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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "800 year ice-core record of nitrogen deposition in Svalbard linked to ocean productivity and biogenic emissions" *by* I. A. Wendl et al.

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This paper presents ionic chemistry data covering 800 years from an ice core in Svalbard. This is a good time period to study as it allows recent anthropogenic changes to be assessed in the light of a long period that was at least not influenced by industrial emissions. Svalbard has an interesting location, within the Arctic but influenced by different air masses compared to the more-studied Greenland records. The paper shows some intriguing trends and correlations, and will certainly become publishable. It does require some further work, mainly in two areas: firstly there are some general points that need drawing out a bit more, and secondly the authors should be a little

more precise in some of their statements about what their data show (which will lead to greater caution in the conclusions).

There are two worrying general aspects of this study. The first concerns the issue of melt in the core. The high amount of melt in Svalbard cores has long been a concern, with the potential to disrupt and confuse records. I would like first to consider the issue of how much melt does occur in the ice. According to Fig S3 in the supplement and the middle panel of Fig 3, the annual melt percent is up to 1%, and when I saw this I thought the authors had been lucky and might not have a problem. However I then looked at data from Kekonen et al for the previous core near this location and found typical melt percents of more than 50%. This leads me to suspect that Fig S3 actually plots melt proportion (ie values not of 1% but of up to 100%). This should be corrected, and is such an important melt proportion that it needs much more discussion.

Given this very high amount of melt, I don't feel the authors can be entirely confident in dismissing the role melt could have played in the profiles they observe. They need to discuss it more. Firstly, the paper needs to present the temperature context of the core: what is the mean annual temperature and the seasonal range? What is the profile of temperature in the ice itself (i.e. is this a temperate glacier, important for knowing whether melt is purely a surface phenomenon, or whether water is also present and moving at depth)? Really the only evidence given here is the reference to previous papers suggesting movement by only 2-8 annual layers, which would justify trusting decadal values – but the authors really need to expand on this, and indicate whether their data can be used to support that previous inference. The observation of low correlations between melt percent and concentration does not seem to me to be evidence that melt is not important: it is by no means obvious why you would expect a correlation. As an example, if melt occurs in a layer you may expect some (but not all) ions to move downwards out of the layer, but that doesn't allow you to predict a low concentration in the layer because you don't know what is being transported into the layer from above. It would be surprising if the eventual balance of ions in and out should depend on the amount of melt in just the single layer.

On the issue of melt therefore I suspect there will be no proof that it has not affected the profiles significantly, but it does need to be discussed more and left on the table as a concern.

A second issue concerns the existence of a second set of data from a core of the same length from nearby (Lomo97). In Fig 3, the Lomo97 (grey) lines look very different from the new data, even after a long averaging, and especially for NH4+ and Na. Especially for NH4+ (compare grey and green in top panel), one's conclusion about anthropogenic versus natural variability would be quite different from Lomo97 than from Lomo09. The authors cannot therefore avoid commenting on the comparison. Is the difference due to analytical issues or is there really enough spatial variability to explain such different concentrations and variability (rendering conclusions less robust)?

I now discuss a range of more detailed issues that occur in the text:

Page 24674, line 14: I am not sure that the shape of the trends alone is sufficient to define the source region for Svalbard. There surely must be data about where air masses to Svalbard originate that would more usefully define the source region?

Page 24676, line 15-18. For MSA-sea ice correlations in the Antarctic, I am surprised you don't cite papers by Curran et al or Abram et al.

Section 3.1. The order in which this is written is a little strange. You start with the nitrate-MSA correlation, the jump over to MSA-sea ice correlations, and then jump back (page 24678) to nitrate. I think this could be re-ordered in a way that makes it easier to follow.

Section 3.1. The idea you are presenting is that MSA is controlled partly by winter sea ice and partly by nitrate fertilisation. This is intriguing, but I struggled to see how you thought the two influences interact, and I think you overstate your case on both counts:

*The correlation between MSA and nitrate looks interesting, but breaks down com-

pletely between 1300 and 1400. This should be acknowledged.

*The relationship between MSA and sea ice is then tricky to assess in isolation: if you are suggesting that the main features of MSA are explained by nitrate until 1900, then it is only the residual (after accounting for that) which you would expect to correlate with sea ice. I'd have to say that, apart from the period from 1900, I don't really see much correlation.

*The idea seems to then be that low ice extent after 1900 draws MSA away from its link to nitrate. In fact you need a really strong effect as the extra (industrial nitrate) should be fertilising the ocean strongly, increasing MSA by your hypothesis, but instead MSA drops way below its long term mean. In contradiction to that idea, ice extent is quite low from 1500-1600, with no apparent effect on MSA.

Taken together I think your story is not quite straight, and needs to be presented in a less definite way.

Page 24679, line 12. Although I don't think nitrate is of marine origin, your correlations show only that nitrate does not derive mainly from sea spray. After all, we all agree MSA is of marine origin, but that also has a very weak correlation with sodium. Therefore your statement in line 14 "not the ocean" is a bit too broad.

Page 24680, section 3.2. I already pointed out that the two Lomo cores have very different patterns. In line 26, you state that the Holte05 core shows the same increasing trend as Lomo09: however in that case you need also to point out the strongly different patterns in the 1700-1800 period.

Page 24681, line 20. While Lomo and Belukha ammonium are similar in the 20th century they appear uncorrelated before that (what is the correlation before 1900?) I don't feel you can just ignore that and claim that the same source controls both of them. It looks more as if they may see the same industrial source, but a different pre-industrial source (or at least a different influence on transport from the source), doesn't it (as also for nitrate)?

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 24667, 2014.