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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The global tropospheric ammonia distribution as seen in the 13 year AIRS measurement record" *by* J. X. Warner et al.

Anonymous Referee #3

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The authors of this paper describe the algorithm to derive ammonia from AIRS measurements using inversion theory pioneered by C. D. Rodgers, and present spatial and temporal (in seasonal sense) analysis of global ammonia distributions. This is important work and the paper should be published. The paper, however, could have been better written and data analyzed more thoroughly (e.g., just noting a particular phenomenon and merely speculating the cause is not enough). I have specific comments below that I would like the authors to address. I recommend the publication of the paper after these comments are addressed. The revisions that I am recommending are not major and should not take more than a month or two to implement. I highlight my concerns below: âĂć The paper needs editorial work. Many blatant errors and sentences with poor word choices are present in the paper and need to be resolved.

For example, on page 4, "R'Honi et al. (2013) discussed the exceptional emissions of NH3 and HCOOH in the 2010 Russian wildfires." Another example is the reference to 13-yr time period as long-term. It so happens that there is a 13-yr record of AIRS ammonia retrievals. However, that does not define what a long-term record is. Because the 13-yr record is close to a decade, that could be used in a general sense perhaps? âĂć Algorithm should be discussed in general terms instead of repeating material from Rodgers book/papers. The methodology should be conceptually explained for ammonia and also other trace gases that are generally simultaneously retrieved inverting hyper-spectral infrared radiances. âĂć Validation of the retrievals is only done for two weeks. I understand that not many ground measurements of ammonia exist but the DISCOVER-AQ field campaign data provided the authors with profiles covering 2-week time period. The comparisons are very encouraging. To explain the differences between retrieved and aircraft observed profiles for some cases, the authors revert to spatial variability of ammonia in a 45-km grid space (aircraft captures that variability but satellites can't resolve it). It would be nice if the authors can actually demonstrate the spatial variability of ammonia (from models or other ground observations) to explain the usability of AIRS ammonia product. Does this mean that AIRS ammonia retrievals at 45-km resolution (is it larger at scan edges?) are of no use to high resolution air quality models for forecasting applications? Are the retrievals more useful in regions where spatial variability is not that high? I think a discussion on these validation results from the application perspective will be very useful. Or perhaps the retrievals are only useful to document trends in ammonia and not for real time applications in models? âĂć I am also a little concerned that the authors have not compared global maps of ammonia from AIRS to other correlative measurements from other satellites (IASI for example). Although instrument and algorithm differences can exist, readers can look at the comparisons in a qualitative sense and decide for themselves if the product is useful for certain applications or not. Also, for validation results please provide information on the geographic location for each profiles to get a sense on where these observations taken (terrain, surface emissivity, etc.) âĂć The authors presumably are continuing

their collaboration with field campaign programs and therefore should recommend to the campaign that future experiments should focus doing multiple spirals within a short distance of each other in a 45-km box to understand sub-grid variability of ammonia. âĂć In Figure 4, the color bar is referred to as x-axis. âĂć There is a lot of discussion on the relevance of biomass burning and ammonia distribution observed in global maps. I think the authors are correct in drawing those correlations but would be nice to correlate with MODIS fire activity maps. Without corroboration from other sources of information, it becomes speculative at best. I say this because in Russia and Siberia, there seem to be elevated ammonia in all seasons and number of retrievals (days of data available) low. Could it be that there are some retrieval issues owing to the persistent snow on the ground? Again, this is why it is important to compare AIRS ammonia retrievals with other satellite retrievals to establish biases as a function of season and location. The 2-week time period is not enough to capture the dynamic range, seasonal, and regional variability seen in ammonia to validate the product. âĂć Minor comment: The few sentences dedicated to World Bank data on page 12 can become a footnote to avoid distracting the reader. âĂć Figure 9 is cited twice. Should there be a Figure 10? âĂć In global season maps, African biomass burning appears to show up prominently in MAM season. However this March peak is absent in Figure 9 for SH. Is it masked by the global averaging? Should this analysis be stratified into crop lands, forests etc. to separate agricultural burning vs. wildfires? I think this stratification will gel nicely with the way the results are presented in Figure 6. In summary, this is important work that should be published after addressing the comments above.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 35823, 2015.