

From: tom crowley <tom@ocean.tamu.edu>
To: Chick Keller <ckeller@igpp.ucsd.edu>
Subject: Re: Low Frequency signals in Proxy temperatures:
Date: Sat, 28 Apr 2001 09:54:18 -0500
Cc: tom@ocean.tamu.edu, p.jones@uea.ac.uk, rbradley@geo.umass.edu, k.briffa@uea.ac.uk, mann@virginia.edu, mhughes@ltrr.arizona.edu

Chick,

look at the instrumental record! there are huge differences between different regions - Alaska has warmed substantially while eastern North America cooled after the 1950s. locking onto local records, no matter how beautiful, can lead to serious errors. If the ice cores are so infallible why do they give substantially different stories for grip and gisp2 over the last 1500 years?

the bottom line is that one cannot make a robust case that decadal hemispheric temperatures over the last 1500 years were even as warm as the late 20th century, much less warmer.

Tom

>
>Well said indeed! This helps me to slowly understand what's being
>done and why.
>
>My nagging problem remains however, and that's that there seem to be
>too many paleo records published that show much larger amplitude
>variations. Now many can be explained, but some look more robust.
>For example I think most people are wondering about the total
>disagreement between isotope temperatures from GISP II and borehole
>temperatures from GRIP and Dye 3. Here the usual land use caution
>doesn't apply since I don't think the ice above the boreholes has
>changed much?
>
>And if I understand Tom Crowley's note to me, his reconstruction
>averaged normalized records, thus missing large amplitude variations
>such as the Keigwin Sargasso one, which he used, but which shows a
>large amplitude signal tantalizingly similar to the GRIP/Dye 3
>records. (Tom used GISP II which essentially has no low frequency
>amplitude)
>
>So I read all the papers, and am impressed by the painstakingly
>careful work, but still wonder about a world in which the
>hemispherical low frequency temperature amplitude could be (see Jones
>et al Science this week) only about 0.4°C between 1000 and about
>1950, while parts of the world could have seen amplitudes of up to
>2°C in the same period. I suppose you could say that, given natural
>forcing only, there can be much larger variance from the mean
>(spatially and temporally) than in the past hundred and fifty years
>when GHG forcing is forcing more uniformity, but does this make sense?
>
>This is why I keep asking questions about the ability of various
>proxies to return low frequency information.
>
>Anything you could say about this would be greatly appreciated.
>
>Finally consider this. I read recently (don't know the pedigree of
>this number but it WAS published!!) that Milankovitch cooling at this
>point in the Holocene should be about 0.4°C/millennium (other plots
>I've seen would suggest about 2.3 to 1/2 of that). If that's true,
>then all the cooling since the year 1000 is Milankovitch and there's
>no room for variations in solar activity and multiple volcanic
>eruptions. Now I'm not saying this is the best way to think about
>such things, but it does remind us that much of the cooling seems to
>have been due to Milankovitch, and, given the small amplitude of the
>proxy records, that is a bit worrisome. What do people think about
>this?

>
>Regards,
>
>
>Well said Malcolm...
>
>mike
>
>p.s. Chick: You might want to check out the review article by Jones
>et al in the latest Science...
>
>At 01:16 PM 4/26/01 -0700, Malcolm K. Hughes wrote:
>>Dear Chick - some thoughts on a couple of the points you raised,
>>Cheers, Malcolm
>>1. There is no reference to the ABD in MBH 98 and 99 because
>>the technique
>>was not available at that time - see the dates on Keith's publications that
>>describe it.
>>2. There are significant regions where the ABD method is not needed,
>>because the trees live much longer than those in the Schweingruber
>>network that
>>Keith has been using, and grow under conditions that make only very
>>conservative
>>standardization necessary. There is a growing body of evidence that these
>>tree-ring records can capture century-to-millennial change accurately (Hughes
>>and Graumlich, 1996 and Hughes and Funkhouser 1998, for example). In
>>fact, the
>>MBH reconstruction before AD 1400 was largely based on these.
>>3. Keith has pooled information from extremely large regions
>>(presumably to
>>get large enough samples), whereas we (MBH) have been particularly
>>interested in
>>spatial variability, ruling out the use of ABD.
>>4. The ABD method is new, needs testing, and, I predict, will be
>>modified
>>as it is tested.
>>5. The benefit of annual resolution is that direct calibration and
>>cross-validation against instrumental records is possible with a
>>high degree of
>>rigor. We are relaxing this condition somewhat in our ongoing analyses,
>>and it
>>will be interesting to see how the uncertainties increase as one includes
>>records with poorer temporal resolution. This is an issue that the
>>advocates of
>>such records do not address, so far as I can see.
>>
>>
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>Is the noticeable increase in surfers off Scripps Beach a possible
>indication of global warming?

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