

This paper uses surface samples from a series of traverses between the coast and the inland plateau Dome A station, along with 3 pit sequences, to study the geographic pattern of chemical concentrations. At heart it is a very simple study, effectively building on reviews written 15 years ago, but with new data from just a single geographical region. The paper throws a lot of different methods (principal component analysis, enrichment factors, ternary diagrams) at the data. Despite a comparatively long paper, the findings of the study are really nothing new and rather obvious: sea salt ions are closely related and at higher concentrations near the coast; ions such as Ca are mainly terrestrial, sulfate has a marine biogenic source.

I find myself a little torn as to what to recommend for this paper. The data are clearly rare in the sense that there are few data from this sector and from inland sites in general other than Vostok, Dome C and a couple of other sites. The authors do understand previous work and have presented their data in the light of that work. However the whole study is very hampered by the unfortunate fact that surface samples (3 cm surface skims) are just really unsuitable for understanding the chemical climate, and despite the work on the pit samples, it is the surface samples that are the bulk of the paper (7 of 12 data figures plus both tables). Because surface samples inevitably do not cover a full year, it is hard to know what they reveal: at least in some cases they give a misleading impression. For example all the surface samples are collected in the summer, when generally sea salt is high and sulfate is low. Surface samples collected in winter might give a completely different impression, but the paper doesn't make this clear. Surface samples probably represent only a single snowfall, so comparing them from year to year with any seasonal weather statistic (as in Figure 13) is not appropriate.

In the end, I want to be generous and say that the data deserve to be published but a more modest paper is needed, in which the shortcomings of the sample set are more clearly explained, and the paper is stripped down to a shorter length (with perhaps 6 figures and the rest removed or at least moved into the supplement). I don't think the paper brings much insight but it would be a shame if the data were not made available in the peer-reviewed literature so I will recommend major revision, and highlight below where I think the paper can be shortened.

Comments on the text:

Abstract is largely OK, but will need to be shortened in line with the text. The section about ternary diagrams is not needed in the abstract as it adds little to the rest of the text, and the part about SIOL should be excluded.

Line 33/34 "In the interior areas, the negative nssSO<sub>4</sub><sup>2-</sup> signal in winter snow resulted from inputs of sea salts being completely swamped by the contribution of marine biogenic emission": this doesn't quite make sense. What you mean is that there are high (positive) nss sulfate in inland snow because of marine biogenic sulfate. You have no data on whether negative values would have been seen, so the current wording serves to confuse the reader.

The introduction is generally quite good and the English (with a couple of exceptions that will be picked up in proofreading) is fine.

Methods: After line 154, it should be mentioned that 3 cm at a density of 0.33 as assumed elsewhere is only 1 cm water equivalent (compare snow accumulation in Fig 2a) and therefore the surface samples represent at best a summer sample and in many cases probably a single snowfall.

Fig 2 and section 3.1 already illustrates the problem with the study: there is huge variation at a single site between years, and yet you have no idea whether this variation reflects changes from year to

year, or from week to week within a year. Given that the samples are inevitably collected over a period of days to weeks within a year, much of the spatial variability can arise from temporal change in practice. This doesn't completely invalidate the work but it should be explained.

Fig 3b. I don't see the value of packaging all the samples into a single wheel like this. Firstly how is the calculation done: do you add the concentrations from each site (thus giving more weight to the samples with high concentrations) or is each site normalised before averaging? But wouldn't it be more interesting to show this wheel separately for groups of samples, eg <200 km from coast, 200-600 km, and >600 km. Then you could discuss in a holistic way how the composition changes as you go inland.

Fig 4. Please state in the caption the year the pits were sampled. I know it's somewhere in the text but it's needed here.

Fig 5 can be removed – it adds nothing, and it is sufficient just to say that the accumulation rate is too low at P3 for seasonal variability to be apparent. If necessary simply point out the relevant section in Fig 4.

Fig 6 is unnecessary. Because it pools data from different sites and years the statistics shown really have no meaning. If you really like it, please put it in the supplement.

Section 4.2: PC1 which is clearly sea salt is fine. However it is then rather obvious that Ca as a terrestrial ion, ammonium and sulfate fall into other PCs, and this can be said much more briefly. Personally I think Table 2 is sufficient and Table 1 adds nothing, but I don't insist on losing it.

Fig 9 and 12 and the associated text seem to add little, and they should go into the supplement of be deleted.

Fig 13 and the associated text are very misleading. The three traverses show different values for different sampling dates, but this could be day to day, week to week, month to month or year to year variability and trying to associate with seasonal indices is not relevant. To carry out any such analysis you'd have to calculate the index for the precise dates of the snowfall the surface samples represent. This section should be removed, as should supplement Fig S3.

The other supplement Figures (S1 and S2) also don't add to the message and should not be included (S1 is essentially identical to Fig 4a with the addition of ammonium, and Fig S2 shows information that is already visible in Fig 4a).