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Climate of the Past Discussions

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Interactive Comment

Interactive comment on "Little Ice Age climate and oceanic conditions of the Ross Sea, Antarctica from a coastal ice core record" *by* R. H. Rhodes et al.

Anonymous Referee #3

Received and published: 19 March 2012

This paper by Rhodes and co-authors is presenting new data (both isotopic and chemical analyses) obtained from a relatively high snow accumulation rate (220 mm water equivalent) ice core drilled on the Mt. Erebus saddle (MES) near the edge of the Ross Ice Shelf (Antarctica) and as such directly influenced by the extension of the Ross Sea Polynya. The site is influenced by the arrival of two types of air masses, those arriving from the interior of Antarctica and those arriving from the Southern Ocean in the form of cyclones. As such the authors are using the different types of chemical proxies (lithophile and marine ones) as well as the isotopic profile, a temperature proxy, to recognize the Little Ice Age (LIA) cooling event. High values of deuterium excess are found in the later part of the record and along with the Na record are used to infer an increase of intrusion of cyclonic systems. Moreover, they try to reconcile the increase of bottom water formation suggested by other authors (Broecker, 2000) with the suggested increase of katabatic winds in lithophile elements recognized in the MES ice core. The increase of katabatic winds during the LIA along with the observation of an increase of the biogenic sulphur specie MS- may be indicative of an enlarged polynya with a consequent higher production rate of bottom water formation during the LIA.

The paper is presenting new and interesting data on a debated question about the timing and the global nature of the LIA and its presence in Antarctica. The manuscript is well written although it presents too many technical details on the analytical methods and I would suggest moving some tables in the Appendix. However, there are some weakness related to the dating of the core and the isotopic analyses for the bottom part of the core (see further comments below). As such, I recommend its publication only after its revision.

Page 217, line 2: the term “abrupt” is not suitable for the LIA. I would suggest removing it.

Page 219, line 9: after 1590 and 1875 please, add AD. Please, change also in other parts of the text, whenever necessary.

Page 220, line 3: I do not understand the term “continuous-melter-discrete”. May you specify better.

Page 221, line 13: 1329-1629. . .. . . add AD.

Page 221, line 16: a question about internal standards for isotopic analyses. It seems to me strange the fact the no standards with more negative values than INS9 have been used. The paper reports dD values as negative as -270 per mil (V-SMOW) and the INS9 has a dD value of -131 per mil only. Usually, the standard values are bracketing the samples values and at least one standard (not included in the calibration curve) is used as an external check. This is quite important when using the new laser techniques

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exhibiting a lower precision, in particular with very negative values. Moreover, are the Los Gatos data being corrected for memory effects?

Page 221, line 20-25: the LGR data from 92 to 120 m, corrected for this offset, are unfortunately very near the d excess shift and part of this shift could be indeed due to analytical artefacts. Perhaps more check, at different depths (between 92 and 120 m) would be useful to validate this deuterium excess shift. However, the shift is probably real but needs more data validation and/or more attention in the text.

Page 223, line 16: Why have been measured only six samples for tritium activity? Why putting these tritium data without using as a tie point in the age scale? The snow accumulation rate is quite high to allow for an improvement in tritium data resolution.

Page 223, line 22-23: which are the chemical species used in the annual layer coulnting, apart from isotopes?

Page 223, line 23: "……. using key reference horizons as age tie points". Which are these horizons? Please provide a table.

Page 224, line 3: The snow pit isotopic data in Rhodes et al., 2009 (their figure 2) are already quite diffused…. Perhaps wind snow drift is acting more effectively during winter removing the winter snow events? How the precipitation is distributed over the year at present? Are there any studies about accumulation distribution at or near this site? Sinclair et al. 2010 or Markle et al. 2012?

Page 224, line 9-14: It would be better to show in a figure both the raw data and the back diffused ones together, since they are used in the annual layer counting up to 90m.

Page 224, line 24-25: at the end the tritium peak is not used…… so why put these data? Is it not possible to increase the number of analyses around the peak Moreover looking at figure 2 (page 251) why the point at about 19m is not put in correspondence of highest values founf in precipitation?

Page 225, line 22-26, and figure 4 (page 253): it seems not easy to fix this minimum Pb value at MES. There is another minimum value (a little before) that could be another suitable candidate.

Page 226, line 1-11: since the annual layer counting is so much important for the dating of the MES core the part related to the dating uncertainties is not very clear: how is it possible that it is 4 yr at 90 m and 8 yr at 1815 AD??? Please explain better. Moreover, again the tritium comes out. . ..

Page 226, line 15: the deuterium excess is not present in figure 5 (page 254).

Page 226, line 17-18: are the data presented in figure 6 been back diffused?

Page 226, line 24-25 and 227, line 2: part of the "abrupt" shift may be due to analysis artefacts. Please, add something here. . .. . . The data in the period 1329-1629 AD have been adjusted for the offset, please, state it. Moreover the values 1.3 per mil higher etc . . ... is equal to the uncertainty value for the more recent period (IRMS analyses).

Page 227, line 11-12: please, add somewhere that the Na peaks in winter in other places in Antarctica (more inland sites).

Page 229, line 16: "Na and d-excess show similar. . ..". This similarity is not so clear at all to me. . .. (figure 6).

Page 229 line 26-29 and page 230, line 1-4: Although I understand the point raised by the authors, here the discussion should be more clear. Although it is true from the paper of Markle et al. 2012 that higher d excess values in a near by place are associated with a higher frequency of oceanic air masses arriving to the site, it is also true that local moisture sources (from polynya??) would produce low d excess values. So, the arguments here should be better clarified. Markle et al. 2012, also suggested that the d excess signal is quite complicated by different processes (SST variability, sea ice extent and ENSO).

Page 231, line 15-17 and figure 7 (page 256): the rapid decrease of lithophile elements,

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referred in the text, is at different timings: Ce and Mn (less clear) high values cease before Al. The timing is different. Please, provide comments.

Page 232, line 7: the Pb values are not in Table 4.

Page 233, line 8: Steig et al. 1998 reported a slope value of 4 mil each °C BUT with a +/- 1.5 per mil of uncertainty. Indeed, there is a lot of uncertainty in the slope values, and so in the determination of temperature anomalies, see also Masson-Delmotte et al., 2008 (J. Clim) and 2011 (Clim Past). Something on this slope uncertainty (and so T calculations) should be discussed, referring also to the discussion between spatial versus temporal slopes and their validity. The calculated T for LIA reported by the authors should consider this uncertainty.

Page 234, line 6-7: how good is the dating of these marine cores?

Page235, line 24-27: "Colder temperatures …. stronger katabatic winds…. greater polynya…" Why not also greater sea-ice extent or higher persistence of winter sea ice? And so relatively more "distant" moisture sources and higher d excess values?

Page 236, line 25: something is missing after "as:".

Page 244 Table 1: please, for simplifying the reading of the table, specify which elements are determined by IC and which by ICP-MS.

Page 245, Table 2: I would move it to the Appendix.

Page 246, Table 3: I would move it to the Appendix.

Page 254 figure 5: the d excess seasonal cycle is not reported in the figure but it is referred to in the caption.

Interactive comment on Clim. Past Discuss., 8, 215, 2012.