Clim. Past Discuss., 4, S600–S605, 2008
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Climate of the Past Discussions

Interactive comment on "Can we reconstruct Arctic sea ice back to 1900 with a hybrid approach?" *by* S. Brönnimann et al.

S. Brönnimann et al.

Received and published: 3 December 2008

1. Introduction

We would like to thank both reviewers for their effort and their valuable comments. The suggestions, especially those of reviewer 1, prompted additional analyses and comparisons that underline both the strength and the weaknesses of the chosen approach, as is outlined in more detail below.

The main point, in our view, is expressed near end of the review of reviewer 1. The reviewer asks the fundamental question whether a paper, the declared purpose of which was to stimulate discussion on a method that eventually "failed" (however promising it might seem), merits publication in the peer-reviewed literature given that fact that it is already published in the discussion section. We agree that "Climate of the Past

Discussions" is a good place for the paper, especially since the data we produce are not superior to existing data products. Even though we are very clear about that in the manuscript, a peer-reviewed paper on Arctic sea-ice reconstructions might give the wrong signal. We do not think, however, that "no results" do not merit publication in general.

We do not agree with the remaining part of that same statement, where the reviewer argues that the success of any such approach is far in the future. Quite in contrast: During the work on these reconstructions, several new data sets were published, including new historical data products (e.g., Mahoney et al. 2008) as well as new reconstructions (e.g., Kauker et al., 2008). At the same time, new climate model versions are being developed in the process of preparations for the IPCC Fifth Assessment Report, and a new version of HadISST is currently being developed. We are confident that in only 4-5 years from now, such an approach can in fact be successful. Progress is much faster than we anticipated, which however raises the question of the value of publishing a "no result" in the peer-reviewed literature at this time.

In view of these thoughts, we have therefore decided not to take this manuscript further, but rather to undertake a new attempt in a few years. However, we still would like to reply to the comments and show the comparisons done in this context, as they may further illuminate the strengths and weaknesses of our approach.

2. New Comparisons

We compared our reconstructions with those of Kauker et al. (2008), as asked for by reviewer 1. Concerning the pattern of the trend between 1915 and 1955 (their Figure 23), we found a very good agreement between the two approaches. In both reconstructions, the main sea ice decline in September was in the Siberian Arctic. In March the decline was largest in the Barents Sea and a (smaller) positive trend was observed in the Labrador Sea. The good agreement between the two approaches was surprising to us since the decadal part (which mostly relies on an analogue period

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in the CCSM3.0 control run) was considered the weakest part of our approach. The agreement was not only good in September (which is less surprising as the analogue was chosen based on August values only), but also in March, demonstrating that the analogue approach in fact can work.

We also performed additional analyses on the interannual scale. While the comparison of the decadal scale revealed surprisingly positive results, the ones for the interannual scale were surprisingly negative. The validations in the model world presented in the paper suggest that the approach theoretically works well, but in this assessment it was not possible to account for a) the applicability in the real world of transfer functions derived in the model world, b) the effect of the mismatch in the ice edge between model and real world and c) the effect of wrong or uncertain historical data. To test this, we calculated average sea ice concentration in the Nordic Seas in April (coordinates) and compared it to the corresponding values from HadISST (Rayner et al. 2003) as well as to the sea ice extent from Vinje (2001), and version Rec IV of Kauker et al. (2008). Note that all series were high-pass filtered as described in our manuscript in order to focus on the interannual variability. The correlations (for 1905-1953) are shown below in Table 1.

Table 1. Correlation coefficients between Nordic sea ice in April from four different data sets (based on high-pass filtered data, 1905-1952, HadISST from 1905 to 1939).

	Vinje	Had	Kauk	our rec
Vinje	1.000	0.787	0.703	0.294
Had		1.000	0.708	0.486
Kauk			1.000	0.320
our rec				1.000

While the agreement between Vinje, HadISST and Kauker is excellent, our reconstruction is clearly worse. The correlations are statistically significant (showing that there

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is at least "some" applicability of transfer functions - the good news), but the problems still seem to be huge. It is likely that all aforementioned effects have contributed. The Nordic Seas is arguably the most difficult area to reconstruct for our approach because this is where the mismatch of the climatologies between model and HadISST was greatest. Our regridding was (to be optimistic) only partly successful in this area. This is most likely a region where the applicability of the transfer functions is most restricted. At the same time it is a very important region - one that one would want to have correct in any reconstruction.

In addition to these errors, we also found an error in our processing. Under some circumstances, the partitioning of the time scales (i.e., the filtering) in the gridded sea ice concentration data, due to yet unknown reasons, caused some of the interannual variability to be folded into the decadal part. This error is probably not the largest contribution to the low correlations, but nevertheless the reconstructions should be re-calculated.

Note that this error does not affect any of the discussion on the methods or on the validation in our paper (all data shown and discussed in the discussion of the decadal time scale are correct). The only part of the paper that is affected by this error is the rather short Sect. 4.4. In fact, the processing error might be one reason for the anomalous behaviour of the year 1937.

3. Other replies

Since we do not submit a revised manuscript, some of the comments (like adding a schematic figure to better illustrate the regridding procedure) can not be addressed, and many are covered by the comments made above. There are three more comments, though, which we would briefly like to address.

Reviewer 2 asks how we compared sea ice concentration (from CCSM3.0) with sea ice extent (in the historical series). It is true that we were not very specific about this. Sea ice extent was defined as the area in which CCSM3.0 sea-ice concentration was

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larger than 15

A second comment of reviewer 2 concerns the incorporation of lags. An analysis of HadISST sea ice after 1953 showed that the autocorrelation function of regionally averaged sea ice anomalies generally remains above 0.5 for a long time (several months), even though it drops off faster from summer to winter than vice versa. This justifies the use of sea ice from both the previous and next spring-summer season for reconstructing the fall and winter season. Concerning the use of spring data (April), commented by reviewer 1, we would like to add that we did use data from April (to August) from the Walsh and Chapman data set (which, to reply another comment, we obtained directly from William Chapman). In a new attempt we will make better use of the available spring-time data.

4. Conclusions

In conclusion, our original manuscript as well as the additional validation experiments presented here strengthen the point that developing hybrid schemes is a promising route for historical climatology, but many difficulties yet need to be overcome.

Acknowledgements. We wish to thank Frank Kauker for providing his reconstructions.

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