Clim. Past Discuss., 5, C1077–C1080, 2010
www.clim-past-discuss.net/5/C1077/2010/

Climate of the Past Discussions

Interactive comment on "Refining error estimates for a millennial temperature reconstruction" *by* M. N. Juckes

Anonymous Referee #1

Received and published: 19 January 2010

This paper presents the rather ambitious endeavor to estimate numerically certain key statements of the IPCC regarding the 'likelihood' of the present climate being exceptional. According to the author, the results confirm the main conclusions of the IPCC, namely, that the late 20th century temperatures were likely to be exceptionally warm in at least a millennial context.

As the study is largely written in jargon style and in part quite sloppy, I am not 100% sure that I got all the details. But from what I understood I must conclude that the uncertainty estimation is incomplete, and that the study is in fact inappropriate for publication in CP. It seems that the author was unaware of the fact that the relevant IPCC statements are 'judgmental estimates of confidence', and that they were not meant at all to be numerical estimates.

The study is a follow-up to JAB2007 where the millennial NH temperature was reconstructed using a method known as 'composite plus scaling' (CPS), and uses an almost identical set of proxies. That means, first, a composite is formed (= average of equally-weighted proxies) and, second, that composite is scaled to match the instrumental temperature variance. The combination of both steps is then analyzed wrt. uncertainty.

In short, this goes along the following lines: There is the jackknife uncertainty of the composite C, and the structural uncertainty of the scaling, s. The uncertainty of the reconstructed $T = s * C$ is then a simple product of the single uncertainties of s and C (Eq. 6).

A closer look, however, reveals that the actual reconstruction uncertainty enters the equations at no point. Suppose, for the matter of demonstration, that all proxies are perfectly coherent, and that $T = C + \text{white noise}$. In that case, both C and s are perfectly known so that the uncertainty of T is zero, regardless the scale of the noise. In other words, no uncertainty for T is allowed other than resulting from incoherence in the proxies.

This is actually acknowledged in the paper: 'The uncertainty thus reflects inhomogeneities in the proxy data...'. But when it continues: '... and the extent to which the true NH-mean temperature variations are captured by the retained data.' this is not true.

But even if it were true there are still a number of other issues:

a) (Abstract) I would have greater confidence in the statement that the end of the 20th century was exceptional, than in the statement that it was the warmest in a millennium.

b) Visually, the 11th century in this paper shows actually the warmest of all **reconstructed** temperatures (based on the 95% pctl. of figures 5, 6, 8, 9, and 10). Now compared to those the maximum **instrumental** temperatures are of course higher, but

CPD
5, C1077–C1080, 2010

I doubt that this is a fair comparison. To be fair it should at least be demonstrated that reconstructed temperatures can reach amplitudes as high as +0.7 (e.g. for the year 1998), a comparison that is however countered by the divergence (or general lack) of recent proxies. (Obviously, this argument applies not only to the current study.)

c) There is a general lack of clear definitions in the study, such as what calibration period the analysis is based on, or what 'reasonable bounds' are in the structural uncertainty estimates, etc.

Minor details:

(2632, 9) 'likelihood that T were likely...'?

(2634, 18) 'unscreened' for what?

(2635, 17+) You don't explain why the proxy selection is 'a priori'.

(2636, 20) time mean of what (probably $p_i' * p_j'$)?

(2637, 3) what means consistent with random variations. What is this analysis for?

(2637, 6) There are 2 things noteworthy here: a) focusing on positive correlations explicitly contradicts the climate field reconstruction approach. b) Using a 50 years timescale seems in accordance with Anchukaitis, but not with Wahl et al. (2006) who claim that temperature-proxy couplings are revealed mainly on centennial timescales. - The calibration period is undefined.

(2638, 4) This paragraph is somewhat obscure. First, one wonders why R15 differs so much from JAB2007 although both use an almost identical set of proxies. Second, what means 'well within any reasonable uncertainty estimate'. If it means they are statistically indistinguishable, then I conclude that it is well possible that around the year 1100 it was warmer than present. And third, I don't think this figure tells anything about a correlation of uncertainties.

(2640, 24) Eq. (2) is unreadable (what is d bar?).

CPD

5, C1077–C1080, 2010

(2641, 15) Very cryptic.

(2642, 9) Where does Eq.(3) come from?

(2643, 17) This is awkward. Would it not be simpler to obtain a Jackknife uncertainty estimate for the unscaled composites and combine it with the scaling uncertainty? - Eq. (5) seems to imply that f_j is independent of γ_{vm}.

Interactive comment on Clim. Past Discuss., 5, 2631, 2009.