November 6, 2018

Dear Editor:

Thank you for accepting our manuscript, pending technical revisions. We have included those revisions in the uploaded documents, including revised versions of the figures to meet ACP formatting guidelines. We also now include Author Contribution and Competing Interest statements, and have reformatted the text to meet guidelines.

And just wanted to say a quick thank you to the co-editor and both reviewers, who all certainly helped to make this a much better paper.

Yours sincerely,

Jonathan Hickman

Response to reviewer and co-editor (responses in blue):

Reviewer comments:

The authors have address my concerns in the revised manuscript. I recommend that the authors carefully go through the manuscript as they seem to have left some of their own comments in there. Here are some revision suggestions:  
  
- page 7 line 9 - add a reference instead of "is there a reference to this statement"

Thank you; apologies for the oversight. Reference added (Page 7, line 10)  
  
- page 9 line 17-20. "Our box modelled NO2 emissions used only satellite  
observations for grid cells where NH3 observations were also present; model results restricted to using satellite observations for grid cells where both NH3 and NO2 are presented in the supplemental information." This statement does not make any sense. In both cases the model seem to only use grid cells where both were present. From reading the figure captions for figures 6 and S3 the figure captions look almost identical, so I am confused as to what the difference between the two is.

There is only one difference here: whether the NH3 observations used for the model are screened based on the presence of NO2 observations. We thought that given the reviewers’ interest in the comparisons between NH3 and NO2 dynamics, there may be value in providing modelled NO2 and NH3 emissions based on observations of the same underlying grid cells We see how our language was confusing here, and have clarified in revision as follows (Page 9, lines 17-21):

“Our box modelled NO2 emissions used only satellite observations for grid cells where NH3 observations were also present, whereas the box modelled NH3 emissions used all available NH3 observations. Modelled NH3 results restricted to using satellite observations for grid cells where NO2 observations were also present are included in the supplemental information, to allow for more direct comparisons between modelled NO2 and modelled NH3 emissions.”   
  
- last line in acknowledgements should be removed.

We thank the editor and prefer to retain the last line in the acknowledgements.

Co-Editor comments:

In the main text of the paper, I believe that the estimated surface emissions of NH3 and NOx are calculated in units of mg N m-2 d-1, but in the abstract and conclusions sections, it’s not clear whether it is the mass of N or the mass of the molecule that is being reported. Please clarify.

Thanks for catching this, as it has helped us find an error. In revision, where we used an updated mask to mask the NO2 data, we inadvertently presented data that was in units of mg NH3 or mg NO2 instead of mg NH3-N or mg NO2-N. Here we include an updated figure with the correct data presented (and so there is very little change from the original figure 6 as a result of using the revised mask). We have also revised Figure S3 to match. And we have revised the end of the abstract as follows (page 2, lines 7-10):

“Box model results suggest that pulses occurring over a 35-day period in March and April are responsible for roughly one fifth of annual emissions of NH3-N from the Sahel. We conclude that precipitation early in the rainy season is responsible for substantial NH3 emissions in the Sahel, likely representing the largest instantaneous fluxes of gas-phase N from the region during the year.”

We have also revised the conclusions at Page 21 lines 22-24:

“Using a simple box model, we estimate that average emissions for the entire Sahel are between 2 and 6 mg NH3-N m-2 day-1 during peaks of the observed pulses, though note that these estimates are subject to substantial bias and uncertainty.”  
  
Page 2, Line 13 - should be 'particulate' not 'particular'

Thanks; fixed.

Page 4, Lines 25-28 - the structure of this sentence is difficult to follow

We have revised as follows (Page 4, lines 27- Page 5 line 2): “The Sahel is characterized by lower levels of fertilizer inputs (FAO, accessed 2018) and smaller loads of atmospheric N deposition than other parts of the world (Dentener *et al*., 2006; Galy-Lacaux and Delon, 2014; Laouali *et al*., 2012; though deposition can be elevated at the Sahel’s southern boundary).”

Page 13, Line 23 – strictly speaking, elevated ‘concentrations’ are observed rather than elevated ‘emissions’

Thanks—fixed.

Page 13, Line 25 – I suggest changing the title of section 3.2.3 to ‘Co-emission of NH3 and NOx’ since it is unlikely that the NO2 is directly emitted

Thanks—changed.

We have also made a change to the SI, providing the correct figure S4.

We have made an additional change to the text: text was moved from one position in the manuscript to another, to avoid repeating the same point. Some small changes were made to the text as a result. We moved text from page 21, lines 1-11, to Page 12. The new text reads (Page 12, line 19 to Page 13 line 2); note that the original text from Page 12 is in blue bold, the original text from page 21 is in blue italics, and new text is in red:

“**Additionally, we would not necessarily expect a linear relationship between the quantity of soil moisture and the quantity of NH3 emitted**—the emission response is a function of *complex processes which vary over time. Multiple studies have shown that rewetting of dry soils results in lower emission pulses for the same level of water addition (Davidson, 1992; Davidson et al., 1991), including of NH3 emissions (Soper et al., 2016). It also seems plausible that there are threshold effects in which an initial increase in soil moisture may simply need to be large enough to activate dormant microbial communities and/or cause a flush of labile microbial N to trigger an emissions pulse, such that the pulse response might best be described with a piecewise function. Asynchrony between plant and microbial activity during soil wet up and dry down (Collins et al., 2008), and different activation thresholds for microbial and plant responses to precipitation (Dijkstra et al., 2012) may also play a role in determining the amount of available NH4+ for volatilization at different times in the early rainy season.”*

The original text on page 12 (lines 18-19) read as follows:

Additionally, we would not necessarily expect a linear relationship between the quantity of soil moisture and the quantity of NH3 emitted.

And the original text on page 21 read as follows (text subsequently deleted is in red):

“Although the explanatory power of the linear relationship between soil moisture and emissions in the early part of the rainy season is relatively low, this would be expected in part due to the complex processes which vary over time. Multiple studies have shown that rewetting of dry soils results in lower emission pulses for the same level of water addition (Davidson, 1992; Davidson *et al*., 1991), including NH3 emissions (Soper *et al*., 2016). It also seems plausible that there are threshold effects in which an initial increase in soil moisture may simply need to be large enough to activate dormant microbial communities and/or cause a flush of labile microbial N to trigger an emissions pulse, such that the pulse response might best be described with a piecewise function. Asynchrony between plant and microbial activity during soil wet up and dry down (Collins *et al*., 2008), and different activation thresholds for microbial and plant responses to precipitation (Dijkstra *et al*., 2012) may also play a role in determining the amount of available NH4+ for volatilization at different times in the early rainy season."