it here. I think this series needs to be removed from the plot. I have a related comment below (point #5).

3) page 3, 1st paragraph:

Remove "this is a moot point" and replace with more appropriate language. It is not "a moot point" because the problem you point out has largely been shown to apply to tree ring density data (which you have largely been using), and much less so tree ring width data (which we are using). Furthermore, the comparison only goes through 1980 at which point there is little evidence that there is a significant decline in tree ring width response, although more evidence that there is already a problem at that point with density data. Your criticism is not quite fair here, and the statements should be revised to reflect more accurately on what we have done.

4) page 3, 2nd paragraph:

When you talk about proxy-based ENSO reconstructions, you should mention our NINO3 reconstruction! This is complementary to Stahle's SOI reconstruction in a number of ways. The appropriate references here are both our Nature '98 papers, and the chapter in Henry Diaz's latest book (in the press):

Mann, M.E., Bradley, R.S., and Hughes, M.K.,  
Long-term variability in the El Nino  
Southern Oscillation and associated teleconnections, Diaz, H.F. & Markgraf, V.,  
(eds) El Nino and the Southern Oscillation: Multiscale Variability and  
its Impacts on  
Natural Ecosystems and Society, Cambridge University Press, 321-372, Cambridge,  
UK, in press, 1999.

if you care to, you can download the galley version here:

<ftp://eclogite.geo.umass.edu/pub/mann/ONLINE-PREPRINTS/ENSO-recon/>

in either pdf format (chapter-diaz.pdf) or postscript (chapter-diaz.ps)

5) accompanying figure (see also my point #3):

There are problems with the 2000 year series in terms of your definition of the baseline for comparing with the other series, and this differs quite a bit from what we are likely to be showing in IPCC. It appears that both the density NH reconstruction and your 2000 year long series fall at least 0.1C below the other series during the 20th century, and are probably running at least that much too cold the whole way through.

Also, correct "global temperature and non-temperature proxies" in your description of our series to "global climate proxies" which is a more honest way of describing them given our methodological approach, and make sure it is clear to the readers which series are extratropical and warm season, and which are full northern hemisphere/annual mean estimates (ours). Such discussion will, again, figure prominently in IPCC, and it would be a shame for Science to be publishing something that is misleading in that respect. In part, it was this issue that forced the publication of a followup to Phil's perspective by me, Ray, Malcolm, and Phil a year ago, and it would be nice to avoid that scenario this time around...

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Thanks for your consideration of the above comments. I believe your piece will make an excellent "Perspectives" article for Science, once these comments are appropriately taken into account. I'll leave it to the Science editor in charge to determine if that is the case.

best regards,

mike.

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