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Climate of the Past Discussions

Interactive comment on "On the origin of multi-decadal to centennial Greenland temperature anomalies over the past 800 yr" *by* T. Kobashi et al.

Anonymous Referee #1

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GENERAL COMMENT:

In this paper, the authors address a relevant research topic, the causes for Greenland climate variability in the last 800 years and its deviation from the global pattern of response. Improving our understanding of the reasons for this different Greenland variability, which shows limited sensitivity to the external forcing, is undoubtedly a question of great importance. The analysis also introduces a new approach to isolate the regional aspects of Greenland temperature variability by defining and analysing a Greenland temperature anomaly (GTA) with respect to the background climate variability. This procedure is novel and interesting. But apart from these positive aspects, I

find that the analysis in its present form lacks some rigor and is rather limited in scope. Physical conclusions are sometimes quite loose and not always well supported. Before being suitable for publication the authors should strengthen the manuscript in a few major points.

As a major issue, the analysis is almost exclusively focused on the influence of solar activity on the GTA. This is justified in Section 2 of the article by a weak correlation coefficient between both variables in the period 1960-2007 (r=-0.29), which is not even significant at the commonly used 95% confidence level, and that explains less than 10% of the total GTA variance. Furthermore, both the correlation value (r=-0.27) and its significance (p=0.35) become even smaller when the whole observational period is considered: 1851-2007. This low correlation cannot be used as a proof of a causal relationship between the GTA and the solar forcing, neither to justify the later analysis with proxy evidence (which is subject to much larger uncertainties). Indeed, most studies relate Greenland climate variability to internal processes such as the NAO (Appenzeller et al., 1998; Vinther et al., 2003), for which this study shows higher correlations with the GTA (-0.44, P. 5459 L. 29), and also to the effect of the AMO (Chylek et al., 2011) or the North Atlantic Blocking (Rimbu and Lohmann, 2011) rather than to the influence of the external forcings. I would expect most of the externally forced signal to have been removed when the background Northern Hemisphere temperature (that captures the hemispheric signal of response to the forcings) was subtracted. For the last 800 years, two different reconstructions for the Northern Hemisphere temperature were employed, while only one for Greenland temperature. A caveat of this latter record from Kobashi et al. (2010) is that it is only decadally resolved and has a short overlap period with observations. I think that the analysis will benefit with inclusion of other proxy records with higher resolution (See comment 13). This will be essential to asses the robustness of the current findings.

Another major issue is the lack of support for several conclusions. For example, in the abstract the authors suggest a link between solar changes with both the atmospheric

circulation and the Atlantic meridional overturning circulation (see Comments 3 and 4). However, no actual analysis on the relationship between the corresponding quantities is done in the manuscript. Nor a proper explanation of how the influence takes place is provided. In this respect I suggest the authors to use at least lead-lag correlation analyses to support the physical links proposed.

Finally, I found some parts of the manuscript hard to read. The use of english is not always clear and concise, thus affecting the general understanding of the text. I would also recommend to double check the use of parentheses (See Comment 29). Regarding the paper structure, I also have some suggestions to better guide the reader throughout the text (developed in detail in Comment 10).

Therefore, I have to recommend to reject the paper in its present form. I strongly encourage the authors first to reconsider their study under a more comprehensive focus giving more weight to the contributions of internal climate dynamics as well as including other proxy sources for Greenland Temperature, and second to be more cautious and rigorous in the interpretation of statistical analyses (such as correlations and wavelet coherence) and their significance assessments. In the following, a list with other minor comments is detailed:

SPECIFIC COMMENTS:

Comment 1 (P. 5456 L.9-12 and P. 5459, L. 18-20) I find that the decomposition and assumptions to calculate the Greenland temperature anomaly are not clear enough. In the Appendix A, the authors explain that they used standardized temperature time series (previously smoothed with 3 year running means) to directly compute the GTA. Even if in practical terms this calculation is valid, I feel it hides some base assumptions regarding the polar amplification. By standardizing what the authors do is to assume that the difference in magnitude between the NH and Greenland temperatures is only due to polar amplification, and by filtering at high frequencies they also assume that this

amplification only affects the lower frequencies. As assumptions they should be clearly stated in the text. In either case, I would recommend a slightly different approach for the calculation of the GTA. Indeed, I think the actual formula used to compute the GTA should appear at some point in the text (something like $GTA = T_G - \alpha_p T_{NH}$). It would be helpful for the reader as it illustrates the relationship between the different quantities. Following the same two assumptions mentioned before, the polar amplification factor α_p can be easily computed as the ratio between the standard deviations of the Greenland and NH low-pass filtered temperatures. And once this value is calculated, the GTA is directly obtained following the previous formula.

