

In “Evaluating present-day and future impacts of agricultural ammonia emissions on atmospheric chemistry and climate”, the authors present simulations of present day and future ammonia pollution at a global scale with the LMDZ-INCA model with new and improved NH₃ emissions from ORCHIDEE, then evaluate the impact of future ammonia burden on air quality and climate. Comparison of present-day simulated NH₃ with satellite and ground-based measurements shows improvement from other inventories. Future projections are informed by SSP scenarios, and consider both changes in aerosol burden and composition, as well as changes in N₂O production from NH₃ oxidation. Overall, this manuscript presents an interesting outlook on future air quality and climate conditions from the perspective of ammonia emissions that is within the scope of ACP. I recommend publication following some clarification and revision of methods and results.

General Notes:

When comparing against ground-based data, no consideration appears to have been made for differences in monitor spatial or temporal coverage, which may hinder direct comparisons. For example, it appears ground-based measurements are only reported for 2015, which is mismatched temporally with the model simulation. What averaging methods are employed to gather a single value for different species that are each measured with different methods and temporal resolution? Beyond data completion of data record, is any further QA performed with this data to ensure measurement validity? The authors report excellent agreement between measured NH₃ and modeled results in the Mid-US in Figure 6; however, there are only 2-3 surface sites shown within the “hotspot” region of the Mid-US to make this comparison, and at least one site appears to have the maximum difference threshold between model and measurements. Surface monitor locations are notably sparse in agricultural regions of the US where emissions tend to be high, and similar biases may exist in other regional networks. Please clarify the methods and findings in this section.

Further analysis may be warranted regarding the findings of section 5.1, particularly for changes projected in China. This topic is important, but to my knowledge there is no substantial evidence that SO₂ and NO₂ controls increase ambient ammonia burden, and the authors only reference one publication in this discussion of these results, though others exist for China and other regions. In Warner et al., 2017 GRL, for example, the authors postulate that ammonia increases in China are due to a combination of sulfur controls, increased fertilizer use, and increasing local temperatures. It seems that these variables could be further investigated within the model outputs here.

I find the expansion of results towards radiative forcing to be a particularly interesting portion of this work; however, the methodologies here are unclear, especially regarding the N₂O estimates from ammonia oxidation. These N₂O estimates are also highlighted as a major conclusion of this work in the abstract, but the analysis presented in the actual manuscript seems overly brief for this to be a main conclusion. Similarly, the abstract references a range of N₂O (0.43 to 2.10 Tg/yr), but only single figures are presented in the results and discussion. This section also refers to figure 5, but N₂O is not a component listed within that table. Should changes in the N₂O budget as a result of increasing ammonia be a main conclusion of this work, more in-depth analysis should be considered, perhaps with a figure or table dedicated to this section. The conclusion section also presents somewhat different estimates than what is represented in the abstract and main text. It feels startling to see mention and estimates of ammonia/hydrogen economy in this section of the

conclusions when it has not been previously mentioned in the manuscript. Clarification of numerical estimates and methodologies in these sections is necessary.

Specific Comments:

-Line 32: the meteorological variables described are unclear. Is this referring to air temperature and humidity, or soil temperature and moisture?

-Line 42: a citation should be used regarding the claim that livestock activities and synthetic fertilizer use are projected to increase.

-Please ensure that citation formatting is consistent throughout the manuscript. For example, on page 2 line 43-48, the authors reference Hauglustaine 2014 on the first sentence but not the second. Later in the same paragraph, the authors reference Beaudor 2024 on both sentences referring to that work.

-Line 67: clarify the gas-phase and particulate species examined in this work.

-Line 68: It may be beneficial to include greater detail of the SSP scenarios used in this work in the introductory section of the manuscript. As mentioned here, it is unclear what is meant exactly by “most and least significant increase”.

Line 72-74: delete “more precisely” and “respectively”.

-Line 75: what is meant by different levels? This type of descriptor is used several times throughout the manuscript but is poorly defined. Delete “structure of the”.

-Page 4: why were the present day years 2004-2014 chosen for this analysis? This time period is somewhat mismatched both with satellite record (2011-2014) and surface-based measurements (2015).

-Lines 97-99: Animal density considerations are an interesting and somewhat underexamined variable towards NH₃ emissions, and are described here as a critical input, yet the text does not describe how changing densities are represented. While this is described by a prior publication, that publication is not referenced at this point of the manuscript, and it may be worthwhile to add further detail to the current manuscript.

-Line 107: change “it represents” to “each represent”

-Please ensure that all acronyms/abbreviations are described before they are used in the manuscript. For example, LMDZ-INCA is not defined until line 120 but is used several times before this.

-Line 151: Sander 2015 is an outdated reference and should be updated to Sander 2023.

-Line 154: Is there a reason the ERAInterim reanalysis product was used instead of the ERA5 product?

-Line 160: What meteorological variables are employed?

-Line 161: delete “however”

-Line 171-172: I'm not fully certain what is meant by low levels and contrasting conditions in these descriptions.

-It would be helpful to have greater reference towards what each CAMEO-SSP simulation represents, such as an additional column in Table 2 describing textually the purpose of the different conditions.

-Line 178: Why are only these years of the IASI record employed for comparison?

-Line 203: change to "CAMEO emission also enhance".

-Lines 209-214: I find this analysis of compensating errors to be unclear. Where is the 47% bias estimate coming from for the US? From table 3, I see a 46% difference between Mean Obs and MBE CEDS. What is the threshold for significance of these differences?

-Line 220: Why is the seasonal cycle omitted? Could this be added to the supporting information?

-Page 9: An additional figure showing the difference between CAMEO and CEDS would be helpful, possibly for the Supporting information. In this figure and others (e.g., Figure 12), differences between outputs are reported only by absolute differences—would it be more clear to represent these as relative (%) differences between outputs?

-Lines 239-241: It is unclear why dust estimates are being brought up here.

-Line 252 and Table: The AMoN network is run by the US National Atmospheric Deposition Program (NADP), not the US EPA. Please ensure the text and tables reflect this.

-Line 261: Beijing

-Line 264: high concentrations of what?

-Related to the comment above, use of panel lettering on plots such as Figures 4-6 and more detailed in-text references would aid in understanding these comparisons. I question if all of these figures are necessary for the main body of the manuscript instead of the Supporting Information, as they are not heavily mentioned or discussed within the main text.

-Page 16: figure caption 6 describes European stations but displays information pertaining to North America. Ensure all figure captions are representative.

-Line 300: delete "also".

-Paragraph starting on Line 315: "depositions" should be referred to as just "deposition".

-Line 322: Change "except in" to "aside from".

-Line 359: Change "relatively" to "relative".

-Page 20: The calculations presented in the text are not correct. Ex: The difference between CAMEO and CAMEO[585] should be 59%, not 37%. Please ensure all percent changes are calculated correctly throughout the manuscript.

-Page 23: Figure 11 is referenced in text before Fig 10. Please ensure all figures are referenced in the appropriate order.

-Line 488-489: Why is AeroCom Phase III and/or GISS brought up here when it has not been mentioned before this? Citations should be provided if the authors are comparing their findings to other literature.

-Line 497: Why is the sulfate radiative impact not shown for both CAMEO simulations?

-Line 499: change “slow down” to “slowed down”.

-Page 33. The last numbered paragraph (9) of the summary and conclusions represent future directions, and therefore should not be included as a numbered conclusion item. I believe it would be stronger to end the manuscript with a paragraph or two that bring together the full conclusions of the manuscript, incorporating the future directions noted in the (9) paragraph.