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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Pronounced signature of arctic surface ozone depletion events after polar sunrise on Δ^{17}O in atmospheric nitrate" *by* S. Morin et al.

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This interactive comments contains only the response to the specific comments by Referee 1. An extensive response to the main issues raised by both referees is available as a separate Author comment.

1.The Referee 1 points out an apparent inconsistency in our conclusions. We derived a correlation between Δ17O(NO3-) and the ozone mixing ratio that could perhaps be used in nitrate embedded in ice-cores but sometimes this correlation may be reversed due to an explosion in the BrO mixing ratio. It must be noted here that ODEs do not normally occur in the vicinity of ice-coring sites, so that the bromine explosion mechanism is most likely not recorded in any deep ice core. In addition, we have

removed the reference to these unpublished data, since it would require a lengthy paragraph to describe clearly these newer observations. This will be the subject of a forthcoming article.

3.We accept with both Referees that the words “best” and “unambiguous” are somewhat a bit strong, regarding our description of the non-linear definition for Δ17O. Also, it is true that ?’ is not defined for atmospheric reactions in general. We decided to choose the simplest Δ17O definition in this article, so that Δ17O=δ17O - 0.52δ18O . This definition makes mass balance calculations easier (Kaiser et al. 2004) and is also easy to derive from existing δ17O and δ18O measurements. Furthermore, the offset resulting in a change between two Δ17O definitions is lower than 1 ^{o}/oo , which lies within our estimated uncertainty (see below).

8. The derivation of the mass-balance equation describing the dependency of Δ17O(NO3-) upon several variables was simplified in the revised version of the manuscript. In addition, we introduced the chemical lifetime of NO, which simplifies the equations but also is relevant to the discussion with regards to the oxidative power of the atmosphere (see point 8).

9.The whole paragraph dealing with OH isotopic equilibration was rewritten (see above), and the suggestion of the Referee 1 in terms of structure was taken into account.

12.Our definition of “theory” is not as exclusive as Referee’s 1 one. However, we agree that in this context it could be replaced by “modeling” or “calculations” or “framework” without affecting the overall meaning of the sentences. This was changed accordingly.

13. This interesting comment by the Referee 1 refers to the difficult question of the exact definition of the oxidative power of the atmosphere (OPA). Within this paper, we use the lifetime of NO as an indicator of the OPA (the shorter-lived NO is, the higher the OPA is), since most NO oxidants are traditionally included in the oxidants that make up the OPA (ozone, HOx and halogen oxides). In this framework, it is clear that

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Δ17O(NO3-) is also a direct indicator of the OPA (from the mathematical derivation). It is also clear, from this point of view, that BrO does not significantly replace ozone at lower ozone mixing ratio, since the correlation is not reversed.

14. Since this figure (fig 6.) was removed from the revised version, the caption was also suppressed.

16. We agree with Referee 1that some heterogeneous processes may possess a mass-independent component, hence affecting the Δ17O values. However, we assume this to be negligible in the case of the condensation of nitric acid on acidic droplets, given the considerable lack of knowledge regarding this emerging topic of research. In addition, the good correlation between Δ17O and δ18O (data not shown) indicates that mass-dependent mechanisms only play a minor role in this system (although two mass-independent mechanisms could also compensate each other, which is likely not to be the case).

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 6255, 2006.