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Climate of the Past Discussions

Interactive comment on "Causes of Greenland temperature variability over the past 4000 yr: implications for northern hemispheric temperature change" *by* T. Kobashi et al.

Anonymous Referee #2

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The paper "Causes of Greenland temperature variability over the past 4000 yr: implications for northern hemispheric temperature change" by Kobashi et al. attempts an attribution of measured high-precision Greenland temperature as well as modeled northern hemispheric temperature anomalies to various climate forcing factors. Unfortunately, I am not convinced that the paper sufficiently supports the claims. This may be partly due to the fact that the paper is structurally very hard to follow, is overloaded with details that do not contribute to the claims and mainly because of the weak statistical significance of the results. In the following I will highlight the most important difficulties I experienced with this manuscript and will also list more specific comments and questions that arose when reading the paper. In summary, I cannot recommend publication

of this paper in CP in its current form, but may see potential for an improved manuscript after getting rid of most of the ballast and by providing a more stringent discussion of the time series analytical and statistical approach. In the end the statistical significance of the results must be sufficient to justify the approach and the conclusions.

General comments

1. The paper relies on high-precision temperature reconstructions using d15N2 measurements on ice samples from the Greenland GISP2 ice core. Unfortunately, these data have been published before, so the paper cannot take credit for this high quality data set but only for new insights from the discussion of the data that have not been gained before.

2. Some subsections of the paper compare the d15N2 derived temperatures with proxy reconstructions from Greenland stable water isotope records. In the end none of this is used for the discussion of the ultimate goal of the paper, i.e. the attribution of temperature changes to individual forcing factors. Accordingly, this discussion of the proxy records should be taken out in a future manuscript.

3. The time series analysis (see also specific comments below) relies heavily on the statement that the Greenland temperature anomaly (GTA) is negatively linked to solar irradiance changes. In a first step a GTA is calculated by using the results from a 1-dimensional energy balance model incl. solar forcing (TSI). This GTA record shows significant correlation with the TSI record. Such a correlation is not surprising as the TSI is intrinsically embedded in the model result as the authors admit. In a second step they, therefore, did a model run without solar forcing to derive a GTA. This record, however, is then barely correlated to the solar irradiance (the joint variance is smaller than 5%). For me this essentially shows that Greenland temperatures exhibit no solar influence at all, questioning the following discussion in the paper (see also specific comment below). In a revised version of the paper, the authors have to make a statistically more convincing case for their interpretation. Note that this implies that dependent on

this result the question is raised whether the Greenland temperature record improves the forcing attribution at all. Ultimately, this could imply that the whole exercise could be done without the Greenland data.

4. Another fundamental issue is the volcanic forcing record as derived by the authors. The authors state that the atmospheric e-folding time of stratospheric SO4 aerosol is on the order of 1 year, implying that any climate forcing of a single volcanic events is restricted to a few years. Nevertheless, the authors calculate a 50 year running mean of the volcanic forcing, thus widening the temporal influence of one eruption beyond its potential impact window. Among others, this leads to the artifact that a volcanic forcing is taking place even before the volcano eruption occurred. Also later the discussion of the volcanic forcing is not stringent enough with respect to the question whether the solar forcing is coming about by a higher frequency of volcanic events in a given time window, or whether one extraordinary volcano leads to a strong forcing. This discussion has to be improved in a future manuscript.

5. Finally, the overall structure and the explanation of the step-by-step statistical approach are not conducive for bringing the message across. I have to admit I got confused several times which records are discussed and how exactly these records have been derived. A clear separation of the statistical approach from its discussion in separate chapters and a clear labeling in the figures in a future manuscript would help to resolve this. In general there are way too many figures, where many of them are not required (see suggestions below).

Specific comments

Abstract

The abstract makes the claim that these data are more precise than other reconstructions. The quality of the data should be quickly reviewed in the method section

The statement that forcing factors can explain (only) 10% of the temperature variability

in the Greenland record is not very reassuring. Taken at face value this would imply that 90% of the temperature variability is related to natural variability.

Introduction

In the last paragraph of the introduction the authors make the statement that additional information for hemispheric temperature reconstructions can be gained from the understanding of the Greenland anomalies. I completely agree with the authors. Unfortunately, the manuscript in its present state does not provide enough statistical base to allow for such an improved understanding of the GTA.

On page 4820 the authors make the claim that their Greenland record is amongst the most precise temperature reconstructions available, but this claim is not supported in the method section. In this respect, especially the potential introduction of systematic errors due to the use of a firnification model has to be discussed. As only Holocene samples are discussed, this issue is probably not so important for this study. In essence, I agree totally with the authors on the quality of their data but this has to be discussed in the method section. Quantifying the size of the error in the temperature reconstructions is also essential for the significance of the attribution of these changes to individual forcing factors.

Section 3.3. I would suggest to delete this section as it does not contribute to the overall question of this paper

Page 4830 represents one of the major problems of the manuscript. The authors state that Greenland temperatures are positively correlated to TSI with r=0.1 and negatively correlated with r=0.08 after detrending the record. This implies that less than 1% of the variance in the Greenland temperature record can be attributed to the TSI variation. In my opinion, these numbers provide not sufficient grounds for any further discussions.

Section 4.2: I would suggest to delete this section as it does not contribute to the overall question of this paper

On page 4835 the authors use a global average CH4 concentration to quantify its forcing in the northern hemisphere. This seems inconsistent with the local forcing of CH4 in Greenland and to some extent also for the northern hemisphere. I would advise to use only the Greenland CH4 record here.

Page 4838 deals with the discussion of the volcanic forcing. See my comments above on the use of a 50 year running mean and the distinction between higher volcanic frequency and single events. Accordingly, the statement on page 4839 (line 12) may be reflecting a time series analysis artifact, as their volcanic forcing record is a low-pass filtered version of the true volcanic forcing, thus transferring power from the multiannual time scale to the multidecadal time scale.

Page 4841: Again, the statistics doesn't convince me that the solar forcing is really negatively correlated to the GTA. Also the use of the hemispheric scaling factor of 1.21 for the 70-80° latitude band cannot be taken for granted. Even if a negative correlation may exist, the scaling factor may be different for individual latitude bands. Can you give the correlation of the measured Greenland temperature and the modeled temperature in the 70-80° latitude band without the solar correction? I assume the correlation will be comparably high.

Section 5.4. The statement in the first sentence of this section is entirely due to the use of a 1d EBM. In this case, all latitudinal bands have to behave similar in their non-orbital components.

Page 4845: The existence of Bond cycles is controversially discussed in the literature. In any case appropriate references to this literature should be given.

Finally the last paragraph in section 5.4 does not really contribute a lot to the goals of the paper and could be deleted.

References:

The Kobashi et al., 2012 paper has been published in CP and is not in press in JGR. . .

Tables 1, 2, 3 can be deleted, after taking out the discussion of the d18O proxy records.

Figure 1: Instead of showing the Kobashi record over the last 1000 years, the entire record should be shown

Figure 2. The poor resemblance of the Box correlation map and the Kobashi correlation map is questioning the significance of the results.

Figure 3 could be deleted

Fig 6 & 7 can be deleted after taking out the d18O proxy discussion

Fig. 8 can be deleted as theses records are published in individual papers and are used in later figures as well.

Fig. 10 is hard to read. Color blind people will have no chance to distinguish the individual lines

Fig. 16 is redundant to previous figures and can be deleted

Fig. 21 can be deleted if the discussion on Dongge cave is taken out.

Interactive comment on Clim. Past Discuss., 8, 4817, 2012.