

Interactive comment on “El Niño Southern Oscillation signal in a new East Antarctic ice core, Mount Brown South” by Camilla K. Crockart et al.

Anonymous Referee #1

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In this manuscript, the authors used few short firn cores from an interesting coastal region in the Indian Ocean sector of the East Antarctica to identify the potential climatic records from this region through comparison with reanalysis data during 1975 - 2016. The study also attempts to compare and differentiate the climate archives at Mount Brown South with a well-known and extensively studied Law Dome site. Study of the Antarctic climate for the past millennia at seasonal to annual resolution using multiple records is a critical requirement for an improved understanding the natural variability and the recent human impact on Antarctic climate. Considering that only few ice core records are available with such time-resolution in the East Antarctica, recovering new ice core records from coastal sites with high snow accumulation rates are important. Therefore, the present study using short firn cores to identify the dominant climatic

C1

signature embedded in the ice core archives at the Mount Brown South (MBS) region is a useful background study for a long-term climate reconstruction. However, the study needs to be more refined and revised to be suitable for publication.

Major comments: 1. Although introduction states that the study aims investigate potential signals for ENSO, SAM and IOD, and also section 3.3 mentions about the SAM and IOD, there exist no proper discussion and the whole effort looks like a half-hearted attempt. Mostly importantly, while there section 4.3 dealing with the lack of SAM record at MBS, the IOD component is completely missing other than stating that MBS salt record is not significantly correlated with DMI. Even if there exist no statistical correlation, it is important to discuss the details, potential reasons and substantiate that the site is reliable for reconstructing only ENSO. 2. One of the critical factors that influence the annual/seasonal snow accumulation rates in coastal Antarctica is the impact of extreme precipitation events (Turner et al., 2019, GRL). The coastal region around the East Antarctica studied here has shown to be strongly influenced by such events. This could also have significant influence on the seasonality of proxy records especially in high accumulation sites. Since impact of the climatic modes are season-dependent, it is important to have an analysis of potential impact of EPEs on the seasonal/annual climate record at the MBS. Undertaking an analysis of the precipitation and impact of extreme events using the updated high-resolution RACMO output could be very helpful. RACMO model is also considered to be more consistent on a long-term basis and also its performance has been extensively evaluated. Since the present study itself deals only the last some decades when reanalysis and model outputs are available (and reliable), it is important to add value to the palaeoclimatic perspective of this study by examining the impact of such local/ regional events. 3. Lack of proper tagging of figures in the methods and results are making it very hard to follow the data and analysis. Another issue that needs to be carefully revised is the mixing of methods in results and vice versa. Also some part of discussion is mixed up in the results section. A careful editing is warranted. 4. There are too many abbreviations in the manuscript making it tedious to follow. While it is acceptable to have the common acronyms like

C2

ENSO, SAM, IOD, as well as shortening some of the most commonly used names (like MBS), the tendency to use acronyms for all and sundry should be avoided. Acronyms like MOCV, RWT, etc. are unnecessary and needs to be avoided.

Specific comments: Abstract L25 – Throughout, authors have used the term “snowfall accumulation rates”. This is misleading, as the snow accumulation at a given site in Antarctica is a product of various processes other than just snowfall. This is especially true in coastal Antarctica as wind-induced redistribution are dominant as well as processes like diffusion are very common. Therefore, “snow accumulation rate” is a more correct term. L30 – Please explain and detail “. . . suggesting occurrence of distinct moisture and aerosol intrusions”. Such a sweeping statement without supporting evidence in the discussion doesn’t help the discussion. Introduction L39 – Most of Antarctic ice cores are resolved at decadal or century scale, not millennial. L47 – Either the full form or just RICE. L49 – Such context is inappropriate; this study deals with only very short cores representing less than 40 years long. It is important to give the importance of array of cores for background of seasonal/annual records. Therefore, introduction may be revised to discuss more on records are available across Dronning Maud Land to Law Dome and beyond that would have more relevance on the science discussed here. L55- Avoid unnecessary acronyms that reduce the flow or reading. L74 – This line is confusing and has no relevance for this study. L98 – “Signals for ENSO in East Antarctica are more muted. . .”. Be specific. East Antarctica is too large a place to make such sweeping statement. L101 – There are some recent studies on the influence of IOD on southern hemisphere and vice versa (Nuncio and Yuan, 2015, Journal of Climate; Zhang et al., 2020, GRL). It would be more interesting and valuable to look at the impact of these possibilities at MBS in discussion and find potential links. L134 – “Main”? This term is only explained later at Methods. Either define here or avoid using it. L137 – Revise. There are many high-resolution (seasonal/annual) ice core records that represent past 100 - 200 years of climate across the coastal East Antarctica. Therefore, there is nothing much to prove on Hypothesis 1. Also Vance et al. (2016) has also given such higher accumulation at this core site. Methods L141 –

