

Thank you to the author for being responsive to many of the comments provided on the original manuscript. I have only a couple comments that should be addressed in finalizing the manuscript.

1. The authors provided the response shown below to a comment in the initial review, however, the changes they claim to have made - to add discussion about coarse nitrate in non-coastal environments - were not actually included in the tracked comments version of the revised manuscript. The text in this section on p. 11 of the revised manuscript remains the same as that in the original manuscript. No change was actually made. I assume this is just an oversight in preparing the revised manuscript.

Original review comment 5. p. 11, lines 8-17: The authors here focus discussion on prior work concerning coarse particle uptake of HNO₃ in coastal environments. This is interesting and relevant, but they should also cite observations of uptake in more continental environments which might be better models for the SLV. Lee et al. (doi:10.1016/j.atmosenv.2007.05.016), for example, examine the importance of coarse particle nitrate at both interior and coastal U.S. environments.

Author response. We have incorporated the suggested reference into the text commenting on the few studies that have speciated coarse nitrate. Lee et al. identify that coarse mode nitrate particles, formed from acid displacement, were more important in national parks areas in Arizona and Tennessee. Measurements at both sites were during Spring and Summer, respectively, so did not have competing NH₄NO₃, but they do highlight the fact coarse particle nitrate extend into the PM_{2.5} size regime and not all nitrate in this regime is associated with NH₄.

2. I appreciate that the authors have now somewhat caveated the use of the snowpack nitrate:ammonium ratio as a proxy in estimating how much nitrate in the atmosphere was present in coarse particles. I do not see convincing evidence, however, to support their claim that “this overestimate is within reason.” Nor is a reader likely to understand what is meant by a “within reason” error estimate.

In the original review I point out that the snowpack composition nitrate:ammonium ratio will be biased high by the greater scavenging efficiency during precipitation and the higher dry deposition velocity to snow after precipitation of coarse nitrate particles vs. fine ammonium particles. The authors’ responses that

- (a) scavenging efficiency differences are not important if scavenging is complete ignores the fact that scavenging is generally not complete for fine particles and we, in any case, have no information about the extent of scavenging in the snowfall episodes studied
- (b) coarse particle dry deposition may not be much faster than gas species dry deposition adds even further uncertainty about the relationship between snowpack composition and airborne nitrate:ammonium ratios since one has coarse and fine particles and gaseous nitric acid and ammonia all dry depositing to the snowpack at differing rates that depend on widely different deposition velocities and concentrations.

I ask that the authors provide the reader with a more complete discussion of these significant limitations to their use of snowpack composition when they provide a quantitative estimate of the potential increase of NH_4NO_3 in the absence of coarse particles.