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Discussions

Interactive comment on "Fluxes of microbes, organic aerosols, dust, and methanesulfonate onto Greenland and Antarctic ice" *by* P. B. Price et al.

Anonymous Referee #2

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This paper was something of a head-scratcher to review. Rather than attempt to reconcile my impressions, I will just list them.

1) I appreciate the novel observations made by this group. The spectroscopic probe they have developed is a fantastic way of characterizing variability of organic material/cells in ice cores. Getting this new data into the open literature is a high priority. The authors have launched a creative new line of research that will surely lead to many new biological and paleoenvironmental insights.

2) The major conclusion of the paper is that there is a higher concentration (and flux) of microbes in Greenland ice than in Antarctic ice, and a higher concentration of non-

cellular organic material in Greenland ice than in Antarctic ice. Furthermore, these differences between Greenland and Antarctica are similar to those for mineral dust. Ergo, the authors infer a terrestrial origin of the microbes and other organic material. Maybe so, but I am not so sure that logic is compelling. Why can't the organisms be of marine origin? At this point, it would seem reasonable to examine major ion data, like Na from seasalt, and Ca from terrestrial input. This leads to point #3.

3) Methanesulfonate is used as a proxy for marine input. Granted, it is ultimately a marine-derived signal. However, it is a complex one and the authors may not fully appreciate the range of processes involved in the incorporation of this signal into the ice. One thing that MSA should probably not be used for is as a universal tracer for marine air mass trajectories or for marine biological productivity. DMS production is species specific - a change in speciation from diatoms to Phaeocystis (say, due to a change in Si availability) could have a huge effect on DMS (and MSA), but none on production. There are also questions about revolatilization of MSA from ice that have been raised. While, I personally don't think that is a major issue for GISP2 or West Antarctica, it is still a question. I think seasalt Na or Mg would be a better choice for a marine tracer. I am not convinced that the MSA evidence demostrates what the authors say it does.

Finally, why not simply examine time series data in the cores they have analyzed to make this case? It would be more compelling if within individual ice cores, one could find a statistical correlation between microbes and non-seasalt Ca or microbes and seasalt Na (for example).

Bottom line - The authors may well have it right (ie. that the microbes are terrestrial) , but the marshalling of the evidence could use some more thought. I think ultimately, DNA analysis of some carefully selected samples will probably settle the matter.

In terms of style, I found the manuscript somewhat difficult to follow, particularly in terms of the methodology and data presentation. Some specific comments:

Abs. line 2. over what wavelength range?

Abs. line 7 "Antarctic Greenland" is missing something

Abs. lines 7,8 This is very unclear. Is the microbial cell ratio Trp+non-Trp? or is it just non-Trp?

Abs. line 11. "The lower fluxes of ...onto Antarctic ice". This is unclear. Lower than Greenland? OK, but the line about MSA should not be inserted between this sentence and what comes before. It is confusing. If this sentence has something to do with MSA then I do not understand the logic.

Abs. line 13. Haptophytes are probably not the only (or even the main) contributor to DMS emissions at the ice edge. I believe sea ice diatoms can also release DMS. The wording "we attribute" is probably inappropriate here anyway - it gives the impression that the paper contains some evidence as to the biological origins of MSA in terms of speciation, which it does not. Furthermore, they attribute higher fluxes to "higher concentrations" which may or not be true. Could be a higher conversion efficiency of DMSP to DMS, etc.

Abs line 17. Could be that the source regions overlap or that the transport patterns carrying them overlap.

Abs line 18. Not clear why the last sentence is in the abstract of this paper.

Intro line 4. The first appearance of MSA could probably use some further explanation, like CH3SO3H (methanesulfonate or MSA; an atmospheric oxidation product of dimethylsulfide), ...

Intro line 7 "Our group" is used repeatedly. This is a bit informal for the published literature and distracts from the flow.

Intro line 15. This sentence needs to be split. It is unclear what "their" refers to at the end.

BGD

5, S2768–S2772, 2009

Interactive Comment

Intro line 22. "One sees" The subject of this sentence should not be "One". It should be "Large fluctuations of microbe numbers are ..."The text needs some explanation here about what is in Figs 1 and 2. Is this Greenland or what? the text should be readable first, without having to read the figure caption yet. The figure captions themselves include abbreviations that are not defined prior to this point. The non-ice core reader will not know whator where "WAIS" is. With regard to defining acronyms, that needs to be in the body of the paper itself, not just the abstract.

Results line 1 why is the wavelength interval important but the wavelength range not?

p. 4684 line 18. It is stated that the non-Trp fluorescence similar to marine organics. I imagine that it is equally similar to terrestrial soil-derived humics. Rohde et al 2008 seems to imply it could be either, but presents no evidence. Why does the text here emphasizes a marine origin?

p. 4686 line 16. Needs clarification as to why is it reasonable to use East Antarctic dust fluxes for West Antarctic sites.

p. 4687. "Swept up with similar efficiency" I have no idea what this means. How can one compare aerosol generation at the sea surface and desert surface with a sweeping comment like this?

p. 4687. line 12 "west" should be "West Antarctic"

p. 4687. line 23 "To minimize the role of climate in MSA production, which is still not settled"... Clearly, the author is not suggesting that their data treatment alters the influence of climate on MSA production. This needs to be reworded. Perhaps what is meant is "Because MSA production is strongly influenced by climate, we separately compare the flux ratios for the Holocene and for the Last Glacial Maximum." or something like that?

The paper to give an overly simplistic view of the origin of DMS (and hence, MSA). I do not think it is entirely clear to what extent the MSA signal in ice cores originates

BGD

5, S2768–S2772, 2009

Interactive Comment

with DMS emissions at the ice edge, as opposed to DMS emissions from open ocean phytoplankton blooms in open water.

Interactive comment on Biogeosciences Discuss., 5, 4681, 2008.

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5, S2768–S2772, 2009

Interactive Comment