Clim. Past Discuss.,
https://doi.org/10.5194/cp-2020-134-AC1, 2021

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

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Received and published: 9 March 2021

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 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.

Author response: Thank you for your valuable comments and suggestions. We will revise the paper and make amendments. This is my (Camilla Crockart) first paper and is the publication from my MSc project. I will now be developing the longer MBS record in my PhD.

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.

Author response: Adding more detail on the lack of a statically significant IOD signal is a good idea. We will add a section that discusses potential reasons for the lack of statistically significant IOD and SAM signals in the MBS record. As the SAM varies from weekly to seasonal timescales, it is possible that a statically significant SAM signal is only preserved in certain months or seasons in the MBS record, such as in the case of LD (Vance et al. 2013). Similarly, the IOD is seasonally locked - peaking in spring - and therefore an IOD signal, if preserved, may only be preserved in the seasonal MBS record, potentially at a resolution that we may not yet be able to resolve. As the frequency of precipitation is yet to be determined in detail, we think it is important to keep the MBS record annually resolved at this stage to avoid any errors associated with binning monthly values. Therefore, potential SAM and IOD signals preserved in the seasonal MBS record will need to be examined in the future, once precipitation at

MBS has been investigated in more detail. We would like to stress that this is the first analysis of annual signals in the new MBS ice cores. Future work is ongoing, and we will endeavour to make clearer in the manuscript that study on signals derived from the longer Main MBS datasets is ongoing.

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 help-ful. RACMO model is also considered to be more consistent on a longer 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.

Author response: That's a good idea. Given the co-authors we have on this work (e.g., Vincent Favier and Jonathan Wille) and their expertise in the French Modele Atomspherique Regionale (MAR), we would like to add additional analysis using the MAR model instead of RACMO. Agosta et al. (2019) suggest that the MAR and RACMO perform similarly well in simulating surface mass balance gradients in both plateau and coastal regions of Antarctica. With the help of a new co-author Christoph Kittel (as well as Favier and Wille), we propose to add additional analysis using the surface mass balance data from the MAR to look at the frequency of precipitation at MBS at a monthly/seasonal scale.

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.

Author response: Apologies. This will be carefully edited so that there is less overlap between the methods, results and discussion. All figures and tables will be correctly labelled and appropriately referred to in the results and discussion sections. We propose to address these issues in detail in a marked-up version of the manuscript.

4. There are too many abbreviations in the manuscript making it tedious to follow. While it is acceptable to have the common acronyms like 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.

Author response: We propose to revise using only standard acronyms (e.g., MBS, LD, IOD, ENSO, and SAM).

Specific comments: Abstract L25 – Throughout, author 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.

Author response: This will be corrected to 'snow accumulation rate' throughout the paper.

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.

Author response: We will revise the statement to be less sweeping and suggest that 'further study is likely to help us define the cause of this inverse relationship between LD-ENSO and MBS-ENSO'. The first piece of evidence to help with this analysis is the work of Udy et al. 2021, which showed that the two ice core sites have differing moisture sources at the synoptic scale, and our results support this finding. Please note it was a condition of the MBS site selection that the MBS record differed to the LD record (Vance et al. 2016). Our results support this as the two records differ (e.g., the annual sea salt concentrations are not correlated, and the MBS-annual and LD-summer sea salts are inversely correlated).

Introduction L39 – Most of Antarctic ice cores are resolved at decadal or century scale, not millennial.

Author response: This will be corrected to 'centennial'.

L47 – Either the full form or just RICE.

Author response: This will be corrected to the full form, 'Roosevelt Island Climate Evolution'.

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.

Author response: We will make it clear that we are only using the upper section of the MBS ice core. We propose to add more detail on high-resolution ice cores collected from East Antarctica, including Dronning Maud Land, and remove any of the West Antarctic examples in a marked-up version of the manuscript.

L55- Avoid unnecessary acronyms that reduce the flow or reading.

Author response: As above – we will revise using only standard acronyms.

L74 – This line is confusing and has no relevance for this study.

Author response: This will be corrected to 'Vance et al. (2016) suggest that a new ice core collected from the MBS region may contain an independent SAM signal'.

L98 – "Signals for ENSO in East Antarctica are more muted. . .". Be specific. East Antarctica is too large a place to make such sweeping statement.

Author response: This will be corrected to 'The ENSO also influences East Antarctica, for example, there is an ENSO signal preserved in the summer sea salt record from LD, although there are far fewer studies on ENSO influences at high latitudes in the East Antarctic to determine this definitively (Vance et al. 2013)'.

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.

Author response: We will add more detail on the influence of the IOD in Antarctica and refer to the above-mentioned papers. As there is limited literature on the influence of the IOD in East Antarctica, particularly the Indian Ocean sector, we will add more detail in the discussion section explaining the lack of information and data on the influence of the IOD in East Antarctica.

