

Interactive comment on “A spatial framework for assessing current conditions and monitoring future change in the chemistry of the Antarctic atmosphere” by D. A. Dixon et al.

Anonymous Referee #2

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Overall assessment

This paper presents new data on snow chemistry data (major, minor and trace constituents) collected along overland traverses in extensive inland regions of Antarctica from which only limited data were previously available. The paper discusses the changes in snow chemistry and physical properties (e.g., grain size) observed along the various transects, and offers some interpretation for the observed variations. It also discusses possible source contributions from volcanic outgassing to the chemical burden in Antarctic snow. The datasets presented in the paper are new and very substantial, and this is a welcome contribution to knowledge of the glaciochemistry of the

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Antarctic interior. However I find that the paper, in its present form, lacks a clear focus, and the data are presented and discussed in a manner that is unappealing and at times plainly uninteresting. This is more a reflection of the structure and presentation style of the manuscript, than of its data content. I also find that there is an uncritical use of multivariate statistics (EOF) in interpreting the datasets, which is often unjustified. Overall, I think the paper deserves publication, but it should be reformatted considerably to give it a clearer focus, articulated on specific scientific questions or hypotheses, and some efforts should also be made to better constrain the interpretation of EOF analyses using statistical validation tools.

Other evaluation criteria:

Does the paper address relevant scientific questions within the scope of TC?

The paper does not actually present any specific hypothesis to test or verify, nor does it present specific questions to answer, which I find problematic in the sense that it lacks a clear focus. The paper does, however, present new data on snow and ice chemistry from previously undocumented regions in the interior of Antarctica, and these data serve to define a baseline against which future changes in the chemistry of Antarctica's atmosphere can be evaluated. In this sense, it makes a meaningful and useful contribution to the field of cryosphere studies.

Does the paper present novel concepts, ideas, tools, or data?

The data are certainly new (see above). The methods used to produce the data are not new in themselves. The use of empirical orthogonal function (EOF) analysis has been frequently used by some of the authors of this paper to evaluate large glaciochemical datasets, but I find that their interpretations derived by the use of this method are uncritical and not always sound, and EOF is now too often being used -and sometimes abused- as a recipe. On the other hand, the paper presents an interesting approach whereby remotely-sensed data on snow properties are used to help interpret glaciochemical data. This aspect is an original methodological contribution.

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Are substantial conclusions reached?

Not many, given the enormous amount of raw data presented. The main conclusion (and most robust one) appears to be that glazed dune and non-dune regions of the Antarctic interior yield surface snow chemistry values that are modified by sublimation processes, and these areas are therefore not suitable sites for ice-core studies of climate and atmospheric variability. This is not an unexpected finding, but the data presented make the case very clear. Other observations, such as the correlation between inland distance, elevation, accumulation, $\delta^{18}\text{O}$ values and sea-salt content, essentially confirm findings from previous studies. These are not novel findings, therefore, but the paper again provide additional observational data to back up previous interpretations. The discussion on trace element enrichments in snow from possible volcanic outgassing is interesting, although it rests on indirect inferences, rather than direct evidence (e.g., plume tracing). On this topic, I find that the conclusions section (section 4) is mixed with elements of discussion that should really be included in the previous section (section 3.4.4).

Are the scientific methods and assumptions valid and clearly outlined?

Too much emphasis is placed on the results of the EOF analyses of the glaciochemical data, and there seems to be a lack of understanding, or absence of discussion of, the uncertainties and caveats associated with this method. To use an extreme example, one can generate statistically meaningful eigenvectors from completely random time series. Statistical significance does not automatically confer physical meaning. Some plausible physical interpretation has to be proposed for the eigenvectors, and some mathematical validation is warranted, and in that respect I find the paper is weak. A good summary of some limitations of EOF analysis is given by Monahan *et al.* (2009) *J. Climate* 22: 6501-6514). I suggest the meaningfulness of the various eigenvectors should be tested against Monte-Carlo simulations with fake, random data series.

Are the results sufficient to support the interpretations and conclusions?

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With regards to the main conclusions, some are reasonably well-supported (like the conclusions arising from the comparison of the physical properties of the glazed areas with the glaciochemical data), while others are not so solid. In particular, many of the inferences based on EOF analyses assume that the eigenvectors necessarily imply some underlying physical relationship(s) between the associated variates. This is something that needs to be demonstrated, not simply taken for granted as a premise.

Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

Since the glaciochemical dataset is not included with the paper (so far as I now), verification of the results is not possible. I assume that there is probably a strategy to archive these data in an open data repository, but no mention is made of it in the paper.

Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes.

Does the title clearly reflect the contents of the paper?

No. I think the paper should be re-titled in a more explicit way. For example, it could be entitled: "Variations in snow and firn chemistry along ITASE traverses" or "Effect of surface glazing on surface snow chemistry in the interior of Antarctica", or something along these lines. The title should indicate clearly what is being presented. The present title is too vague does not announce the subject explicitly.

Does the abstract provide a concise and complete summary?

Yes.

Is the overall presentation well structured and clear?

I find the presentation of the glaciochemical data very dry and unappealing to read. The graphs suffer from several problems. There are too many tables, especially those

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presenting EOF results. See additional comments.

Is the language fluent and precise?

Generally, yes, but the presentation of data is dry and rather uninteresting to read (see above).

Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Yes.

Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

With respect to the manner in which the data are presented, I find that the graphs on figures 3 to 10 are difficult to read. Sometimes the ordinates are linear, sometimes on a log-scale, and the axes are so small as to be barely readable. The different scales used for the different glaciochemical elements tend to exaggerate the variability, such that most profiles look similar. The presentation of data used by Betler et al. (2005, *Annal. Glaciol.* 41: 167-179), using colour-coded maps, was far more effective and appealing to read. This would also allow one to relate, at a glance the glaciochemical data variability with the presence / absence / proximity of glazed areas, or aerosol source(s) (e.g., Trans-Antarctic Mountains).

Are the number and quality of references appropriate?

Yes.

Is the amount and quality of supplementary material appropriate?

There is no supplementary material that I am aware of.

Additional comments.

The paper lacks focus. There is an enormous abundance of data presented without a

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clear hypothesis or question to answer / test / address. It makes the manuscript unappealing and at time uninteresting. It reads too much like a report, as opposed to scientific paper with a clear message. I would suggest the authors re-write the manuscript with a more specific focus. For example, the properties of the glazed dune/non-dune areas would make a good subject on its own. As it is presently, this paper tries to present too much information without a clear focus.

With respect to the EOF analyses: EOF modes that account for < 10 % variability are generally suspect and could be mathematical artefacts. Statistical validation techniques need to be used to verify the robustness of the various modes. The plots of EOF components against the traverses (figures 4, 6 and 8) are not very telling. EOF1 and 2 show distinctive spatial patterns (one being essentially the complementary of the other) but the remainder of the EOF series looks suspiciously like noise to me.

In many, if not most instances, I think the use of EOF analysis is unjustified or poorly justified. Most often, the data speak by themselves. Many of the relationships between the variates are plainly obvious from the transect plots and the use of EOF analysis does not seem supply much additional knowledge. The most obvious patterns are the decrease of $\delta^{18}\text{O}$, accumulation and temperature with distance inland, and with higher elevation, and the opposing pattern for backscatter and grain size (e.g., Fig. 3). Only hardly needs an EOF analysis (as is done in the text) to highlight these relationships. Likewise, the greater variability in the surface snow chemistry in the glazed areas is plainly obvious on the transect plots, and does not require EOF analysis to be demonstrated. If the objective of using EOF analysis is to "reduce" this vast multivariate data array into fewer components, then one should make the choice of either presenting the EOF eigenvectors as surrogates for the data, or present the actual variate plots, but not both. This creates an overload of information that is often redundant.

The plots of variates across the transects (Figures 3 to 11) are difficult to read. The numbers at the top of the plots that identify the coring sites and which allow one to determine the direction of progression are almost unreadable, such that ones does

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not know easily where the distance (on the x-axis) is measured from. The overlays of colours on Fig. 10 area also difficult de decrypt.

In summary:

I think there is a highly valuable dataset presented here, the fruit of an enormous effort of collection from a large number of participants. But the paper, in my opinion, does poor service to the value of the dataset, because it lacks focus and offers an analysis that is, in part, poorly constrained by a somewhat sloppy use of mathematical methods. The most solid aspects of the discussion are, I think, those that pertain to (1) the properties of the glazed areas; and (2) the volcanic contributions to the snow chemistry.

My principal criticisms of the paper are:

- The discussion should be far more focussed on a few key messages.
- The interpretation of the glaciochemical data should rely much less on EOF analysis, and more on known geochemical behaviour and relationships of the various elements.
- The data presentation should be greatly simplified and presented as in Betler et al.
- Parts of the text should be modified to read less like a long description of EOF loadings, and more like an actual coherent interpretation of the main features in the data set.

Interactive comment on The Cryosphere Discuss., 5, 885, 2011.