**Atmospheric
Chemistry
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Discussions**

Interactive
Comment

Interactive comment on "Reassessment of the factors controlling temporal profiles of nitrate in polar ice cores using evidence from snow and atmospheric measurements" *by* E. W. Wolff et al.

Anonymous Referee #2

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General comments

I found the manuscript pleasant to read. It is well organized and each section is well introduced. I have no major comments but only minor remarks detailed later on. However I found strange that the authors are still discussing the possibility of recording SPEs by the nitrate peaks. With now the well-established strong post depositional effect taking place in low accumulation sites and the well-known affinity of nitric acid for alkaline species, only a small and minor group of researchers are still thinking that SPEs are clearly visible in snow nitrate. The ice core and atmospheric communities have no more doubt about this question. May be the recent publications in solar physic journals

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attesting once again the present of SPE (eg McCraken et al., 2004) in nitrate snow deserve once again a counter publication. Thus this brings me to a little criticism of this manuscript that might be actually unfair as I'm so convinced by what the authors say. I did not find really new ideas in this manuscript: SPE, alkaline aerosol effects, source emission by the snow and possible PSC primary source are not really new topics in this field.

Specific comments

Page 11041, line 29: Mayewski and Legrand, 1990 should not be cited as these authors exposed a wrong explanation for the nitrate profile. As a rule, this limits the propagation of faulty manuscript.

P11042, around line 15: By reading this section I had the feeling that there was only one way to interpret ice core data. Actually, the interpretation of ice core data will highly depend on the environmental context of the drilling site. Some ice core profiles (eg vostok?) may actually represent only surface effects. I can only encourage the authors to rephrase that section, clearly stating that nitrate ice core concentration will be interpreted differently from different sites. It is crucial to convey such idea.

Same page, end of page: Well may be at the BAS, I think ITASE was a strong effort by the community to document among other things the variability of nitrate

P11043, L18: Why describing the denuder collection system when no data are presented.

P11044, L20: I wonder if the first event in figure 1 is a good example to show the discrepancies btw SPE and nitrate peak. There is apparently 10 days btw the two events. Can the transport of SPE from the high atmosphere to ground explain this gap?

P11045, L19: Getting the right concentration of surface snow can be a difficult task because of the sharp decrease of concentration in low accumulation sites. Collecting

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the first few mn or 1 cm can give concentrations with an order of magnitude difference. Halley is a high accumulation site but still a small snow pit would have been better. This remark applies also for all the profiles shown in this publication. Can the author give more details of how the snow was collected? If the summer peak should be insensitive to collection bias, part of the variability may be due to the way snow was collected

P11047 L2: Sentence issue.

P11047 L7: Gas phase nitrate can dominate particle phase even a ground level. If high concentration of NaCl is present, the partition btw gas and particle should show this mass transfer even at ground level. It is frustrating to not see the denuder data. I will rather suggest the authors to show the denuder data they have or to remove completely the reference to those measurements, may be just mentioning at the beginning of section 2 that denuder sampling fell.

Section 3.3: I wonder if this section cannot be merged with 3.1

P11051, L13: Reference to Blunier experiment. I will advise the author to be caution about Blunier experiment. Using NaNO3 to dope the artificial snow may not be the best way to mimic Mother Nature, see for instance the discussion by Davis et al, 2008, Atmos Env special issue ANTCI. Photolysis may be well dominated the loss process

P11052 first lines: As I said before, in most inner part of Antarctica, nitrate profile may actually become a proxy of meteorological factor more than representing a change in NOy production, thus changing completely our view of this proxy record and this may not be bad actually. If nitrate is strongly impacted by meteorological conditions as it seems, I cast doubt that someone could one day interpret concentration profile in term of NOy source.

P11052, L13: Again caution should prevail here. Blunier, Anastasio Chu and others abundantly shown that even in the form of a salt, nitrate is photolysed in snow. So apparently nitrate in the form of a salt is not an absolute protection against photodisso-

ciation. With an accumulation rate half of present, who knows what the outcome have been during the glacial ages;

P11053, L5: mass distribution with depth in central part of Antarctica may be different that the one exposed by the authors. See again Davis et al., 2008. A very tiny layer at the surface may indeed carry most of the mass of nitrate so that mass calculation for the entire continent given here might be bias.

Typo: P11053 L22 and in bibiography: Subscript 3 for nitrate and x or y for NOx and NOy. Please check and correct.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 11039, 2008.