L134 – "Main"? This term is only explained later at Methods. Either define here or avoid using it.

Author response: This will be changed to the 'the extended MBS Main ice core'.

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.

Author response: This will be change to '(Hypothesis 1) contains signals for past climate variability at a high-resolution that extend beyond short spatial scale variability, and (Hypothesis 2) contains climate signals that differ from the LD record.' We would like to determine whether the MBS record preserves climate signals from the lower latitudes (i.e., outside of the Antarctic region), and whether it meets the drilling requirements of Vance et al. (2016) of containing a complementary ice core record to LD. Although we present the short MBS record here, we think it is important to introduce the longer record that spans 1,000 years. The brevity and scarcity of high-resolution ice cores in this region is a limitation of our understanding of climate variability (Jones et al. 2016; Stenni et al. 2017; Vance et al. 2016). Hence, both the length and location of the extended Main MBS record is what makes this ice core record unique. We would like to emphasise this point, while making it clear that the extended Main ice core has not yet been fully developed. Additionally, the upper portion of the Main MBS core constitutes part of this analysis. We disagree that there are 'numerous high-resolution cores spanning 100-200 years in East Antarctica'. Yes, there are more in Dronning Maud Land, but in the Indian ocean sector from Enderby Land through to Wilkes Land there are very few. We propose to change this to define as the 'Indian Ocean sector from Enderby to Wilkes Land'. This is still a vast section of coastline in East Antarctica.

Methods L141 – C3 Better to give the short forms in the title (MBS, LD) for helping the reader.

Author response: Good suggestion. This will be changed to 'MBS and LD (Dome Summit South site)'

L151 – "The MBS. . .". You mean the "Main"?

Author response: That's correct. This will be corrected to 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.

Author response: We agree that Figure 1 could have contained more detail. However, we would prefer to have a site map in the methods section detailing the exact layout of the new ice core site where the MBS ice cores were drilled. We will change Figure 1 to include a map of the four ice core drilling sites at MBS, as the second Reviewer has asked for more detail on the MBS drilling site. Again, this is the first data paper for the new MBS ice core/s, and we think a detailed site map is appropriate (e.g., similar to Abram et al., 2011)

L194 – See previous comments on the "snowfall accumulation rates".

Author response: This will be corrected to 'snow accumulation rate' throughout the paper.

L203 – May replace ". . . the usual proxy. . ." with something like "the more conservative proxy.."

Author response: This will be changed to '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.

Author response: Apologies for the overlap between the methods and results. This will be carefully edited in the marked-up version.

L225 – This needs an explanation in the context of the present study. Why summer for LD and annual for MBS?

Author response: We will add a more thorough explanation so that it is clearer for the reader. We focused on the annual MBS record, as detailed analysis of the frequency of precipitation at MBS is the subject of extensive future study (e.g., with Favier and Wille), meaning it is not appropriate yet to use a seasonal MBS record due to errors associated with interpolating monthly values when the uniformity (or otherwise) of annual accumulation has not yet been properly assessed. As the frequency of precipitation at LD has been extensively studied, and it is known that extreme precipitation events have little impact on annual accumulation at LD, it is appropriate to use seasonal LD records (e.g., van Ommen & Morgan, 1997, McMorrow et al. 2001; 2002, Pedro et al., 2011; Vance et al 2013; 2015, Roberts et al., 2015). We chose to focus on the summer sea salt record from LD for the composite analysis because it is known to preserve a signal for the ENSO (e.g., Vance et al. 2013).

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.

Author response: The LD sea salt and annual accumulation records are established climate proxies containing climate signals as diverse as modes of variability (e.g., ENSO, IPO, SAM) and surface climate (Australian rainfall, south-west Pacific circulation variability) (e.g., Goodwin et al., 2004; van Ommen & Morgan, 2010; Vance et al. 2013; 2015; Roberts et al., 2015; 2019; Marshall et al., 2017; Udy et al., 2021), and hence we disagree that these are convenient picks. In the original manuscript we used a standard seasonal average for the LD summer sea salts (December-February). Given the Reviewer 1's concerns, we propose to use a December-March average salt concentration as the proxy to test for LD links to ENSO, as this is the established, published proxy for the ENSO and eastern Australian rainfall from Law Dome (Vance et al. 2013; 2015). This means we remain consistent with previous studies which have been developed in detail including analyses of the mechanisms responsible for the summer signals preserved at Law Dome (Vance et al., 2013; Udy et al., 2021; Udy et al., in prep), which may allay the concerns of the reviewer. Note that a change from a December-February sea salt average to a December-March sea salt average produces essentially the same timeseries ($r = 0.911$, $p = 0.000$). As a result, the findings in the paper are identical. For the new MBS ice cores, this initial study looks only at annual resolution because we need to undertake further study on signal preservation prior to looking at seasonally resolved records.

