

Interactive comment on “The Ross Sea Dipole – Temperature, Snow Accumulation and Sea Ice Variability in the Ross Sea Region, Antarctica, over the Past 2,700 Years” by Nancy A. N. Bertler et al.

Anonymous Referee #2

Received and published: 14 October 2017

In this study, the authors are presenting high-resolution isotopic (dD) and snow accumulation rate records of a new ice core (RICE) drilled on the Roosevelt Island in the eastern Ross Sea and covering in its upper part the last 2700 years. The authors are comparing these new records to other ice core records present in the near-by areas interpreting these records in terms of the climate variability which in these areas (Eastern and Western Ross Sea) is characterized by a climate pattern referred to as the Ross Sea dipole. The paper is noteworthy and the authors are doing a good job in calibrating the new records against the Era-interim re-analysis data (temperature and precipitation) as well as to other climate indexes as SAM, SOI, Nino3.4, IPO, as well as Sea Ice extent. However, some methodological questions are arising in this part (see

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below). The manuscript is quite well structured and the topic is appropriate for Climate of the Past. Nevertheless, the authors should consider the comments reported below before resubmitting a revised version. One general comments refers to the fact that the manuscript (Winstrup et al., CPD) presenting the ice core dating, on which most of the interpreted data are relying on, is not published yet. There are also other papers (one is Emanuelsson et al.), to which the authors are referring that are still at the submission7review stage, please check and update. General as well as detailed comments are reported below. Page 3, line 3: may you check this sentence? Over the observational period (satellite era) the sea ice should be increasing in the Ross Sea sector and decreasing in the Amundsen–Bellingshausen sector. Page 3, line 11: here the authors are saying that they will compare these new records to other ones existing in the region but it is not clear why they do not consider the Taylor Dome ice core record. Page 6, lines 27-32. Here the authors are optimizing the dD/T relation to the age scale. I am wondering why the authors did not consider optimizing the snow accumulation rate to the ERA-I precipitation rather than the dD/T relation. In fact, it is known and also the authors are clearly showing this, that the isotopic composition rely not only on the temperature but also on other circulation-related factors. How, this choice is affecting the climate interpretation? The authors should answer to this comment. Page 6, line 31: Figure 2b should be 2c. Page 6, line 35: in the Masson-Delmotte paper only spatial d/T slope are considered. Page 7, line 5: Is the lack of correlation valid also considering only the 1979-2012? Page 7, line 6: RICE dD should be RICE dDo . Page 7, lines 11-14: Is this strong accumulation rate gradient suggesting possible movements of the dome in the past. May you explain this? Page 7, line 37-38: the region which exhibits a negative correlation seems to be at lower latitudes than the ASL, at least looking at the figure Page 8, lines 10-12: the strong impact of blocking events at this site would support my comment above (Page 6, lines 27-32). Page 8, line 13: the negative correlation seems to interest more the Amundsen Sea. Page 8, line 17: are polynyas resolved by the model data? Page 8, line 21: I would move this sentence: “We focus” at the beginning of the paragraph. Page 8, line 20: please add

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a URL link to these data (SIEj). Page 9, lines 1-7: please add data citation or URL in Data Availability section for all the climate indexes used. Page 9, line 9: why using the SAMa index instead of SAM for this period? Are these two indexes the same over this period? Page 9, line 13: (but not with Rice snow accumulation). . . See again my comment above (Page 6, lines 27-32). Page 9, lines 22-24: again why not using Taylor Dome? On the other side, regarding the use of TALDICE data: I would suggest to use Talos Dome (89 m core) isotopic and snow accumulation rate data (Stenni et al., 2002) rather than TALDICE for this recent period. The TALDICE data are low resolution data and the snow accumulation rate, here considered, comes from the dating model and as such implicitly connected to the isotopic record from which it is derived. On the contrary, the isotopic and snow accumulation rate records from the Talos Dome core, although limited to the past 800 years, are high-resolution data and the dating has been performed by nssSO₄ annual data constrained by the volcanic chronology. Moreover, the TALDICE data, here reported are on the Buiron et al. (2011) age scale which has been replaced by the AICC-2012 chronology. Between the two there are in some cases differences up to 150 years than for the purposes of this paper could be important. So, the authors could consider using both isotopic data sets (Talos Dome and TALDICE but the latter on the AICC-2012 age scale) and the snow accumulation rate from only Talos Dome. An alternative to the AICC2012 age scale for TALDICE is the Severi et al. (2012, CP) chronology, which uses the volcanic synchronisation between the TALDICE and the EPICA Dome C ice cores and in practice identical to AICC2012 age scale. Page 10, line 14: the onset of a decline in WDC isotopes at 579 CE is not clear from the figure . . . Page 10, lines 25-26: the sentence “From the 17th century “ it is not very clear, at least looking at the figure, and the word in phase seems to be not the correct word to use. . . Page 10, line 30: TALDICE data: see the comments above. Page 10, lines 37: from here (“until the 15th century . . .”) to the end of the paragraph it is not easy to follow the reasoning. . . Page 11, line 19: how this date (1367 +/-12 CE) is chosen? Not clear. Please, explain . . .I suppose from the SAMA record. . . Page 12, line 24: increased marine air mass intrusions: from borehole

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or isotopic record? Page 13, line 30: are anti-correlated before the authors were claiming that they are correlated (see fig 8) at least from 400 to 1900 CE. Page 17, line 1: the author list of this reference (Jones et al) seems to be not complete. Page 22 Table 1: I would suggest adding the resolution of the different records. Again, for TALDICE I would suggest using the AICC2012 or the Severi et al. (2012) age scales. Always in the Table 1: the reference WAIS Divide Members, 2013 refers to WDC and not Siple Dome. Page 26, Figure 4 caption: add the explanation for panels c and d. Page 28, figure 6 caption: for RICE, are these decadal averages or what? Page 30, figure 8 caption: please add that these are isotopic data.

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2017-95>, 2017.

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