

From: "Malcolm Hughes" <mhughes@ltrr.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, "Malcolm Hughes" <mhughes@ltrr.arizona.edu>, Tim Osborn <t.osborn@uea.ac.uk>, "Michael E. Mann" <mann@virginia.edu>
Subject: Re: J. Climate paper - in confidence
Date: Tue, 20 Jan 2004 10:27:09 -0700
Cc: Scott Rutherford <srutherford@rwu.edu>, mann@virginia.edu

Mike - you are right that we should probably leave the network unchanged for this mss. In fact, however, as Keith indicated, the Vaganov data probably retained a fair amount of low frequency because of the use of the corridor method (i.e. were not "heavily standardized"). Cheers, Malcolm
On 20 Jan 2004 at 7:58, Michael E. Mann wrote:

> Thanks Keith,
>
> I agree w/ this--I think the Vaganov chronologies were pretty heavily
> standardized, and the other issues you raise are important. In the
> future, we would (and will) be a bit more circumspect about the use of
> some of these data.
>
> In the present case, however, I think we are forced to use the exact
> same network.
>
> Re, the omission of some results. I think we can probably keep them.
> Simply by cleaning up the text, removing redundancy, etc. I've
> shortened and tightened the manuscript considerably, and I think I've
> improved the logical flow a bit in the process. So my feeling is that
> we will not have to split this up, but I'll leave this to all of you
> to decide after you see the revised draft from Scott and me...
>
> Thanks,
>
> mike
>
> At 09:45 AM 1/20/2004 +0000, Keith Briffa wrote:
> Malcolm seems to have done a good job sorting out these
> constituent sets , and I don't have anything to add other than
> agreeing that as a general principal , where possible, original
> chronologies should be used in preference to reconstructed
> temperature series (the latter having been already optimized
> using simple or multiple regression to fit the target temperature
> series). This applies not only to our western US reconstructions
> (which it should be stressed are based on very flexible curve
> fitting in the standardisation - and inevitably can show little
> variance on time scales longer than a decade or so) but also to
> the Tornetrask and Polar Urals reconstructions (each of which was
> based on ring width and density data , but standardised to try to
> preserve centennial variability - though the density series had by
> far the largest regression coefficients). There is though a
> question regarding the PCs of the Siberian network (presumably
> provided by Eugene?) . The correlation between density and ring
> width can get high in central and eastern parts of the network ,
> so even though these are different variables , it might not be
> strictly true to think of them as truly independent
> (statistically) of the density chronologies we use from the
> Schweingruber network (there may also be a standardisation issue
> here , as the density chronologies were standardised with
> Hegerhoff functions for our initial network work (as reported in
> the Holocene Special Issue) whereas your PC amplitudes may be
> based on "Corridor Standardisation" - which likely preserves less
> low frequency?) . These remarks are simply for clarification and
> discussion , and I too will wait on your response draft , though I
> would throw in the pot the fact that omitting the time dependent
> stuff would simplify the message at this stage. cheers Keith
>
> At 01:42 PM 1/19/04 -0700, Malcolm Hughes wrote:
> Mike - there are the following density data in that set:
> 1) 20 Schweingruber/Frttss series from the ITRDB (those that

> met the criteria described in the Mann et al 2000 EI paper)
> 2) Northern Fennoscandia reconstruction (from Keith)
> 3) Northern Urals reconstruction (from Keith)
> 4) 1 density series for China (Hughes data) and one from India
> (also Hughes data) - neither included in Keith's data set, I
> think. 5) To my great surprise I find that you used the Briffa
> gridded temperature reconstruction from W. N. America
> (mis-attributed to Fritts and Shao) - of course I should have
> picked up on this 6 years ago when reading the proofs of the
> Nature sup mat. It was my understanding that we had decided not to
> use these reconstructions, as the data on which they were based
> were in the ITRDB, and had been subject to that screening process.
> So depending on whether you used the long or the shorter versions
> of these, there will have been a considerable number of density
> series included, some of them twice. It means that there is
> considerably more overlap between the two data sets, in North
> America, than I have been telling people. I stand corrected.
> Cheers, Malcolm . .Malcolm Hughes Professor of Dendrochronology
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