

Interactive comment on “Satellite evidence of substantial rain-induced soil emissions of ammonia across the Sahel” by Jonathan E. Hickman et al.

Anonymous Referee #2

Received and published: 6 August 2018

I have read the comments from the first reviewer and agree that this is a clear, well written and very interesting paper. I also agree that the offset between the NH₃ and NO₂ peaks should be discussed in slightly more depth. What does the INDAAF NO₂ data show?

I do think some clarification from the authors would be helpful on the following issues:

Page 7, line 7: please add a sentence of two on the regridding technique used.

Page 7, line 11: what is the IASI detection limit in the Sahel?

Page 8, line 24: it appears that a uniform profile is assumed here. Is this really a good

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assumption?

Page 11, line 11: I don't see the increase in fire emissions in late March

Page 12, line 25: It could be helpful to rewrite this as: " except during the biomass burning season"

Page 13, line 23: at what scale where these correlations calculated: 0.25 deg or over the entire box?

Page 15, line 22: possibly repeat that IASI NH₃ appears to be biased low

Page 17, line 17: only NO₂ concentrations become comparable to NH₃, not the fluxes

Page 18, line 8: a more sophisticated inverse modeling of the emissions? This should be made clearer, and should suggest an approach

Page 18, line 10: the increased cloud cover during the rainy season could certainly mask increased NH₃. Cloud cover would also impact OMI NO₂, but possibly not to the same degree. If at all possible, the authors should discuss this.

Minor comments:

Page 4, line 27: the second sentence does not follow from the first

Page 7, line 7: IASI data are only used if the pixels are 75% cloud-free, while OMI data is included only if the cloud-cover is less than 30%. While these statements are almost equivalent, it would be clearer to say 75% cloud-free and 70% cloud-free, or cloud cover less than 25% and cloud cover less than 30%.

Page 9, line 25: "burned area and fuel consumption in savannas are"

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-550>, 2018.

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