Comments on:

Responses of surface ozone to future agricultural ammonia emissions and subsequent nitrogen deposition through terrestrial ecosystem changes

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Decision: accept with minor revision and clarification

General comments:

In this manuscript, authors present a novel linkage between agricultural activities and ozone air quality, by examining the responses of surface ozone air quality to terrestrial changes caused by 2000-to-2050 increased ammonia emission and resulted increased nitrogen deposition. Authors make use of CESM model to investigate each individual and combined effects of LAI, canopy height and soil NOx, and try to isolate biogeochemical effects by using prescribed meteorology. In general, the manuscript is very well written! I think this manuscript meets the criteria for publication on Atmospheric Chemistry and Physics:

- It is an advancement in understanding the linkage between ozone air quality and agricultural activities.

- Evidence provided by the authors are strong for the conclusion drawn

- This work is of importance to researchers studying atmospheric chemistry, physics and atmosphere-biosphere interactions

However, there are some questions and details needed to be further addressed from my perspective:

General revision suggestions:

- Figure 1. This illustration is very helpful to readers who are not very familiar with the complex interactions between atmospheric chemistry and terrestrial ecosystem. Since one of the major conclusions is that ozone changes are typically larger when meteorology is dynamically simulated, I am wondering whether some biogeophysical effects/pathways could be added to this diagram. I understand it could get

overcomplicated very fast, but maybe one or two pathways explained in Figure 7 should be added.

- Spin-up period for the model. I see that CLM45BGC mode has been spun-up for 150 years, and then 50 years for steady state. Perturbation experiment is then done for another 60-70 years. This seems an impressively long period of time for spin-up and perturbation. Is this a common practice for this mode of CLM model? Or how did you determine that the model has reached a steady state? Did the model start from zero vegetation (LAI=0)? I am interested to look at maybe just one figure showing the evolution of mean LAI over certain region during these hundreds of years of simulation. You don't have to include it in the appendix.
- P7L12, '…, we estimated that year-2050 NH3 budget to be 71 Tg N yr^{-1}, …'. I noticed and you discussed later as well that this number is the same as RCP8.5 projection. It is probably worth mentioning the fact and that FAO makes similar assumption as RCP 8.5 scenario here.
- Figure 3b. I think it would be more beneficial to have this figure in percentage changes rather than absolute changes.
- In Figure 3c, you have shown GPP reduction due to nitrogen limitation. I noticed some discussion about it is given in Section 4. However, I am wondering how you obtained this variable. I might have missed the part where you introduce this, but did you compute it by comparing two simulations (one with and the other without nitrogen limitation), or is it from some nitrogen limitation parameter in the model? Some introductions could be added in Section 3 or 4.

Also, some **technical corrections** need to be made before the publication:

- P3L4, 'facilities' to 'facilitates'.
- Figure 7 and 8, labels are inconsistent between caption and subpanels. Also, there are two subpanels labelled f.