Response to Referee 1 comments

The authors (TA) would like to thank Referee 1 (R. 1) and Referee 2 (R. 2) for the time taken to review this manuscript. We value the referees' general and specific comments, and suggestions, and we will consider them in detail to prepare the revised version of this manuscript. Here we present our preliminary response to the main comments of R. 1 and R. 2.This response will be further expanded and detailed during the final response period.

R. 1: *This paper presents new ice core chemical data for a coastal region of Antarctica. It interprets particularly the sea salt chemistry, and attempts to discuss the mechanisms behind sea salt production and deposition from the data. The data have some interest, particularly the unusual record from S100, and it may be possible to make a workable paper out of them. (The application of this paper is somewhat reduced because sites so close to the sea can be interesting but do not tell us too much about processes affecting inland sites.)*

TA.: The data presented in this manuscript contribute to fill in the gap in ice core records that exists for low elevation coastal areas. The importance of acquiring more data from the coastal areas is stressed by recent research (Stenni et al., 2017, and Thomas et al., 2017), and also the general comments of R. 2. We agree with R. 1 that those sites are interesting. However, information about inland sites cannot be expected from a study dealing with data from coastal sites only.

Response to R. 1 points:

Point 1: *The snow pit data are all from samples covering less than a year of snowfall. This makes it impossible to use the average values generated quantitatively, both because they are not a real yearly value, and because interannual variability means that the average for one year should have a huge uncertainty on it. There was a time when we were so desperate for new data from unexplored parts of the continent that we would at least consider surface snow data from part-years but those days are over. Without the snow pit data, the discussion of spatial variability is impossible, so section 3.4, Table 4 and all discussion about spatial variability should be removed from the paper.*

TA. We agree. We will discuss the spatial variability using only the ice rises and S100 cores, or as the referee suggested, eliminate the section.

Point 2: *The authors seem to be under the impression that if they don't observe negative nss sulfate, then there is no fractionation and no sea ice source. Of course this is not correct: while sea ice fractionation removes sulfate and causes negative nss-sulfate values, biogenic sulfate gives positive nss-sulfate. Only if the former overwhelms the latter will net negative values be seen. At sites very near the coast where marine biogenic inputs are large, this makes diagnosing fractionation tricky. As a rough estimate, one can note that typical values of MSA/mss-sulfate in biogenic input are 20% (Legrand and Pasteur 1998). From that we can estimate for example that biogenic sulfate at BI could easily have contributed all the sulfate seen, so that fractionation must have occurred. Uncertainty on the MSA concentration and the ration MSA/nss-sulfate makes this calculation very uncertain, but just illustrates that any of these sites could be experiencing large proportions of fractionated aerosol. The nss-sulfate discussion is valuable but needs to be done in a much more sophisticated way.*

TA. We do not think that no negative nss-sulfate values mean no fractionation. We will re-write the part in which we introduce the nss-sulfate findings so the message can be clearly delivered. We will also improve the discussion, including MSA and ss-fractions, as the referees suggested.

Point 3: *The authors use the correlations between concentration or flux and snow accumulation rate to try to diagnose the deposition mechanism. This could have some value if interpreted sensibly. However for S100 (1950-2000), it is obvious that the main feature is an immense rise in Na and Cl (factor 6) accompanied by a small drop (perhaps 20%) in accumulation rate. The relationship between these two trends will dominate any correlation but a 20% drop in snowfall cannot in itself cause more than a 20% increase in concentration even if dry deposition dominates completely. One simply cannot learn about dry and wet deposition for this site: something else is overwhelming the situation by causing a huge increase in sea salt to the site.*

TA. We will check this part of the analysis. However, we do not understand the referee sentence "One simply cannot learn about dry and wet deposition for this site: something else is overwhelming the situation by causing a huge increase in sea salt to the site". Actually, because of this increase in Na^+ and Cl^-, we tried to constrain wet and dry deposition; if neither wet nor dry deposition is the cause of the increase in Na^+ and Cl^- (as we understood from the referee comment), then what else can we causing the increase? We would be very grateful if the referee could clarify what she/he meant so we can improve the analysis following the referee's suggestion to this point.

Point 4: *The something else is causing huge sea salt concentration increases after 1950. It cannot be a change in the source to the ice shelf as a whole, since KC doesn't see it. I feel I am missing crucial information to allow me to interpret this. The obvious explanation would be that S100 has been getting closer to the ice shelf edge since 1950. But the paper gives no glaciological information that would allow us to interpret that. My assumption would be that the ice front at S100 occasionally calves icebergs, and that S100 is moving forwards at 10s to 100s of m/yr. The authors need to check and discuss what happened between 1950 and 2000. Did the S100 site simply get nearer the ice front?*

TA. We didn't include more glaciological data because those have been published elsewhere. However, we agree with the referee's point and we will include relevant glaciological data of the area so the discussion can be clarified and better understood.