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.

Author response: Apologies, all figures and tables will be correctly labelled and referred to in the results and discussion sections.

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?

Author response: The climate indices months correlated against the LD record were selected based on the findings of Vance et al. (2013), which used September-October averages of ENSO indices. We propose to use a standard seasonal average for austral spring (i.e., September-November) in order to simplify the story, but still maintain consistency with the proxy records developed previously (i.e., December-March sea salt concentrations) This does not change the findings of the manuscript (e.g., the correlation between the LD summer sea salts and the ENSO indices are still significant, e.g., $r = 0.398$, $p = 0.01$ Southern Oscillation Index). We do not suggest that the ENSO impacts MBS and LD during different seasons, rather that the ENSO signal is preserved differently in the two ice core records, as these sites are $\sim$1000 km apart, at different elevations and subject to different synoptic scale processes. Udy et al. 2021 suggest differing moisture transport to the two ice cores sites, implying that any climate signals may be preserved differently at the two sites. One explanation for why the LD record preserves an ENSO signal in the summer may be because the LD record may be a noisier record in winter, and therefore any signals for the ENSO in winter may be masked by local weather systems (see L407). Future detailed analysis on precipitation at MBS is needed to determine whether this is the case, however, this is too large a task for the present study and is the subject of ongoing and future work.

L267 – This is confusing. Revise.

Author response: This will be changed to 'The composite years are based on anomalous sea salt years, while the months displayed were chosen based on months that had significant correlations with the relevant climate modes'.

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?

Author response: This will be changed to 'MBS ice core features 1975-2016'.

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.

Author response: We agree to edit the methods, results and discussion sections so that there is less overlap. In regard to this paragraph, the second Reviewer argues that the main result of the paper is the accumulation and sea salt datasets (rather than any climate mode signals). We agree with the second Reviewer, as this is the first manuscript for this new ice core, therefore, we propose to leave this paragraph in the results. However, we will revise for clarity.

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.

Author response: This will be corrected to 'The MBS sea salt site record is significantly correlated with the seasonal Multivariate ENSO Index, Niño 4, Niño 3.4, and the Southern Oscillation Index (June-November, see Table 1)'. As above – the September-October averages of the ENSO indices were based on the Vance et al. (2013), although we propose to extend this to include September-November to make it a seasonal average. June-November ENSO indices were correlated against the annual MBS site record because sea surface temperature anomalies and convection anomalies related to the ENSO tend to emerge in early austral winter in the equatorial and south-west Pacific and propagate to higher southern latitudes during austral spring and into summer (Fogt and Bromwich 2006; L'Heureux and Thompson 2006), and we are developing an annual record for MBS at this stage. This means, it makes more sense to test

against the spread of seasons where the ENSO anomalies develop in the Southern Hemisphere and high latitudes. In addition, we wished to align as closely as practicable to annual MBS sea salt record, and hence do not extend the ENSO indices into the following year. We have correlated the MBS sea salt record against the ENSO indices in May-December, June-December, May-November and the difference in r-values and p-values are negligible. Again, this is the first paper for MBS – there are currently multiple studies underway developing more understanding of the signals and mechanisms to deliver these signals preserved at MBS.

L326 – As commented earlier, it is important to refer to the Figures /Tables to guide the readers.

Author response: Apologies, all figures and tables will be correctly labelled and appropriately referred to in the results and discussion sections.

L342 – The scale on Fig 7 (also Fig 6) needs check. Seems the sign missing.

Author response: Apologies, negative signs on the scalebar will be added.

L360 – This section needs revision, as there is no discussion on the results on IOD.

Author response: We appreciate this is the case however, there is very limited literature on the link between the IOD and high latitude climate, particularly in the Indian Ocean sector. We will add more detail in the discussion section explaining the lack of information and data on the influence of the IOD on the high latitudes (particularly the Indian Ocean sector), which may explain why we do not get a statistically significant IOD signal in the MBS record. Moreover, the IOD is seasonally locked and may only be preserved, if preserved, in the seasonal MBS record, which has not been developed yet. Please note that a seasonal MBS record may only be developed after the uniformity (or otherwise) of annual precipitation at MBS is investigated thoroughly, which is ongoing work.

Also the data/figures are not referred.

Author response: Apologies, all figures will be referred to correctly and appropriately referred to in the results and discussion sections.

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 . . ..”

Author response: This will be changed to the ‘lack of a statistically significant SAM signal’.

Discussion L377 – Section 4.1 title doesn’t convey its content. May revise.

Author response: We will revise to better suit the content once the results, methods and discussion sections have been revised.

