Point-to-point response to reviewers' comments

Reviewer #1

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 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 divergencesamong the various reconstructions and the results of this study might be strongly dependent on which one is chosen.

In our original manuscript, we have indicated explicitly that the reconstruction of Jones et al (1998) is the only available "*non-filtered*" global mean surface temperature reconstruction "*covering the past millennium*". Surely, we know many other reconstructions listed in the NOAA paleoclimatology page, however, they are either filtered or shorter in length covering only a few centuries.

2) The technical details of the simulation are not well described, especially 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.

We have added more descriptions of model simulation related to these questions. The solar forcing used is "Be10/Lean splice" in Crowley (2000). It is converted into solar irradiance by the solar constant (1365 Wm^{-2}). The global volcanic forcing is applied as negative deviation from solar constant.

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 drives 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.

To address the 50-80 years oscillation issue raised by the Reviewer, we used multi-taper method to analyze the 1000-yr reconstructed and modeled temperatures. Each time series is first pre-processed by a cubic polynomial best-fit and then 21-yr moving average to remove their trend and high frequency oscillations (>0.05/yr). We find that statistically significant low frequency oscillation with period 54-91-yr and 49-91-yr exists respectively in reconstructed and modeled temperatures, both at the 95% confidence level (red noise null hypothesis). These results are included in the revised manuscript. With regard to the Reviewer’s other remarks, we feel they are out of the scope of the present study.

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 difficult to assess how unusual the warming rate for the last 50 yr is 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.

We rewrite this paragraph.

5) ’50–80-yr oscillation is statistically significant 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’ behavior. The null hypothesis is as important as the statement that the oscillation is statistically significant.

We rewrite this sentence as “A multidecadal (~65-yr period varying from 50- to 80-yr) oscillation has been found in observational global surface air temperature (Wu et al., 2007, 2011).”

6) 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?

1961-1990.

7) ’after 1978. 50–80-yr oscillation is statistically significant multidecadal signal in this time series. Its wavelet filtering 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.

We rewrite this sentence as “the signal of 50–80-yr oscillation is substantial in this time series. Its wavelet filtering shows that 50–80-yr oscillation accounts for 24.6 % of the total variance.”

8) 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.

Indeed, we estimated linear trends by linear regression on time and did not consider the possible autocorrelation of the residuals. We have conducted an autocorrelation analysis on the residuals of observational data. The results show that significant autocorrelation exists in 11%, 35%, and 88% of sliding windows on 20-, 30-, and 50-yr time scales, respectively. Having considered that our interpretation is mainly based on 30-yr time scale and the confidence interval does not affect our interpretation significantly, we don’t recalculate the confidence intervals for a consistency. However, we address this issue in the figure caption of the revised manuscript.

9) ’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 everyone.

We change the climatological time to 30-yr time scale.

10) ’Figure 2a shows observational and reconstructed global surface air temperature. During their overlap time period (1880–1991), reconstructed temperature closely matches the observational temperature in both magnitude and temporal evolution with a significant 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 multi-centennial shape is more or less similar, they disagree in many details such as the amplitude and timing of variations).

We rewrite this paragraph.

Reviewer #2

The manuscript ‘Rates of Global Temperature Change during the Past Millennium’ by Shen et al. presents an analysis of instrumental, reconstructed and simulated global temperature series. Although the topic is interesting, the manuscript is not well written and some fundamental choices are questionable. There is a lack of a clear structure and paragraphs are not well connected. Several parts have to be revised. Implemented/applied methods and procedures are not properly described in the manuscript as well as simulations. The removal of the 50-80-yr oscillation is questionable and not supported by an adequate explanation and/or additional analysis. In the cited references, the aforementioned oscillation was identified using time series from _1850. Therefore, it has to be clearly proved that this oscillation characterizes the 1000-yr series.

We rewrite the implemented/applied methods and procedures as well as simulations. More detailed descriptions are added in the revised manuscript. We have conducted spectral analysis on 1000-yr reconstructed and modeled temperature time series. The results indicate that the 50-80-yr oscillation characterizes both time series. (Please also see the response #3 to reviewer 1).

Some Specific Comments

29: Rephrase this sentence.

We rewrite the sentence as “The analysis focuses on the rates’ characteristics of the 20th century within the context of the past millennium as well as their sensitivity to the low frequency variability of sea surface temperature (SST) and time scales.”

51-56: Revise the entire paragraph.

We rewrite the paragraph.

76-78: Revise this paragraph.

We rewrite the paragraph.

85-86: Please make a link between the two paragraphs.

We rewrite the second paragraph.

87-89: Schlesinger and Ramankutty (1994) identified a 65-70-yr oscillation in global mean temperature lying within the timescale band of these regional oscillation, 50-88-yr.

We rewrite this sentence.

89-92: Revise the paragraph.

We rewrite the paragraph.

91-95: this is an oversimplification.

We have added more descriptions of wavelet filtering.

100-104: Revise this paragraph.

We rewrite the paragraph.

117-118: Connect this part with the rest of the paragraph.

We rewrite this part.

131-134: Clarify this statement.

We rewrite this statement.