Response to Referee 2 comments

Point 1: *some experimental procedures should be clarified*

TA. We will clarify the procedures as suggested by R. 2

Point 2: *ss- and nss- fractions of most of the analyzed components (especially Na and Ca) should be calculated as more reliable markers of sea spray (ssNa) and crustal (nssCa) contributions.*

TA. Since we used Na^+ as reference ion for the calculation of nss-fractions, we assumed all Na^+ has a sea-salt origin, therefore, we cannot separate it into nss-Na^+ and ss-Na^+. We will present such apportioning for the other ions (Table A) in an additional table in the manuscript. In fact, we have done it in a previous version of the manuscript but we decided to let it out to focus on the

$nssSO_4^{2-}$ only and keep the paper concise. But we agree with the R. 2, and including the nss-fractions (and ss-fractions) for all ions will improve the discussion.

Table A. Median nss-ion concentrations (in μmol L^{-1}) in the KC, KM, BI, and S100 cores.

Core	*Period*	*$nssCl^-$*	*$nssSO_4^{2-}$*	*$nssK^+$*	*$nssMg^{2+}$*	*$nssCa^{2+}$*
				μmol L^{-1}		
KC	1958–2007	0.1	1.1	1.8×10^{-2}	2.2×10^{-2}	0.3
KM	1995–2012	3.9	0.7	0.2	7.6×10^{-3}	0.4
BI	1996–2012	0.7	0.7	6.4×10^{-2}	-3.8×10^{-2}	0.2
S100	1737–2000	-3.1	-7.2×10^{-2}	3.4×10^{-3}	-0.4	0.1
S100	1995–2000	-28.4	-4.8	-8.8×10^{-4}	-5.5	-5.4×10^{-2}
S100	1737–1949	-1.9	0.1	-7.5×10^{-4}	-0.2	0.2
S100	1950–2000	-18.3	-3.1	4.4×10^{-2}	-2.6	2.7×10^{-2}

Point 3: *seasonal characterization of the sub-samples should be made taking into account the $\delta^{18}O$ profiles, instead of using an interpolation procedure.*

TA. We agree, we will re-work this section using the $\delta^{18}O$ maxima/minima chronology that we previously published. For that we will use the winter minima and summer maxima found in the $\delta^{18}O$ profiles for the KC and BI cores (Vega et al., 2016) (Figure A), and seasonalities in the $\delta^{18}O$ found in the S100 core (Kaczmarska et al., 2004), to obtain the subannual variability of the different ions, including ss-fractions and nss-fractions.