L415 – As commented earlier, it is important to explore the impact of extreme precipitation events on the proxy records discussed here. Such an evaluation would enhance its value for a journal like CP.

Author response: Determining the impact of extreme precipitation events at MBS in detail is a study in itself and will be investigated thoroughly in the near future. However, we propose to add additional analysis using the surface mass balance data from the Modèle Atmosphérique Régional to look at the frequency of precipitation at MBS at a monthly/seasonal scale.

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.

Author response: We disagree. In this study, we have not investigated the IPO as it requires a longer dataset. However, we do mention it in the discussion, as it is important to recognise that decadal variability may influence the strength of the ENSO signal in the MBS record, as it does influence the strength of ENSO signal in the LD record (Vance et al. 2013). There is strong decadal and multidecadal climate variability in Antarctica, which is often not captured in the observational data (Jones et al. 2016), and a highly cited reconstruction of the IPO has been developed form the LD record

(Vance et al. 2015). Determining decadal climate variability is a key objective of the MBS ice core project (Vance et al. 2016), hence we think it is important to state that decadal variability may influence the strength and the stationarity of any climate signal preserved in the MBS ice core. Moreover, the second Reviewer argues that the ENSO signal may change over a longer timescale, therefore we think it is important to mention the potential causes of decadal variability in the ENSO signal (i.e., the IPO), and that we will be exploring decadal variability upon the development of the longer MBS datasets.

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.

Author response: We propose to add more detail on mechanisms controlling the transport of ENSO signals from the equatorial Pacific to high-southern latitudes using previous studies (e.g., Turner et al., 2004; Vance et al. 2013, Fogt et al. 2006, Datwyler et al. 2020, and Clem et al. 2019). Figure 8 and the discussion around this figure is our first attempt at describing the mechanism linking the broader ENSO variability to winds in the southern Indian Ocean, and thereby to variability in sea salt aerosol generation and deposition at MBS. We will make this clearer in the manuscript and link it to previous work.

L450 – A discussion on the potential robustness of MBS records (compared to the LD site) for ENSO reconstruction would be useful.

Author response: Given the short record, we suggest there is some evidence for a robust ENSO signal. However, one of the benefits to studying ENSO in palaeoclimatology is the long-observed records of sea surface temperature spanning up to 150 years. As we continue analysing and dating the MBS record back in time, we will be able to determine with more confidence not only the robustness of the signal, but also any decadal variability (e.g., from the interaction of the IPO) or whether there is a stationary or non-stationary aspect to the signal. We will highlight this in more detail in the conclusion.

L459 – Exactly. This potential impact of extreme events needs to be explored.

Author response: As above - determining the impact of extreme precipitation events at MBS will be investigated thoroughly in the near future, as it is a large task and is likely a whole paper in itself. However, we propose to add extra analysis using the surface mass balance data from the Modèle Atmosphérique Régional to look at the frequency of precipitation at MBS at a monthly/seasonal timescale.

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.

Author response: This is based on Figure 8, as the winter anomalies are stronger and more extensive than the spring anomalies. However, we agree that this was not written very clearly. It will be revised.

L471 – This is speculative at this stage without discussing proof and reasoning.

Author response: This will be revised to ‘It is possible that particular off-shore circulation features shown in Fig. 8c and 8e are influenced by a teleconnection related to the ENSO’.

L473 – Section 4.3 may include a discussion on IOD signal (or lack of it) at the MBS site.

Author response: We will add more detail in the discussion section explaining the lack of information and data on the influence of the IOD on the high southern latitudes (particularly the Indian Ocean sector), which will help to explain why we did not find a statistically significant IOD signal in the MBS record. Moreover, the IOD is seasonally locked meaning that an IOD signal, if preserved, may only be preserved in the seasonal MBS record, which has not yet been developed.

L475 – Is this consistent with the Marshall et al. (2017) study? If not, may be some reasoning needs to be brought out.

Author response: Apologies, we are unclear why Marshall et al. 2017 would apply here. Marshall et al. 2017 discussed evidence of SAM signals at LD compared to Byrd (West Antarctica) ice cores in annual accumulation. We maintain that we will have to produce longer, and seasonally resolved records prior to definitively investigating whether there is a SAM signal preserved at MBS.

Conclusion L520 – Section 5.0 needs revision. It also needs to be nuanced that it’s a composite of 3 records.

Author response: The conclusion will be revised. When referring to the site average from the three MBS records, we will write the ‘MBS site record’ to avoid confusion. This was also noted by the second Reviewer. We will state the fact that we only use the upper section of the MBS ice core, while still mentioning that there is a longer record coming soon.

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.

Author response: Please note that the second Reviewer has asked for more emphasis on the accumulation and sea salt datasets, rather than any climate mode signals. Therefore, we will revise the conclusion and try to balance the emphasis on both findings.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2020-134, 2020.