Answers and explanations to all detailed questions and annotations raised by the reviewers are provided in the following. (RC: Reviewer comments; AC: Author comments).

Specific Comments

RC 1: 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?

AC: *The dating was improved with the use of stable isotope data (these data were not available until then). More details about dating will be added to the text and to the supplement information. Manual interpretation of the data was done by multiple individuals to identify the individual layers. The CCI software package (Kurbatov et al., 2005) was also used to identify matching seasonal peaks from Ca, Na and Sr and the major historical volcanic eruptions. In this study, water isotopes were used to confirm the dating previously performed in Schwanck et al., 2016.*

RC 1: 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.

AC: *We agreed that this chapter was confusing, so we decided to take the comment into account and redo the analysis using PCA. The PCA resulted in four PCs. PC1 is dominated by Ba, K, Mg, Mn, Na, and Sr, accounting for 42.24% of the total variance. PC2, dominated by Al and Ti, accounts for 13.27% of the total variance, while K and Na are negatively correlated. PC3 is dominated by Ba, Fe, and Ti, accounting for 11.16% of the total variance. PC4 is dominated by Ca and Sr, accounting for 8.11% of the total variance, while S and Mn are negatively correlated. We are still working on the interpretation of these results.*

RC 1: 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.

AC: *We revised the whole calculation of ss and nss taking Al into account as crustal reference.*

Non-sea-salt ratios were calculated using the equation reported below (Palmer et al., 2002, Becagli et al., 2005):

$nssS = S - 0.084 \times ssNa,$

where S is the total sulfur concentration on the sample, 0.084 is the mean S/Na ratio in seawater (Lide, 2005) and ssNa is the Na actually derived from sea spray. Since some Na derives from continental dust, ssNa was calculated using the four-equation system reported below:

$ssNa = Na - nssNa$

$nssNa = nssAl \times (Na/Al)_{crust}$

$nssAl = Al - ssAl$

$ssAl = ssNa \times (Al/Na)_{seawater},$

where the mean Na/Al ratio is 0.3315 in the crust (Wedepohl, 1995) and the mean Al/Na ratio is 0.000000185 in seawater (Lide, 2005).

RC 1: 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.

AC: *We agree. Sections 3.2 and 3.4 have been merged into a single section (Section 3.2 - Interannual atmospheric variability). We changed the order of discussion, placing the transport session (3.3 - Atmospheric transport to Mount Johns ice core site) to the end. We are also expanded the discussion of results.*

RC 1: 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.

AC: *The section is being rewritten and improved. We agreed that some interpretations were superficial and we are expanding on these issues. The order of the sessions was changed, leaving the discussion of transport to the end. We are also expanding the discussion on how transport and seasonality have affected trace elements concentrations on MJ area.*

Technical Comments

RC 1: Line16: insert “of” between “reanalysis” and “trace”

AC: *Done*

RC 1: Line22: remove “that of the”

AC: *Done*

RC 1: Line27: please consider to add a short passage about the importance of WAIS in relation to climate dynamics and sea level.

AC: *The text was changed to: “During recent decades, rapid changes have occurred in the WAIS sector, including flow velocity acceleration, retraction of ice streams, and mass loss (Pritchard et al., 2012). These changes influence the global climate through their contributions to sea level rise (Pritchard et al. 2009, Shepherd et al. 2012) and deep ocean circulations (Holland and Kwok, 2012). WAIS contains sufficient water to raise the global sea level by over 3 m (Bamber et al., 2009, Fretwell et al., 2013)”.*

RC 1: Line40: change “recognize” with “distinguish”

AC: *Done*

RC 1: Line41-42: remove from “furthermore” to “continent” and replace with “both presenting specific seasonal cycles.”

AC: *The text was changed to: “To interpret chemistry records from Antarctic ice cores, it is imperative to distinguish the long-range transportation of continental dust and regionally derived sea salt, both presenting specific seasonal cycles.”*

RC 1: Line54-56: please reformulate, it is not clear.

AC: *The text was changed to: “Marine aerosol concentrations are strongly linked to cyclone frequency and intensity that provides high wind speeds over the ocean surface, with the aerosols deposited along the storm track.”*

RC 1: Line56: change with “Another primary source of aerosol is mineral dust. It is transported. . .”

AC: *Done*

RC 1: 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.

AC: *Done*

RC 1: Line69: add "of WAIS" after "systems"

AC: *Done*

RC 1: Line76: please give a reference for modern snow accumulation rates in the considered area

AC: *We add Medley et al., 2014*

RC 1: Line86: In the text, Mount Johns is never described. Is it a topographical height of Pine Glacier? A peripheral area of this glacial system?

AC: *Mount Johns is a nunatak in the Pine Island Glacier area.*

RC 1: Line116-117: please specify only significant digits

AC: *We have removed the values. Details about MDL values are contained in the supplement information.*

RC 1: Line310: some references concern Talos Dome, which is located in EAIS, not WAIS.

AC: *The reference was being used as an example but to avoid misunderstanding we decided to remove.*

RC 1: Table1: is it possible to add a further column with average uncertainty for each element?

AC: *Done*

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

AC: *This is correct; some elements presented EFc less than 1.*

RC 1: 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.
AC: *Figure 3 was removed from the text and added to the supplement information. Mg was replaced by Al in the graph.*

RC 1: Figure4: specify in the caption that volcanic eruptions were identified using sulfates.
AC: *Done*

RC 1: 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.
AC: *The graphs that were not shown in the figure did not present very significant results. These have now been added. We did not simulate for ss-S and nss-S only for total S.*