

Interactive comment on “The role of the global cryosphere in the fate of organic contaminants” by A. M. Grannas et al.

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Overall this is an interesting paper that addresses a crucial topic in a timely manner and it certainly should be published. Given current rapid changes in the cryosphere and the ever increasing awareness of the present and possible future impact of organic contaminants, having this review will be helpful both to the relevant community but also to other communities for which arctic contaminants is a side problem.

However, the current version illustrates the fact that it is very difficult for a large group of authors to write a homogeneous review accessible and usable beyond its community of origin. Some sections are well written and clear, with a vocabulary and concepts that will be grasped by most readers, while other sections are just a list of vague general considerations that will not interest many. Some aspects that should have been written

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in section n-1 to understand section n are missing. In general, a section should have general considerations, followed by specific and simple examples supporting these general statements, and backed up by a figure or table. Overall, the number of figures is clearly insufficient, as many points would have been a lot clearer with graphic support. Furthermore, most figures are of poor to terrible quality, lack clarity, some of them totally, and the captions are almost always inadequate and insufficient. The main point of a figure should be understandable without reading the text. Figure 5 is just unacceptable and only serves to irritate the reviewer, which is never a good idea. Although I have an overall good opinion of this paper, I urge the authors to take the time it requires to improve its appeal, internal consistency and clarity. I offer a few suggestions below.

1- How about a list of abbreviations at the start of the paper? Sure, we know what PCBs are, but some of the acronyms are much less common and are not even explained. How about LRAT: will all potential readers understand that?

2- I recommend discussing the possible location of contaminants in snow and sea ice at the beginning of the paper to make this point clear from the start. Partitioning between ice crystal surfaces and organic particles, solubility in brine in sea ice, etc; all this needs to be clarified at the very start.

3- P. 927, l. 6-8. Perhaps detail a bit more the feedbacks mentioned. Referring to AMAP does not suffice. One must be able to understand a paper without looking up references.

4- P. 931, l. 10 ff. How about ice nuclei that also contain impurities?

5- Section 3.1. Processes taking place during the atmospheric phase of snow crystals should be clearly separated from those affecting deposited snow. It is not always clear what is being discussed. This needs to be better coordinated with section 3.3. Comparing scavenging by snow and rain needs to discuss the impact of the processes involved: adsorption (snow), dissolution (rain) and scavenging of particles. If solubility and adsorptivity are not discussed, how can the end result be discussed sensibly?

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6- P. 933, l. 17 ff. Discussing the effect of increased precipitation without discussing temperature effect is incomplete, especially since both effects are linked. 7- P.935, Re. volatilization from snow. Perhaps consider discussing the paper of Jaffrezo et al. (1994), AE 28, 1139. This discusses long term decreases in contaminants in multi-year snow.

8- P. 936, l. 11. Are organic particles not a phase involved here? l. 19: why the factor 100? l. 26. That MTC factor will surprise micrometeorologists. Is there any connection with air turbulence?

9- P. 938. Last paragraph of section 3.3. Regarding PCB 28, Taillandier et al. (2006) had already concluded that subarctic snowpacks would take up much less of this compound than an arctic snowpack. Their conclusion was that in the subarctic, 71% of the PCB would be in the boundary layer whereas in the Arctic, only 6% would be in that atmospheric layer. The connection between the last sentence of the paragraph and the rest is not clear.

10- Section 3.4.1. only includes vague general statements of little interest. It must be completely rewritten, and must include specific examples and a figure or table.

11- Section 3.4.2. Perhaps Table 1 should be discussed half way down the section.

12- Section 3.4.3. needs at least one figure with actual experimental curves. Table 1 is fine, but it should have been discussed earlier, and here we expect some hard data, not just theoretical considerations.

13- P; 945, top. How can type 5 of Table 1 not be explained and still be reproduced well by a snowpack melt model?

14- Section 3.5. Fine section, clearly written, with a clear and totally adequate figure. If all sections were like this one, clear, concise, with adequate graphical support, the paper would take a quantum leap in quality. Watch for the size of the figures, however, and "glacier movement" is ambiguous. You mean the position of the front of the glacier,

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I guess.

15- Section 3.6 is good and interesting, but lacks clarity and many readers will miss several aspects. Should we know why different reservoirs have different enantiomeric ratios? Fig 3 is OK, but another one illustrating increases and decreases (both with long term trends kept and removed) of HCB would probably have been more useful. Is the purpose of the X axis of Figure 4 to confuse the reader? Having just years was too simple? This section illustrates difficulties in coordinating the paper. One would have liked to be explained that POPs partition to the ice surface and to organic particles, so that overall snow composition will affect contaminant location and reactivity. How do we know the actual location of contaminants (top of p. 953)? I recommend dividing this section into snow and ice subsections. Lastly, I suggest showing a table of saturating vapor pressures of POPs at -15°C and referring to it to explain POP behavior.

16- Section 3.7 is vague and just general. We need actual examples with hard data and if possible a figure.

17- Section 3.9. Arguably, this is a difficult section to write. It is easy to guess who wrote it but I recommend that another coauthor critically evaluates it. All the experiments discussed here are good and interesting, but have severe limitations, as acknowledged by the authors. The impact of those limitations on our actual understanding of environmental processes must be stressed more. What is the impact of using ice mimics? What is the impact of the mode of formation of the ice on the location, and hence reactivity, of contaminants? Sure, there are fine papers on the suitability of ice mimics, but then are the arguments used still fully convincing? How could the presence of organic particles in actual snow affect reactivity? Can we extrapolate to natural snow the concentration effect and the presence of photoinitiators in lab experiments? By the way, snow is porous, snow crystals are not. It would be OK to conclude that today, we are far from understanding processes in actual snow. For example, have we tried to detect products such as those of Figure 8 in actual snow? There needs to be a better coordination between section 3.9 and section 4.1.

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18- Section 4.2. This is a bit thin. For example, do not we need to know more about the interactions of POPs with organic particles ?

In conclusion, this is a potentially very good paper, but some sections definitely need improvements, sometimes complete rewriting, and the coordination between sections needs to be improved with great care and much attention to detail.

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