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The Cryosphere Discussions
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Interactive comment on "A 125-year record of climate and chemistry variability at the Pine Island Glacier ice divide, Antarctica" *by* Franciele Schwanck et al.

Anonymous Referee #1

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General Comments

The work from Schwanck et al. concerns the detailed analysis of a new West Antarctic ice core (92 m long). An accurate elemental characterization of the core is here presented, based on the determination of several elements with an impressive resolution. Chemical records from 1979 to 2008 are thus compared to meteorological data reanalysis and to atmospheric back-trajectories. Among the many points discussed in the work two are of extremely interest, making the work worth to be published in this journal. 1- an impressive work was carried out to obtain an elemental compositional database composed by 2137 samples. 2- the possibility to compare such database with meteorological data has a great and poorly unexplored potential for the understanding of aerosol transport and deposition in Antarctica. Coupling ice core records and meteorological observations will give important advances to the interpretation of data extracted from ice cores.

Despite the great potential I believe that the paper could be greatly improved with further and additional data analysis. If on one side the authors could realize such an impressive database, the discussion side of the paper is rather poor. Discussion should be generally improved and some additional statistical tools could be applied to extract further information from the data and to obtain robust evidences. Several points present a poor discussion. A new revised version of the paper will substantially benefit from further data treatment. My final suggestion is to consider the work for publication after major revision.

Specific Comments

The key point of the work is the comparison between meteorological and ice core data. This was possible because the considered site presents a high snow accumulation rate which allowed obtaining records with high temporal resolution. The description about the development and validation of the chronology is rather poor, not to say completely lacking. If high resolution meteorological data are compared to record obtained from natural archives, it is necessary for the latter ones to be accurately and precisely dated. The authors just state that the chronology was based on annual layer counting (using Na and S records) with the additional consideration of 4 major volcanic eruptions. No further details are given. A previous work (Schwank et al., 2016 Atmos. Environ.) is cited as reference for the chronology, but also in this work few details are found. This part needs a substantial extension. A first element would be the comparison between annual layer counting and historical eruptions, which error is found? Is this consistent with a record which is claimed to present a seasonal resolution?

Another important part of the paper is dedicated to the calculation of different contributions for each element, i.e. crustal, volcanic, marine and biogenic. This part is a little bit confused. The authors follow three different approach: the selection of reference elements and reference elemental ratios, the calculation of enrichment factors and the calculation of Pearson's coefficients. It would be important to put all this elements together, discussing them in a comprehensive way and not separately. If the discussion is kept separated controversial results are found. For example we can consider Mg. According to the use of reference elements and ratios it has a dominant marine source (30 %, supplementary material) and a secondary associated to crustal material (5 %). But Pearson's coefficients reveal that Mg is strongly associated to Al, a typical crustal element. Also successive interpretation about the comparison with meteorological data point to strong similarities. The application of a multi-variate statistical tool as principal component analysis could greatly improve this section of the work. PCA could help the authors to identify different contributes and to understand the role played by each element in these different contributions. Since its starting point is the calculation of Pearson coefficients please consider to make a further step in this sense and complete data treatment with PCA. In addition I suggest the authors to improve the method they used to distinguish ss and n-ss Na. The assumption that Al is only crustal is justified, but this is not the case for the assumption that Na is only marine. Please consider to separate the two fractions by using Al as crustal reference and an UCC Al/Na ratio.

Section 3.2 should be deeply revised. In its current version it seems a review about atmospheric depositional issues in Antarctica, but very poor discussion points are reported. High time resolution data described in this work should be better exploited to understand seasonal dynamics. Are elements presenting parallel seasonal oscillations? If this is not the case and no significative observations are found please consider to dramatically shorten the section and to merge it with section 3.4, so as to have a single section about temporal variability.

Section 3.4 I suggest to develop the discussion presented here with a comparison with back-trajectories analysis and seasonal trends. Some interesting trends are observed but their interpretation is poor. The authors present a huge amount of observations concerning literature and what was observed in other studies, but the connection between their evidences and literature is lacking. For example looking at Fig.7 the correlation of Al and Mg is completely different with respect to the other elements. This is clearly pointed in the text, but a true interpretation is missing. The phenomenon could be related to a different seasonality pattern, with dust peaks and marine aerosol peaks occurring in different periods of the year, when SST is different.

In the light of these comments conclusive remarks will need a final revision.

Technical Comments

Line16: insert “of” between “reanalysis” and “trace”

Line22: remove “that of the”

Line27: please consider to add a short passage about the importance of WAIS in relation to climate dynamics and sea level.

Line40: change “recognize” with “distinguish”

Line41-42: remove from “furthermore” to “continent” and replace with “both presenting specific seasonal cycles.”

Line54-56: please reformulate, it is not clear.

Line56: change with “Another primary source of aerosol is mineral dust. It is transported. . .”

Line57: add a further reference. Li et al., 2008 is based only on models, add Revel-Rolland et al., 2006 EPSL which is based on isotopic data from EPICA Dome C. Also the consideration of New Zealand as dust source for Antarctica is still only an hypothesis based on modeling works, no direct evidences are known.

Line69: add “of WAIS” after “systems”

Line76: please give a reference for modern snow accumulation rates in the considered

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Discussion paper

area

Line86: in the text Mount Johns is never described. Is it a topographical height of Pine Glacier? A peripheral area of this glacial system?

Line116-117: please specify only significant digits

Line310: some references concern Talos Dome, which is located in EAIS, not WAIS

Table1: is it possible to add a further column with average uncertainty for each element?

Figure2: I guess that y-axis of upper figure is wrong. Al EF should be 1, not 0.1. Is this right?

Figure3: here you present some examples to show seasonal variations. You considered Na and Mg. What about considering also Al? Being exclusively crustal it could present a different behavior.

Figure4: specify in the caption that volcanic eruptions were identified using sulfates

Figure6-7-8: Why for each figure you report different elements. It would be nice to have three perfectly comparable figures with all the elements you considered in this work. Did you try to apply the same procedure to nss and ss-S. It would be nice to see them.

Best Regards

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