

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

P. Brohan et al.

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Referee1 makes some useful criticisms of the paper, which we will address in a revised version, and also makes some valuable suggestions for additional work. Although this paper must stand alone, as so many of the referee’s comments suggest additional analysis, we would like to point out that the paper is part of a larger program of work on historical marine climate. In particular, we are currently digitising a large number of additional observations from different sources covering the same period. Quantitative estimates of biases in the observations, and detailed comparisons with modern observations and historical simulations, are best postponed until all the observations are available. So while we are appreciative of the referee’s suggestions for additional analyses, we generally don’t propose to address them in this paper - they will influence future work.

Specific points:

> 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.

To be done in future projects.

> 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.

We'll add the suggested references to a revised version - but comparison with circulation patterns must await future analyses.

> 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

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diurnal ranges of temperature and > pressure. How unusual are some of the lowest and highest pressure 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?

Again, a good suggestion for future work.

> 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.

There is some discussion of this in Ward and Dowdeswell 2006 (referenced in the paper), but the documents we have at present don't indicate whether alcohol or spirit thermometers were used.

> 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.

We'll add the other years in a revision of the figure.

> 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 -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).

We'll strengthen the reference to Chenoweth's work in a revised version.

> Page 38, line 20. I found the wording of this paragraph somewhat ungainly > and

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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.

Will be re-written as suggested.

Interactive comment on Clim. Past Discuss., 6, 35, 2010.

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