Clim. Past Discuss., 7, C1362–C1366, 2011
www.clim-past-discuss.net/7/C1362/2011/

Climate of the Past Discussions

Interactive comment on "Rates of global temperature change during the past millennium" *by* C. Shen et al.

Anonymous Referee #1

Received and published: 30 August 2011

The manuscript focuses in global temperature series of the 20th century and the past millennium and analyses the multidecadal trends present in these different observational and modelled data sets. The main aim is to establish in which circumstances the multidecadal trends observed in the 20th century stand out clearly from the background of trends in the past millennium. The analysed data are the GISS temperature data set for the last decades, the paleo-climate reconstructions by Jones (1998) and several simulations over the past millennium driven by different forcing configurations.

I see several serious problems in this manuscript, as I explain below.

1) The authors analyze only one millennial reconstruction (Jones, 1998) with the argument that it is the only one available. This is incorrect. In the NOAA paleoclimatology page at http://www.ncdc.noaa.gov/paleo/recons.html, many more reconstructions are

available. Although the Jones reconstruction was pioneering, it is based on a much smaller proxy data sets and produced with a much simpler statistical method than more recent ones. There is no justification, in my opinion, of using the Jones reconstructions and ignoring all others. Probably, all of them should be included in the analysis, since the uncertainties are still large and it is not easy to identify which one is better than others. There are significant divergences among the various reconstructions and the results of this study might be strongly dependent on which one is chosen.

2) The technical details of the simulation are not well described, specially the external forcings used to drive the climate model (GCM). The manuscript refers to Peng et al, but here also this external forcing is not satisfactorily described. For instance, Crowley (2000) presented 3 different reconstructions of past solar forcing. Which one was used here? Also, solar forcing cannot be used to drive a GCM, it requires as input the solar irradiance. How was the reconstructed solar forcing translated to solar irradiance. Crowley (2000) includes estimation of the global volcanic forcing. how was this forcing used to drive the GCM ? Since the external forcing strongly determines the low-frequency variability of the model, and therefore the multi-decadal trends, a detailed description of the external forcing used is important.

3) In the abstract, and through the manuscript, the authors state that the effect of the SSTs on the global mean can be included or filtered out by filtering the 50-80-year oscillation present in the observed record. I have problems accepting this without further justification. Actually, the concept that SST drive the air temperatures in a coupled system is very simplistic. Although some authors argue that this oscillation is originated in the internal climate dynamics, I do not think one can separate the upper ocean layers from the atmosphere, or categorized the ocean as driver in this oscillation and the atmosphere as a passive subsystem. Also, the period of 50-80 years has been determined in the short observational record. There is no guarantee that this period remains unchanged through the past millennium, or even that this oscillation existed also in the past. It has not been shows either that this quasi-oscillation is present in the

model for the same reasons as in the observations.

Even accepting that the 50-80 quasi-oscillation is entirely caused by internal mechanisms and may '50–80-yr cover major part of low frequency variability in SST variance' (this is a very vague statement), I fail to see why this oscillation is so important for the goals of this manuscript. At 20-year time scales internal variations do play a role as well, and so it cannot be claimed that by filtering out the 50-80 oscillation the effect of the ocean is filtered out. Actually, depending on the relative amplitudes of forced trends and internal variability, it is not clear at which timescales the effect of internal variability is more disturbing to estimate the uniqueness of the 20th century trends.

4) In general terms, the manuscript is intelligible, but not very clearly written. The conclusion section seems to have been hastily formulated and some paragraphs elsewhere require some clarification.

Further points:

4) the present (Solomon et al., 2007; Swanson et al., 2009). Given on a uniform time scale, similar warming rate to that of the last 50 yr might occur in the early 20th century. Therefore, it is diïňČcult to assess how unusual the warming rate for the last 50 yris in the context of millennium without using a uniform time scale in the computation of temperature change rates. Rates of global temperature calculated on uniform time scales are thus essential for assessing this issue.'

This paragraph is unclear. It becomes a bit clearer after reading the manuscript, but I think it could be formulated more clearly here.

5) '50–80-yr oscillation is statistically signiïňĄcant multidecadal signal in observational global surface air temperature (Wu et al., 2007). '

It is debatable that a quasi-oscillation with a period of 50-80 can be really detected in the short observational record, although these results may have been published elsewhere. Nevertheless, the words 'statistically significant' require the prescription of

a null hypothesis, for instance that the time series are gaussian white noise or similar. The very same oscillation may be statistically significant or not depending of what is the 'default' behaviour. The null hypothesis is as important as the the statement that the oscillation is statistically significant.

6) 'Figure 1a shows the variation of global land-ocean surface temperature from 1880'

annual mean surface temperature anomalies

7) 'Figure 1a shows the variation of global land-ocean surface temperature from 1880 to 2009. In its original temperature time series, negative anomalies occurred before'

which the reference period to define positive and negative anomalies?

8) 'after 1978. 50–80-yr oscillation is statistically signiïňĄcant multidecadal signal in this time series. Its wavelet ïňĄltering show that 50–80-yr oscillation accounts for 24.6 % of the total variance of this time series.'

see my previous comment on statistical significance

9) Fig. 1 shows the gliding linear trends with their confidence intervals. A bit more detail is needed here. How were the linear trends estimated (I assume by linear regression on time). More importantly, the manuscript should explain how the confidence intervals have been estimated . I would assume that the authors have not taken into account the possible autocorrelation of the residuals, since they do not mention it. However, in the global temperature series the residuals of a fit to straight line are quite likely seraphically correlated, which invalidates the 'usual' estimation of the confidence interval for the trends. If this is true, the confidence intervals shown in figure 1 are too narrow, depending on the serial correlation of the residuals. This is important because the manuscript discusses the position of the maxima and minima of the trends and their differences to those calculated after the global temperature has been filtered. The amplitude of the confidence intervals is here critical.

10) 'and excluding the low frequency oscillation are similar to that on the climatological

time' That the climatological time scale is 30 years is perhaps not clear to every one.

11) 'Figure 2a shows observational and reconstructed global surface air temperature. Dur- ing their overlap time period (1880–1991), reconstructed temperature closely matches the observational temperature in both magnitude and temporal evolution with a sig- niïňĄcant correlation, suggesting that this reconstruction is reliable. '

This is a very risky assertion. After the Jones (1998) reconstruction was published, many others are available, as indicated before, and all of them of course agree with the observations in the 20th century (up to 1980), and yet they may diverge in the past centuries (although the basic multicentennial shape is more or less similar, they disagree in many details such as the amplitude and timing of variations)

Interactive comment on Clim. Past Discuss., 7, 2341, 2011.