

From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Malcolm Hughes" <mhughes@ltrr.arizona.edu>, "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 09:45:44 +0000
Cc: Scott Rutherford <srutherford@rwu.edu>

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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 Huggershoff 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/Fritts 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
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>.Malcolm Hughes
>Professor of Dendrochronology
>Laboratory of Tree-Ring Research
>University of Arizona
>Tucson, AZ 85721
>520-621-6470
>fax 520-621-8229

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Professor Keith Briffa,
Climatic Research Unit

University of East Anglia
Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909
Fax: +44-1603-507784

<http://www.cru.uea.ac.uk/cru/people/briffa/>

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