Comment 2 (P. 5456, L.14-18 and P. 5462, L. 23-27) The GTP is not orthogonal to the Greenland and NH temperatures, but to the GTA. By definition, two time series are said to be orthogonal when the are uncorrelated. The GTA and GTP should meet this condition, since they are defined respectively as the addition and subtraction of two standardised timeseries (the common term goes in phase, and the opposite in antiphase, thus canceling out in the correlation). But I do not see why the GTP should be uncorrelated to the individual temperature records. The authors also claim that the GTA is a principal component of the same two timeseries. Did they actually perform a principal component analysis to calculate this GTA? This part should be better rephrased.

Comment 3 (P. 5456, L.18-21) No actual proof of a link between solar variability and atmospheric circulation has been shown in the article. Not even from Fig. 9 (See Comment 18). As already said before, the authors should be more careful with the inferences of causality, especially in the abstract where the major findings are summarized.

Comment 4 (P. 5456, L. 21-23 and paragraph starting in P.5465, L. 26) The link with the AMOC has not really been proved. Are the AMOC and the GTA significantly correlated (in-phase or at any given time lag)? The authors only showed that there is a rather small weakening of the AMOC (0.3 Sv) in response to solar forcing. Indeed, it is not

clear how this value has been obtained. Is it just the difference in the mean AMOC state between to periods of weak and strong solar forcing? In that case it should be better explained. Also, a significance test for this value would be helpful. In either case, the statement supporting the link between the AMOC and the GTA should be removed unless further evidence is provided.

Comment 5 (P. 5457, L. 8-10): How can a local temperature record fluctuate inversely to a spatial pattern? I believe that the authors are trying to say that Western Greenland temperature is anticorrelated with the NAO. Please rephrase.

Comment 6 (P. 5458., L. 4-7): Fig. 1 in Shindell et al. (2001) shows a warming in the northern North Atlantic during a period of weakened solar activity, but its effect over Greenland is reduced to its very southern edge (Cape Farewell). I would not mention it here as the overall signal over Greenland is clearly a cooling.

Comment 7 (P. 5458 L. 18 to P. 5459 L. 18 and further on throughout the text): Discussion is established in terms of temperature trends, when it seems more related to interdecadal/multidecadal changes in temperature. It is rather confusing as these "trends" are compared even after applying a linear detrending (P. 5459, L. 7). I suggest to rephrase and keep the use of the term "trend" to refer exclusively to linear trends over a complete time period.

Comment 8 (P. 5458, L.7-9): Some information is missing. For what particular variable the NAO-like pattern is observed (I assume SLP changes)? And for which data source? models? proxies? Also, I suggest to remove "solar-induced" as the spatial features are inherent to the NAO/AO pattern itself and not specific to the effect of any particular forcing.

Comment 9 (P. 5459, L. 6-18): Correlation analyses and their significance are not always satisfactorily discussed. For example, in P. 5460 L. 8-10 : "GTA correlates weakly but significantly with the 11-yr solar cycle over the past 48 yr (r= -0.29, p=0.09 for the 1960-2007 period) (Fig. 1, middle panel; however, the correlation is not significant -r

=-0.27, p=0.35; Appendix A - for the entire observational period 1851-2007)". What is the exact criterium to decide the significance or not of the correlation values? If it is exceeding the p-value of 0.10, it should be at least clearly stated at the beginning. I also wonder if the effect of autocorrelation is taken into account in the calculation of the effective sample size. If not, it could affect substantially the significance level of the correlation values. To conclude, please see Comment 29 regarding the use of em dashes "-" inside parentheses.

Comment 10 (Section 2): I strongly recommend to include a section with a detailed description of the different datasets (observational records and proxy reconstructions for Greenland and NH temperature, the GISS-ER model and the simulations) just after the introduction. The different descriptions are currently scattered throughout the manuscript (sections 2, 3 and Appendix B, F) and not always precede the respective discussion of results. However, this information is crucial to fully understand to what extent those results are robust, and if there are constraints on their validity. I would suggest at least to move the descriptions from the Appendix to the beginning of the corresponding sections.

Comment 11 (P. 5460, L.1-7): I do not understand why the solar signal would be in opposite phase with the GTA? What is the physical mechanism that would explain that kind of counter-intuitive behaviour (more radiative forcing producing lower temperatures)?

Comment 12 (P. 5460, L.8-9): Was the 11-yr cycle really isolated? If it was, the authors should specify how this was done (applying a band-pass filter?). If not, it should just be called solar variability. I agree that the major contribution comes from the 11 year period, but other modulations at lower frequencies are also present (for example an increasing trend in the TSI can be seen in Fig. 7 from year 1970 onwards) and also contribute to the final correlation.

