

Interactive comment on “Climate variability in subarctic area for the last two millennia” by Marie Nicolle et al.

Anonymous Referee #3

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The climate of last 2 k has been very intensively investigated over recent years, and especially since the 2 k datasets were compiled and became available. It is hard to publish paper which would present new, fresh results and/or interpretations. The paper at hand demonstrates this difficulty. It presents the arctic temperature reconstructions based on 56 proxy records, divided into three subregions which are North Atlantic, Alaska, and Siberia. The resulting temperature curves for these subregions are feasible and sound, showing the long-term decline from the beginning of the period until an uptick since the early 19th century. These are the same features as shown in most of the earlier temperature reconstructions for these regions or for larger regions, such as Kaufman et al. (2009) for the whole arctic or Hanhijärvi et al. (2013) for the N Atlantic. Of course this is no wonder, as the paper is to great extent based on the same data as the other papers. The only subregion where there is some novelty is Siberia, but

C1

this composite time series is based on only 6 records. I find the new output in these 2000-yr temperature curves minimal.

I also find the chapter “Secular variability” inevitably trivial. First, only the Little Ice Age (LIA) is dealt here, although for example, the Medieval Warm Period or periods between 0 ka to 1 ka could have been included here as well. In the scrutiny, some suggested dates for the beginning and end of LIA are given, and they seem to be compatible with those given in tens of studies about the LIA earlier. Similarly, a set a standard explanations for the cause of LIA are mentioned. So much has been written about the LIA and its causes that one would expect something more substantial than reading that “the LIA has been attributed to the combinations of external climate forcings including solar activity fluctuations and/or volcanic activity”. In her book “Little Ice Ages” J.M. Grove (2004) went through possible causes of LIA – 30 pages I notice – discussing solar and volcanic activity and many other possible causes. So one can ask whether the one sentence in the paper at hand adds anything to this issue.

As for the methodological description, it is not necessary to describe the basic mathematics behind the Mann-Kendall test or the wavelet analysis. These are basic techniques in climatology, and if it was necessary to describe the math in this paper, it would be equally necessary in hundreds of other papers where they have been used. The same is true for LOESS, used to provide a smoothers for Fig. 3 – this is a basic smoothing technique, and someone who wants to know more can look at the original paper by Cleveland (1979) – incidentally, this citation is missing from the list of citations in the paper.

To make the paper worth publishing, I suggest that the authors would make a serious effort to find the novel aspects of their dataset and analyses. One way could be to look more at the multidecadal variability and the covariability between the temperature trends in the subregions and the atmospheric and ocean oscillations indices, such has been done in chapter “3.3 Recent warming and internal climate oscillations” and in Figs. 6-8. Here the authors compare their temperature records with instrumental AMO

C2

and PDO indices. One possibility would be to use the non-instrumental AMO and PDO records to go farther back in time to see the correlations between multidecadal temperature changes and internal oscillation systems.

To conclude, I would not like to sound discouraging, but it is hard to see enough novelty in the paper to justify publication, unless the authors can substantially expand the new results and aspects of their study.

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