C3

Better to give the short forms in the title (MBS, LD) for helping the reader. L151 – “The MBS. . .”. You mean the “Main”? L157 – Fig 1 is uninteresting and a missed opportunity to give more useful information. It would be very useful to give a schematic diagram of dominant features of ENSO/PSA impact around the Indian Ocean sector. L194 – See previous comments on the “snowfall accumulation rates”. L203 – May replace “. . . the usual proxy. . .” with something like “the more conservative proxy.” L211 – A good part of this section (and methods in general) deals with results that could be best placed at results section. L225 – This needs an explanation in the context of the present study. Why summer for LD and annual for MBS? In Fig. 3, for accumulation, annual rates are used, but for sea salt it is summer. Such convenient picking needs to avoided or a more specific reasoning. L227 – As mentioned earlier, you need to refer the figures and tables as you start discussing. In absence of it, it is very difficult to follow the discussion. This is the case at many places. L244 – This does not explain why only Sept-Oct data of Law Dome was used for statistical study. Is there any data/reasoning to support that the ENSO is impacting MBS and LDS at different seasons? L267 – This is confusing. Revise. Results L279 – Section 3.1 title doesn’t convey much. Why didn’t you give a title that reflects what is discussed in the section? L281 – This entire para deals with chronological constraints that could be best placed at section 2.3. This section should deal with more on results of the study on proxy data. L319 – “. . . seasonal. . .”. You need to give in bracket, which seasons for clarity. Also this para should give some explanation why JJASON for MBS and SO for LD records were used. Otherwise it is more an act of convenience. L326 – As commented earlier, it is important to refer to the Figures /Tables to guide the readers. L342 – The scale on Fig 7 (also Fig 6) needs check. Seems the sign missing. L360 – This section needs revision, as there is no discussion on the results on IOD. Also the data/figures are not referred. L363 – It is not correct that there is a “lack of a SAM signal”. May consider to revise it as “lack of a statistically significant SAM . . .” Discussion L377 – Section 4.1 title doesn’t convey its content. May revise. L415 – As commented earlier, it is important to explore the impact of extreme precipitation events on the proxy records discussed here. Such an

C4

evaluation would enhance its value for a journal like CP. L419 – The entire discussion on IPO is pointless as the records discussed here are less than 40 years! It’s all speculation and lacks purpose. L429 – Some discussion on the potential mechanisms on the influence of ENSO to the study site is important. There are some previous studies on these that could be used as a starting framework. L450 – A discussion on the potential robustness of MBS records (compared to the LD site) for ENSO reconstruction would be useful. L459 – Exactly. This potential impact of extreme events needs to be explored. L467 – This is more speculation in the absence of any proof on “anomalies develop in austral winter than spring”. Either you need to provide a proof or remove such statements. L471 – This is speculative at this stage without discussing proof and reasoning. L473 – Section 4.3 may include a discussion on IOD signal (or lack of it) at the MBS site. L475 – Is this consistent with the Marshall et al. (2017) study? If not, may be some reasoning needs to be brought out. Conclusion L520 – Section 5.0 needs revision. It also needs to be nuanced that it’s a composite of 3 records. L526 – This doesn’t actually reflect the important findings of the study; for example, the MBS is ideally suited for ENSO reconstruction and issues with SAM and IOD at this region.

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