

# ***Interactive comment on “Temporal and Spatial Variability of Ammonia in Urban and Agricultural Regions of Northern Colorado, United States” by Yi Li et al.***

## **Anonymous Referee #1**

Received and published: 23 December 2016

The manuscript by Li et al. reports measurements of NH<sub>3</sub> concentrations in northeastern Colorado, and compares them to remote sensing retrievals and model estimates. The combination of measurements, including surface and tower, with the satellite and model analysis is fairly unique, and is quite valuable for learning more about the accuracy of each. The paper is generally clear and well written, although there are some parts where more references could be provided and the introduction could use more content regarding remote sensing and modeling, given their importance to the value of this work. My comments below touch on these points, as well as a few others, which constitute minor revisions.

Comments:

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55-56: Please split up these references so that they are associated with the specific impacts being discussed, rather than all placed at the end of the sentence such that it's not clear which paper is about which impact.

70-72: That seems rather obvious. What is the contribution of NH<sub>3</sub>, relative to oxidized sources, to Nr deposition more broadly, not just near farms?

74-75: Which of these references are government regulators? I don't think any of them. You might instead specifically cite voluntary NH<sub>3</sub> control programs in the US, or actual government control programs in other parts of the world.

85-88: Please provide references for the RoMANS studies. Also, several other studies also examined source of Nr in RMNP, such as: Benedict et al., AE, 2013; Malm et al., JAWM, 2013; Thompson et al., JGR, 2015; Lee et al., ACP, 2016.

111 -115: Can some background be provided on comparison of remote sensing of NH<sub>3</sub> to in situ measurements? Surely this isn't the first study to perform such a comparison, for IASI or other instruments; or if it is, this aspect should be more prominently featured. Additionally, has the vertical profile of NH<sub>3</sub> been studied before, with tower measurements, remote sensing, or aircraft-based instruments? I find some text on this later in the manuscript, but sees like for a major component of the work it should be included in the introduction.

111 - 115: Same goes for CAMx – what have previous studies found with regards to CAMx model estimates of NH<sub>3</sub>, or is this work the first study to compare CAMx estimates with in situ NH<sub>3</sub> measurements?

202: Can the authors briefly comment on how close these stations are to the NH<sub>3</sub> measurement sites?

284: Could the authors be more specific about the improvements? Not sure if I see the value of this statement otherwise.

296: Are the emission factors constant in time and season? How reasonable is this?

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311: It's interesting that this is the first place in the paper that non agricultural source of NH<sub>3</sub> are mentioned. Biomass burning then shows up on line 368. Should some background on these other types of sources be included in the introduction?

307: Specify over what time period the word "average" refers to here.

296: It appears the authors are generating their own emissions estimates through a combination of local livestock information from CDPHE with emissions factors from EPA. How different then are their estimates of livestock NH<sub>3</sub> emissions in this area compared to other NH<sub>3</sub> emission inventories assembled by research groups or government organizations?

313-316: The analysis here is a bit weak. The overall mean NH<sub>3</sub> at GC is 25% higher than at FC\_W and 17% higher than at LD. Why are these insignificant differences? Given the variances in the measurements, couldn't one calculate whether these differences are significant with a t-test? My guess is the difference is systematic, not random, given that the reported value at GC is the highest of these 3 in every sub-period proved in Table 2. From a physical perspective, also would there be a particular time and or season when one would expect fertilization of the golf lawn to have a big impact, and thus should analysis of the impact of this local source be sought within a narrower window? I guess after reading the rest of this paragraph, where differences in other locations are up to x15, I understand better why such small differences here may have been glossed over, but still. . .

324 - 341: I recognize that not much data is available for trend analysis, but for those sites where trends were estimated to be significant, the authors did little here to comment on the nature of these trends, which was a bit disappointing. For example, is the decrease in BH possibly related to any of the management practices mentioned later?

343: Can some reference be provided for these?

370: There was also a paper from the TES group on correlation of NH<sub>3</sub> and CO from

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biomass burning as observed by TES.

376: There have been aircraft measurements from CalNeX, discover AQ, and over Canadian oil sands. These are probably different in terms of the extent and resolution of the analysis of vertical distributions. Still, it seems some specific references to earlier work is warranted.

Fig 3: Why don't we see the impact of the High Park Fire in the time series of any other sites besides FC\_W? What do the authors posit is the explanation for the peaks in GY in 2011?

477: Provide some references.

478-480: Provide some references.

498-503: Please include a brief summary of the model performance.

Fig 8: The color scale here is a bit odd, and the gradation rather coarse, given that only one site exhibits concentrations in excess of 20 ug/m3. Could it be changed to show more of the variability at lower concentration levels?

523: Can this be explained a bit more? Did Battye 2016 find model concentrations much lower than the CAMx concentrations found here, or did they find observed NH3 concentrations much higher than those reported here? Did the two studies use similar emission inventories?

Fig 6 and 8: I realize that a quantitative comparison between the CAMx estimates and the IASI columns are not possible, given the lack of a an averaging kernel provided for the latter (so please don't try it); still, qualitative comparison might be useful. Even if the authors could remake Fig 8 to be on the same scale as Fig 6, and put them side-by-side, it would help show that some the features (regional max's) in the IASI data appear to correlate well with the locations of emissions and concentrations shown in Fig 8.

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## Editorial:

19-22: This is a very long first sentence; consider breaking it up into two or more smaller ones.

26: missing comma before 'with'

40: the phrase "regional performance of each" is a bit awkward and unclear abstract: seems a bit long, in general "can it be made a bit more concise?"

67: I'm not sure that "ag" is an appropriate abbreviation for agriculture in a manuscript text.

91: comma after 2010

115: missing comma

118: missing comma

325: remove extra comma

Fig 2 caption: clarify what is being regressed (concentration vs time, presumably)

367: It's still 2016 as I write this, so how are you citing a paper from 2017?

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Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-1008, 2016.

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