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# *Interactive comment on* "Spatial and temporal variability of sea-salts in ice cores and snow pits from Fimbul Ice Shelf, Antarctica" *by* Carmen Paulina Vega et al.

**Anonymous Referee #1**

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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.) However at the moment the paper suffers from three very major flaws, and an omission that render most of the interpretations dubious:

1. The snowpit data are all from samples covering less than a year of snowfall. This



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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 snowpit 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.

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.

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





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by causing a huge increase in sea salt to the site.

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?

I'm afraid all these points call for a major rethink about the purpose and conclusions of the paper. I will discuss a few more detailed points, but clearly any revision will be close to a new paper (it's borderline between major revision and reject) and will need reviewing again.

Abstract, page 2, line 5. As discussed above, the authors cannot conclude about dry deposition from the method they used. The very high concentrations do suggest a very high atmospheric concentration above the site by the year 2000, which would likely be both wet and dry deposited (such local material would have large particle sizes so would deposit fast). But this cannot exactly be described as dry deposition in the conventional aerosol dry deposition sense.

Page 10. The MSA-nitrate connection is overdone here. They surely end up on the same PC mainly because they don't show the sea salt pattern. We are not shown data that would allow us to judge this. However, for sure trying to link nitrate to MSA as a fertiliser seems far-fetched for a number of reasons. The Southern Ocean is not generally considered to be nitrate-limited; it seems unlikely that nitrate in the ocean is dominated by local atmospheric deposition. If you want to make this point you need to show data that would make a convincing case that high nitrate really is associated with





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high MSA.

Page 11, line 8: they don't all show a 6 fold increase.

Section 3.3. You are doing something very difficult here. Please start with a discussion about the caveats: that it is very difficult to reliably divide the annual layers into 4 sections of equal time so the uncertainty on this is very large.

Section 3.4 should be removed as discussed above. At the end of the section, you dismiss the importance of elevation, but this cannot be excluded as a factor for the 3 ice rises reaching 200 m.

Section 3.5 – see discussion above.

Section 3.6 is very confused. The best way to treat this is to use the slope of lines such as that in Fig 5 to estimate the degree of fractionation, rather than trying different ratios (0.06, 0.04, 0.02). However, the line in Fig 5 should be a best fit through all the data (not just the negative), and should not go through zero (because when there is no sea salt there is still nss-sulfate from biogenic sources). Treated this way, I guess the slope will be about -0.04, implying 66% fractionation for the whole dataset (not just the post 1950s unless you see a significantly different slope for the two time periods)

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