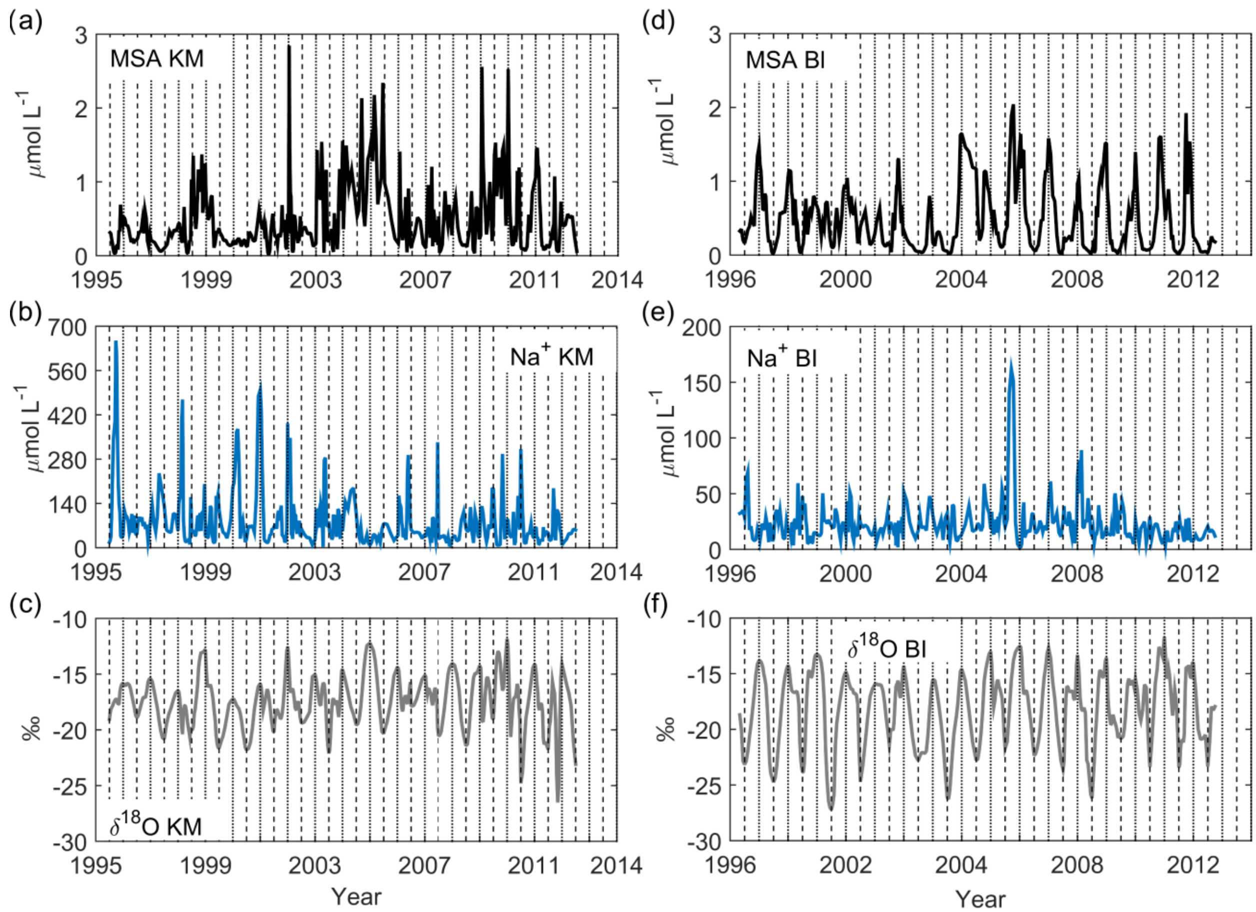

Figure A. Seasonality of (a, d) MSA, (b, e) Na^+, and (c, f) $\delta^{18}O$ for the KM and BI cores. Dashed lines and dotted lines indicate winter (summer). Figure from Vega et al. (2016).

Point 4: *the evaluation of the spatial variability by snow pit data appears to be not significant, because of the short record (lower than 1-year deposition).*

TA. This was also pointed by R. 1, so we will consider removing this section or else, re-write it considering only the core data.

Point 5: *the evaluation of ss-sulfate depletion from negative nss-sulfate values has to be completely revised.*

TA. We will do as suggested by R. 2 in the revised version of the manuscript.

Point 6: *I'm not convinced about the explanation of abrupt changes in sea salt deposition since 1950 in the S100 ice core.*

TA. We will re-write the discussion. We will follow R. 1 and R. 2 comments and include glaciological data that will give more clarity to our explanation. Without satellite imagery before 1979, it is not possible to proof our hypothesis. It is a plausible explanation, however, there might be other things that have played an additional role.

References

Kaczmarska, M., Isaksson, E., Karlöf, L., Winther, J-G., Kohler, J., Godtliebsen, F., Ringstad Olsen, L., Hofstede, C. M., Van Den Broeke, M. R., Van De Wal, R. S.W., Gundestrup, N.: Accumulation variability derived from an ice core from coastal Dronning Maud Land, Antarctica, Ann. Glaciol. 39, 339–345, 2004.

Stenni, B., Curran, M. A. J., Abram, N. J., Orsi, A., Goursaud, S., Masson-Delmotte, V., Neukom, R., Goosse, H., Divine, D., van Ommen, T., Steig, E. J., Dixon, D. A., Thomas, E. R., Bertler, N. A. N., Isaksson, E., Ekaykin, A., Frezzotti, M., and Werner, M.: Antarctic climate variability at regional and continental scales over the last 2,000 years, Clim. Past Discuss., doi:10.5194/cp-2017-40, in review, 2017.

Thomas, E. R., van Wessem, J. M., Roberts, J., Isaksson, E., Schlosser, E., Fudge, T., Vallelonga, P., Medley, B., Lenaerts, J., Bertler, N., van den Broeke, M. R., Dixon, D. A., Frezzotti, M., Stenni, B., Curran, M., and Ekaykin, A. A.: Review of 23 regional Antarctic snow accumulation over the past 1000 years, Clim. Past Discuss., doi:10.5194/cp-2017-18, in review, 2017.

Vega, C. P., Schlosser, E., Divine, D. V., Kohler, J., Martma, T., Eichler, A., Schwikowski, M., and Isaksson, E.: Surface mass balance and water stable isotopes derived from firn cores on three ice rises, Fimbul Ice Shelf, Antarctica, The Cryosphere, 10, 2763–2777, doi:10.5194/tc-10-2763-2016, 2016.