Comment 13 (P. 5461, L. 9-20): To give more robustness to this analysis, other

paleo-reconstructions for Greenland temperature could have been included. For example, the annually resolved winter δ^{18}O stack from Vinther et al. (2010), that shows significant correlations with local temperature observations (r=0.56 for the period from 1785 to 1970 with unfiltered data). Note that even if this winter δ^{18}O record (accessible from: http://www.iceandclimate.nbi.ku.dk/data/Vinther_etal_2010_data_02feb2010.xls/) is not expressed in term of temperature changes, it can still be used to reconstruct the GTA since Greenland temperature is introduced as a standardised (and therefore dimensionless) variable.

Comment 14 (P. 5461, L. 11-12): How were these percentages calculated? Through the square of the correlation coefficient? Does it imply that the background climate explains the other 65-69%? This result would be surprising to me as Greenland temperature variability deviates considerably from the Northern Hemisphere signal (see Fig. 1).

Comment 15 (P. 5461, L. 23-28): This is not a common wavelet analysis, which is used to assess the variability of one only particular timeseries. This is actually a cross-wavelet transform analysis (as properly said in the caption of Fig. 4) investigating the spectral coherence between two different timeseries. Also, the subsequent discussion is not fully convincing. I remind the authors that this technique should be used and interpreted with caution (Grinsted et al., 2004). Thick black contours in Fig. 4 only indicate that the two timeseries vary similarly at those particular timescales, but has no implication for causality. The analysis of phase relationships can help to support or discard the potential physical links between the two quantities. But cannot be used as a definitive proof of any mechanism. In either case, I disagree with what the authors claim to be an antiphase relationship between the GTA and the TSI around the period 128 yr. Fig. 4c shows a clear shift in the phase relationship at that particular frequency before and after 1500, with arrows pointing upward in the first period, and completely horizontal in the second. And in Fig. 4e arrows point downward from year 1500 to 1600. This means that sometimes the GTA lags the TSI (which is physically consistent)

but also that sometimes the GTA is leading (which cannot be based on a physical mechanism). And overall, arrows in all plots tend to show varying phases throughout time. I suggest either eliminating this part, either improving substantially the discussion.

Comment 16 (P. 5464, L. 7-12): The fact that some of the ensemble members show correlation coefficients of different sign (e.g. 0.11 or -0.71) seems to suggest that the actual link proposed between the GTA and the TSI is just an artifact of similar autocorrelation values in the two time series. Indeed, the GTA in Fig. 7 is rather variable among the different ensemble members, thus showing evidence of a great effect of internal variability, with a larger influence than solar irradiance.

Comment 17 (P. 5464, L. 18-20): But what is the actual mechanism behind the "physically explainable" negative correlation?

Comment 18 (P. 5465, L. 14-19): I find SLP changes in Fig. 9 rather patchy in the northern latitudes. There is no clear large-scale structure and therefore it makes little sense to establish inferences on the associated wind or temperature changes.

TECHNICAL COMMENTS:

Comment 19 (P. 5457, L. 12): "particularly in winter" should be separated by commas.

Comment 20 (P. 5458, L. 8): Remove "to" after resembling.

Comment 21 (P. 5458, L. 15): "Proxy grid temperature reconstructions" sounds awkward. I suggest "gridded temperature reconstructions".

Comment 22: When multiple references are provided they should follow a chronological order, so that the first article cited correspond to the first article that did/showed/proved what is being cited, and so on. Please correct throughout the text (e.g. P. 5457, L. 1-2).

Comment 23 (P. 5459, L. 7): Change "liner" to "linear"

Comment 24 (P. 5460, L. 25-26 and P.5464, L. 11): Change “multi-decadalto-centennial” to “multidecadal-to-centennial”

Comment 25 (P. 5461, L. 14 and L. 18; P. 5464, L. 18): Missing full stop after “Mann et al. (2009)”, “the intervening period” and “(see Appendix D)”, respectively.

Comment 26 (P. 5463, L. 2): Separate “millennialscale”.

Comment 27 (P. 5463, L. 8-12): Add “The fact that” at the beginning of the sentence.

Comment 28 (P. 5463, L. 2): Verb tense is “supporting” not “support”.

Comment 29 (P. 5459, L. 7): For clarity, I recommend to avoid or at least minimize the use of parentheses one inside another (e.g. P. 5459, L.29) or one immediately after another (e.g. P. 5459, L.8, L.18, L.27-28; P. 5460, L.9-11; P. 5462, L.19, ...), as well as the use of em dashes within parenthesis (e.g. P.5460, L. 10-11).

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