

## ***Interactive comment on “Modeling the Diurnal Variability of Agricultural Ammonia in Bakersfield, California during CalNex” by C. R. Lonsdale et al.***

### **Anonymous Referee #2**

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General comments: This study presents a study combining surface, aircraft and satellite measurements of NH<sub>3</sub> and NH<sub>4</sub> concentrations in the San Joaquin Valley with a model study using CMAQ. The approach taken enables the authors to identify lacks in knowledge in both model description and emission inventories. While this is a worthwhile effort, the analysis and discussion could be improved by more explicitly including a discussion section in which the possible explanations of mismatch between model and observation are listed, as well as an outlook section with possible improvements to the model or emission data. If these points are improved upon (I give a few suggestions below), this paper could really contribute to improving NH<sub>3</sub> modelling and to a better understanding of the sources of mismatch between model and measurements.

Specific comments: For readers not familiar with the SJV geography and the location of the Bakersfield site, providing a map of the region could be valuable.

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Title: Not everyone is familiar with CalNex. Adding 'campaign' (or otherwise clarifying the term) at the end of the title would make it clearer.

Introduction: While the introduction presents a thorough overview of previous work, it is rather elaborate. Condensing this by focusing on the most important points would increase readability. This is later also true for the description of the TES data and the CMAQ model. Please specify at some point in which period the CalNex campaign was active.

Data: Please give coordinates and elevation of the Bakersfield site.

Models: In the text on the CMAQ model results of sensitivity studies are already provided. Consider moving this to the results section. You could also consider dedicating a paragraph to the description of the emission database (which are the most important ammonia sources, etc.) as this is so important in your uncertainty analysis later. Page 8, line 32: '... soil emissions potential and NH<sub>4</sub>': sentence is incomplete.

Results: Section 4.1: You claim that the relative changes in NH<sub>3</sub> concentration along the transect are captured well by the model, but to me this seems not to be the case: the highest concentrations are underestimated much more strongly, also in a relative sense, than the lower concentrations outside the direct source region. Also, based on figure 3 you conclude that CMAQ with the CARB inventory captures the spatial variability near Bakersfield well, but given the correlation coefficient of 0.22 for the overpasses closest to Bakersfield I'm not sure this statement holds. The purpose of highlighting these points is not clear as it is later not at all discussed. If I understand the plot and caption correctly, each point represents one overpass in one grid cell, i.e., this plot shows both temporal and geographical variability. Is this correct? If yes, could you comment on which part of the scatter is caused by temporal and which by geographical variability? Section 4.2: From figure 4 I don't see an underestimation of a factor 2.5 during the daytime, rather 1.5-2. Line 26-27 (page 9) would be better supported by adding a time series of the measurement-model comparison to show the seasonal pat-

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terns. Lines 3-6 (Page 10) seem redundant. Lines 13 and onwards (page 10): please mention that you now compare concentrations at 400+ meters above the surface; otherwise the step from the ground-based observations to air craft might be confusing. Page 10, line 28 – page 11, line 7: This section can be shortened significantly; consider if results that are not worth showing are worth talking about. Section 4.3: What does ‘consistent with measured temperature patterns’ mean? I assume it suggests that temperature is the driving variable for the emission variability during the day, could you state that more clearly? Why did you only adjust the hourly emission profile for NH<sub>3</sub>, was there no day-to-day variability (e.g. related to temperature) to take into account? With the approach taken, you assume that concentrations in a certain hour are dominated by emissions in that same hour; could you comment on this assumption? Why did you decide to test the new diurnal profile for 7 days only? A comparison to the aircraft data would be valuable here as well, to see to what extent the changed diurnal profile impacts modelled concentrations and model performance at higher altitudes.

Section 5: This section would be stronger if it contained more than a summary of the most important points of the paper, but also a discussion on future steps / important work to be done to improve the modelling of ammonia and the representation of emissions. For example, a discussion on the relative importance of the misrepresentation of emission diurnal cycles vs. misrepresentation of the vertical mixing (which should we work on first?) would be valuable.

Technical comments: Page 2, line 6: photoxidize should be photo-oxidize Page 3, line 11: CONUS might not be a known acronym for non-US readers; please explain. Page 3, line 25: Write out TES as it is the first mention in the main body of the article. In general: check for unexplained abbreviations. Page 7, lines 11 and 12: HSRL is mentioned as acronym but only written fully at the second instance. Page 8, line 14: SoCAB is not explained. Page 9, line 13: scatterplot should be scatter plot Page 11, line 23: remove ‘mostly’ as the CARB NH<sub>3</sub> emissions are completely constant.

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