

## ***Interactive comment on “Arctic marine climate of the early nineteenth century” by P. Brohan et al.***

### **Anonymous Referee #1**

Received and published: 17 February 2010

Brohan et al provide the most systematic look at the daily weather reports from the Arctic made on ships of opportunity from 1810-1825. While not all of the original notes are available for use, there are sufficient amounts of data to make this a worthwhile effort (digitization alone can be a thankless task but is an essential aspect of this work). One feature not unique to the Arctic, is that more abundant and good quality historical data often exists in areas that are not well covered in the modern record. This makes comparisons of these observations with modern data difficult and the task is particularly hard when working in remote areas such as the Arctic.

Understanding the context in which the observations were gathered is very important and the paper provides a brief, but still useful, background on the reasons for the observation's existence. Given the constraints the original observers worked under the observations are probably as good as any that can be expected under the conditions. The author's discuss a number of potential non-climatic biases that can be expected

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in the original methods which are also common in modern records. The author's use of the best available modern reference data sets for comparison of the historical data with the modern results. Their comparison of the results are particularly interesting with reference to local sea ice conditions as they appear to be less problematic in comparison with modern conditions than the surface air temperature (SAT) or pressure data.

For this reviewer, the failure to quantify the differences in historical air temperature from modern observations is disappointing even with the hard to quantify uncertainty in the estimates. I would prefer subjective estimates of the most likely range of uncertainty to be mentioned along with the quantified departure from the modern climatology.

Another feature that would be useful to consider is the atmospheric circulation patterns that are likely to cause unusual sea ice anomalies. There are reconstructions of the North Atlantic Oscillation (NAO) back to 1821 from instrumental records so at least some of the years could be compared with what might be inferred from regional circulation patterns known to prevail in the mid-latitude region of the North Atlantic. Prior to 1821, there are other less reliable estimates of the NAO that could be examined. Such an approach might be able to make reasoned speculation about the larger-scale pattern of weather conditions beyond the handful of areas the ship observations provide. Cynthia Wilson made a detailed study of the climate on the east coast of southern Hudson Bay from 1814-1821 (Syllogeus series) which would be interesting to compare against the observations further north. Likewise, the work of Catchpole and Hanuta (1989) on sea ice in Hudson's Strait should also have been referenced.

The daily data in the modern record are interpolated from monthly data and this does not permit a direct comparison with the hourly and daily records. However, it would be good to try such a comparison. For example, the *Isabella* in 1818 kept a course near the coasts of Greenland and Baffin Island so it should much of the time be within the range of modern coastal stations to permit some comparison in the diurnal ranges of temperature and pressure. How unusual are some of the lowest and highest pressure

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readings seen in the figures? Is every hourly observation plotted in these figures (I can't tell)? Do the diurnal ranges fall within our best estimates of the modern record? If not, are they systematic in a fashion that it can be corrected? Can it provide indirect evidence of instrument placement?

As to the SAT record, can the author's provide documentation of the proportion of alcohol (spirit) thermometers versus mercury thermometers for the ships used in this paper? Since relatively few readings fall below the freezing point of mercury I am curious to this aspect of the instrumentation.

In short, there seems to be a lot of potential substance to add to the paper to make a substantive advancement in our understanding of the climate processes that were at play in the early 19th century and one I believe the author's are capable of supplying. This reviewer's appetite is whetted but not anywhere close to being sated.

#### Other points

Figure 1: I would like to see an additional figure with the odd-numbered years. The text mentions on page 38 about Scoresby's comments on the lack of ice in 1817. I'd like to see his ship's position plots for that year.

On page 36-37, it is commented that estimates of global change in temperature following the Tambora eruption is not known or how climate varied around the world at that time. An estimate of  $\sim -1.0$  C maximum global cooling was estimated by Chenoweth (2001) from available instrumental marine air temperature data which was consistent with model data and estimated stratospheric aerosol loadings. Some idea of the global climate around and after the eruption of Tambora was reported in Chenoweth (1996).

Page 38, line 20. I found the wording of this paragraph somewhat ungainly and the point being made is unclear on a first reading. I suggest a re-write to make the point apparent without the reader having to re-read it several times to understand the author's point.

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