

## ***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 #2**

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In the study by Crockart et al, the accumulation rate records and the chloride records of 3 shallow firn cores from Mount Brown South as well as from a new shallow core from Law Dome are presented. The correlation of these records to the El Niño Southern Oscillation signal is investigated by means of Pearssons correlation coefficient and discussed. The aim of the study is to show, that a future longer record from the site will contain climate variability signatures such as the Southern Oscillation and will give different/additional information than a comparable record from Law Dome.

Obtaining and analyzing ice cores from coastal Antarctica is highly valuable in order to resolve sub-annual and annual climate variability. However, the presented study aims to look at variability at larger time scales in order to evaluate the benefit of a future longer ice core record. This overall aim prevents a thoughtful analysis of the certainly

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valuable data available.

The paper would benefit from a clear and consistent outline on - how the cores and records are obtained - a clear presentation of the full data set - a clear outline on which time scales are investigated and why (by which species, resolution –which processing step) and what possible drawbacks/bias have to be considered, i.e. Example 1: comparing annual values of accumulation rate – by averaging the annual accumulation rates of all the three firn cores (arithmetic average?), problem: lack of high-res density – what is the influence of intrusion, single events on the overall record/correlation analysis; Example 2: looking over a time period of 40 years (in comparison to ERA5 etc)- which is by no means close to centennial. What is the significance of this correlation over this time period with respect to a future millennial record? Did you test for trends (affecting the correlation?) What if the correlation is (only) a result of recent changes and not due to variability itself? For variability analysis the trends have to be looked at (and removed)? - a better structure in distinguishing between results and discussion - a focus on the available time scale, the high-resolution data available and what one can learn from it

Further general remarks: 1. Terms on climate variability time scales are used inflationary and ambiguous throughout the text, for example: High-resolution ice core – long term climate variability and multi-centennial – all in one sentence/context. (Abstract, Line 21-22, Introduction Line 44), Usage of very different time scales (as relevant for the paper), for example: variability over past millennia (Intro, Line 55), sub-decadal (?) signals for climate variability (Intro, Line 126), multi-centennial (Intro, Line 86) Suggestion: There should be a clear outline of which times scales the authors aim to address with their data – both in resolution (sub-annual) and coverage (30-40 years!). It is in the nature of coastal high-res records, that they resolve-sub-annually ( a benefit which is unfortunately not discussed or presented here more in-depth). Overall the (only) time scale considered here is annual mean over a period of 40 years – for both accumulation rate and sea salt concentration.

2. Talking about climate signals/climate variability at different time scales would require a profound analysis of the climate signal contained in the record (especially given the fact, that overall a short period of 40 years are considered. What is the common climate signal in the three cores? And what the common climate signal with the core from Law Dome? Are the 40 years long enough? What about variability vs mean/trend?

3. Already in the introduction 14 different abbreviations are introduced (and used later in the text) – its hard to read and to follow your argumentation. Maybe it is possible to stick to few, relevant terms – as not all of the modes are used later or relevant for the paper (or one could stick to summarized, overarching terms of comparable modes).

4. Information to the firn cores Do you analyze the core until the surface? How do you deal with the upper meter(s). How do you cut the core in the field, transportation, cutting in the lab. . . A table of the exact coordinates, the length/logging depth etc is missing, and then the obtained coverage in time There is inconsistency in the number of cores included in your analysis. In chapter 2.1 (methods) it reads, that from the three MBS firn cores, the Bravo core is not used in the study. That makes 2 shallow cores plus the upper part of the main core. i.e three cores from the MBS site. However in line 133 it reads: The MBS record is unique in that it contains three short ice cores (20-25m) (. . .) in addition to the Main core. There is also unclear usage of the term: “record” – is this always meant as the stacked (averaged) record over the three (?) cores? This should be stated clearly.

Comments in detail: Line 21: wording in combination: high-res ice core vs long term, multi-centennial versus long-term Line 30: occurrence of moisture and aerosol intrusions -is briefly touched in the discussion but not shown in the data Line 52: high-resolution records are required to fill spatial gaps. . . High-res records address temporal information, not spatial? Line 141: please explain, what is meant by “wet deposition”? Line 151: The MBS record: It is not explained, how you derive the “record” here – what exactly is done? The record of the single accumulation rates or the stacked/merged? Line 160-165: In the figures only 1 record for LD is shown – how did you combine

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the old and new records? Where they exactly the same (in the common period)? A little note on how the two records are combined is needed here Chapter 2.4. Deriving accumulation rates from empirical density model? Are there now bag mean densities obtained in the field/lab? As accumulation rate is one major result of the study and it is based on the empirical equation, it would be good to show the density data (modeled in comparison to bag means). In any case, different layers of density will not be considered and may bias the derived ice equivalents. A more in-depth description/uncertainty analysis should be given (again, based on the fact, that this is one of major results of the paper). Line 198-199: I do not understand: You convert your profile to ice equivalent in order to do exactly this: to compare different layers of different depth of an ice core, no matter of the thinning (by compaction). If you refer to thinning because of flow then it reads very strange, given the fact, that you are looking at 25m depth max.

Equation 1: description/ labeling of the terms is missing (i.e d = depth, what are the number standing for?) Line 280: The MBS record. . . Again, it is not clear to what is referred here. Figure2: why do you not show the full record? Line 302/303: wind/blown snow effect- where has this been shown? (Reference or short explanation how)

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