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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global distributions and trends of atmospheric ammonia (NH_3) from IASI satellite observations" *by* M. Van Damme et al.

Anonymous Referee #2

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1 Overview

2 Comments and questions

The manuscript by Van Damme et al., "Global distributions and trends of atmospheric ammonia (NH3) from IASI satellite observations," marks a significant step forward in mapping observations of NH_3 from space. This new dataset includes comprehensive spatial coverage with quantitative error estimation that will be a valuable resource for reducing uncertainties in our understanding of the sources and fate of reactive nitrogen. The only aspect that is sorely missing is some discussion of the vertical sensitivity of

the retrieval, which is critical for validation of the IASI NH_3 product and application of this product for constraining model simulations. Overall the manuscript is fairly clear and well organized; it will be suitable for publication in ACP following consideration of this point in addition to the comments below.

- A significant motivating factor for this work is improving NH_3 emissions estimates and N_r model simulations. In order to compare the values presented here with models, it is necessary to know how the retrievals depend upon the vertical distribution of NH_3. Can the authors provide representative averaging kernels for their data, or at least discuss their findings in light of the sensitivity of the retrieval to NH_3 concentrations at different altitudes? Further, have values from IASI ever been directly compared to NH_3 observations, from air craft or in situ measurements? Could the authors provide the necessary information regarding vertical sensitivity of the retrievals to make such comparisons possible?
- Introduction: IASI NH_3 was also compared to GEOS-Chem in Kharol et al., ES&T, 2013, and TES NH_3 measurements were used to constrain emissions with GEOS-Chem in Zhu et al., JGR, 2013.
- 24307.9: It might be nice to include in Fig 1 a line indicating the region used by TES that is discussed here in the text.
- 24310.15 - 20: It's not clear why model profiles are scaled by this amount. Did the LUT start with only a single mean profile over each land-type based on GEOS-Chem? Were there not actually profiles in the GEOS-Chem simulations spanning the necessary range of concentrations to include in the LUT? If so, is this indicative of a shortcoming with the model simulations, either owing to underestimated emissions or to coarse model resolution?
- 24311.2: It wasn't clear to me why artificially enhanced thermal contrasts are necessary – wouldn't these be present in the data already if they were important?

I'm probably just missing something here, but maybe it could be explained a bit more in depth.

- 24312.6: It might be worth mentioning some typical column values here to put these numbers in context.
- The prominence of the biomass burning regions stemming from single-year events in the five-year mean is striking, and a bit odd. My expectation would be to see signals over persistent source regions (e.g., India) to be much larger than any singe-year event. Is this just an artifact of saturating the color scales at 3 $\times 10^{16}$, an order of magnitude smaller than the peak values? Could the authors indicate this somehow in the plots?
- Why is the outflow from West Africa so much more pronounced than other regions with much larger hotspots? Is this owing to the meteorology in the region or a lack (relative to more industrialized areas) of NO_x and SO_2 to react with NH_3?
- 24318.12: What is the nature of the source in southeast Calgary?
- The writing contains some awkward phrases (e.g., "emissions in the atmosphere" or "marked emissions") and grammatical errors (usage of commas). It would be nice if any of the native english speaking authors would be willing to spend an hour smoothing out the language prior to final publication.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 24301, 2013.