Atmos. Chem. Phys. Discuss., 8, S5227–S5229, 2008
www.atmos-chem-phys-discuss.net/8/S5227/2008/

Atmospheric Chemistry and Physics Discussions

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 #1

Received and published: 23 July 2008

This manuscript revisits several long-standing issues in glaciochemistry. On the larger scale, the first author is taking up an issue he personally raised to prominence quite some time ago, i.e., "What does nitrate in an ice core tell us about nitrogen oxide chemistry in the atmosphere?" (Wolff, 1995). The more focused question of whether time series of nitrate recovered from glacial ice record SPE has an even longer history, but would seem to have been settled (short answer, no) for well over a decade.

The answer to the larger question is still open, and remains an active area of research. One could argue that this manuscript adds little to the debate beyond the summary in Grannas et al. (2007), except for the effort to try to expand from individual focused

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

studies on air/snow exchange processes by looking at the longer timescales covered by glaciochemical records, and trying to take an Antarctic wide view in later sections.

Overall, I have mixed feelings about this manuscript. I could do without the SPE discussion entirely, though it does allow the case for local (chemical) control on deposition/preservation of nitrate at one coastal Antarctic site to be clearly made. The bottom line message that interpretation of any single ice core nitrate record in terms of atmospheric chemistry is perilous can stand reiterating, but I am not convinced it needs quite so many words as are used in the present case. The manuscript is long, largely due to what I am tempted to call a tutorial tone.

Because I endorse the message, I would like to see this paper published. But, because the ground is reasonably well trodden, I would like to see it shorter by at least 1/3 and possibly as much as 1/2.

The authors should consider whether the CHABLIS data really need so much attention, if the primary focus really has the grand scale outlined in the title. (Note, one might quibble that "polar" really should be "Antarctic" in the title since Greenland is only mentioned in passing once or twice.) The new data from Halley has been recently published, and, as they point out, has only limited relevance to the vast interior of Antarctica. Granted, the contrast between plateau and coastal sites, and the apparent connection between them through drainage flow advecting nitrogen oxides from the interior toward, or even across, the coast are now recognized as central to the Antarctic nitrate puzzle, but this understanding did not arise solely through the CHABLIS year of sampling at Halley. Perhaps my point is that I feel the "bigger picture" aspects of the manuscript are the more important points, and the detail about sea-salt and nitrate at Halley is more distracting than edifying (especially if we accept that SPE source of nitrate spikes is a dead issue). Related point is that strong uptake of nitric acid onto sea-salt is well established among the atmospheric chemistry community, so it seems odd to spend so much time "discovering" that this process also operates in coastal Antarctica.

ACPD

8, S5227–S5229, 2008

Interactive Comment

Perhaps a semantic quibble, but I am not sure the goal of understanding nitrate in ice cores really centers on "NOy production" over Antarctica. In the historic debate about SPE attention of the "debunkers" was focused on long-range transport from lower latitudes (perhaps largely driven by tropical lightning producing NOx) in addition to downward mixing from the stratosphere. Likewise, in the Greenland example cited as a rare case where a "robust" conclusion linking glacial nitrate to sources could be drawn, hemispheric scale transport of pollution drove the observed trend. Thus it seems that the motivation to understand nitrate in ice is in order to constrain the abundance and cycling of nitrogen oxides in the paleoatmosphere on relatively large spatial scales, and to sort out the relative contributions from multiple sources. Perhaps the shorthand of "NOy production" captures this, but it is misleading to restrict the spatial scale to just the polar ice sheet itself. Maybe "nitrogen oxide chemistry" could be defined to stand in instead.

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

ACPD

8, S5227–S5229, 2008

Interactive Comment