

Review of MS No. tc-2011-112 "A synthesis of the Antarctic surface mass balance during the last eight centuries" by Frezzoti et al.

Summary

The paper presents a reconstruction of the Antarctic surface mass balance over the past eight centuries based upon ice core accumulation records. The material upon which the paper is based is of great interest, and the authors provide an interesting review of the literature on the issue of Antarctic SMB. Done right this could be a major contribution. However, the paper contains some major flaws (described below) pertaining both to the presentation and to the science. The necessary revisions to the manuscript are so substantial at this stage that I do not recommend the paper for publication.

Scientific significance: Good

Scientific quality: Poor

Presentation quality: Poor

Recommendation: Rejection

Major comments

1. The first and most obvious flaw of the paper is the poor quality of the English which makes it often difficult to read (in addition to problems #2 and #3).
2. The manuscript displays relatively little logical structure or coherence within each section or sometimes even within the paragraphs themselves. The most obvious example is the Discussion section. Along the same lines, the Introduction does not provide any clear motivation for the study (how do lines 17-28, p. 825 relate to the rest of the Intro?). Some paragraphs are excessively short (one sentence) and appear disconnected from the rest of the text (p. 826, l. 9-11; p. 828, l. 15-18; p. 832, l. 8-10).
3. The presentation and discussion of the results are often confusing.
 - (i) Some methodological aspects are contradictory: the authors argue that "unassessed records" should be discarded and yet they include them in "most of the analyses" (p. 828 l. 11). The discussion of Fig. 2a (p. 831 l. 1-17) is based upon the results from full ice core array. However, Fig. 2a is actually based only upon the assessed array.
 - (ii) In the Results section, the authors seem to primarily discuss the results from other studies and only incidentally their own results. One has to wait the end of the "Results" section to see Fig. 2 actually described.
 - (iii) The reference to the figures sometimes seems to be out of place (p. 829 l. 18) or introduced in a clumsy fashion (p. 829 l. 18). Rather than just "Fig. 2", the authors should rather refer to Fig. 2a or Fig. 2b. A lot of the Results section deal with the temporal variability of the SMB during specific periods. Yet, no reference is made to Fig. 3, which is only discussed at the very end of the Results section. Much more synthesis/integration of the presented material is needed.
4. Some scientific results presented in the paper are questionable.
 - (i) The authors derive SMB values for the entire continent by simply taking the arithmetic average of all available records. However, as they point out, the spatial distribution of the cores is heterogeneous. Therefore, the averaging method should take into account the spatially-varying density of the records. For example, one could estimate the spatial footprint of each ice core record using output data from

global reanalyses as in Monaghan et al. (2006). At the very least, if the authors decide not to use spatially-weighted averages, they should include some discussion about this issue. Also in Figure 3, a count of the number of cores as a function of time used in each series is needed; right now I don't understand how the count in A relates to B-E.

(ii) The conclusions based upon the relationship between Antarctic SMB and Total Solar Irradiance (TSI) are certainly the weakest scientific aspect of the paper. Sachs et al. (2009) only show that the southernmost position of the Pacific ITCZ coincided with a minimum in TSI, *not* that the southernmost position of the Pacific ITCZ is correlated with the TSI. And yet, the authors seem to use the TSI as a proxy for the position of the Pacific ITCZ, for the tropical teleconnections to high southern latitudes, for the PDO, etc., which is obviously highly questionable. The verb "mirror" (used in the Abstract, Discussion, and Conclusion) is also inappropriate to describe the variability of the SMB with respect to the TSI. Figure 4 shows periods where the SMB anomalies and the TSI anomalies are either in phase or out of phase. In particular, the authors seem to minimize the periods of out-of-phase relationship by stating that "the correlation... nevertheless 'are' well inside the one-sigma uncertainty", an argument which is by the way very unclear. Why don't you calculate a correlation coefficient to test the association?

Minor comments

At this stage, it is premature to provide an extensive list of minor corrections, most of which should be addressed prior to submission through careful proofreading of the manuscript.

p. 823, l. 19-20: I can only think of *one* other component of the ice sheet mass balance (the ice discharge). The text suggests that there are several of them.

p. 823, l. 22: The paragraph begins with references to the interannual variability of the SMB. The authors also quote the SMB estimate from Van den Broeke et al., which is based upon a regional climate model simulation, with 6-hourly output. Then, paradoxically, the text mentions "shorter (decadal) time scales".

p. 824, l. 6-7: It is unclear what is meant by "climatologic/environmental effects".

p. 824, l. 7: satellite observations

p. 824, l. 11-24: This paragraph could be removed.

p. 824 l. 25-27: On the contrary, Steig et al. (2009) show stronger warming over WAIS than over the AP. Following sentence: The magnitude of the temperature trends in Antarctica is a highly debated issue, and the results from Steig et al. show some important differences with respect to Chapman and Walsh (2007). Therefore, the authors should provide a range of estimates for the temperature trends.

p. 825, l. 17: The paper does not deal with changes in the "volume" of the ice sheet, but with changes in its mass. The paper is organized in four sections, not "paragraphs".

p. 831 l. 1-3: Fig. 2 only shows the results for B40 vs B150, not B40 vs Btot.

p. 831 l. 10-17: It is unclear to me how the authors derive the results discussed on lines 11-17 simply by comparing the total and assessed datasets.

p. 831. l. 14: Explain what is meant by "base of the SMB".

p. 832 l. 2-3: This sentence appears verbatim three times in the manuscript (Abstract, Intro, Discussion).

Figures 3 and 4: It should be clear that the SMB time series are normalized *anomalies*, and that the TSI (Figure 4) is also shown as normalized anomalies. The legend in Figure should include "SMB" in addition to "Continental".