The authors have conducted a very thorough revision of the manuscript in response to reviewer comments, and I thank them for their efforts. I do still think a few things need some additional consideration, the most important of which are the interpretation of the NH3 time series, the NOx titration discussion, and the BDSNP default fertilizer assumptions. All line numbers refer to the tracked changes version of the manuscript. Please ignore any highlighting below, which is copied from the tracked changes version of the manuscript.

General comments:

1. **NH3 time series:** Figure S4 and S5 (the NH3 time series): I mentioned this in part in the first review, but I think the NH3 time series are extremely important for interpreting the NO2 peaks—so much so that I continue to (strongly) argue for overlaying the NH3 time series over the NO2 time series in Figure 2 of the main manuscript. I'd also love to see a version of Figure 6 for the entire spring (March through June).

   The issue is that NH3 peaks are occurring in June, after the first topdressing application, with *much* smaller peaks associated with the planting fertilizer application. This is consistent with a split application in which considerably more fertilizer is applied at topdressing than at planting. But it differs from the NO2 time series, which has a larger peak in March than in June.

   To me, the NH3 time series looks like a very convincing signal of fertilizer emissions. The essential question here is why the two time series don't have similar dynamics. Even if no clear explanation is found, it needs to be acknowledged that the ammonia time series raises some questions about whether the March NO2 peak does indeed represent a fertilizer pulse.

   As it is, I'm not sure we understand what's happening in this system. Possibilities that come to mind that might be worth exploring to explain the differences in the two time series could include some combination of 1) a fossil fuel source of NOx in March, 2) elevated background soil NOx emissions in March associated with the 'spring thaw' period in which soil microbes become active during a period where there is no competition for N from plants, and after the winter during which N is expected to accumulate in soils (this is separate from any emissions associated with a freeze/thaw transition), 3) differences in fertilizer type (i.e., fertilizers with different potentials for ammonia volatility) between March and June, and 4) differences in application method (e.g., banding or deep soil placement vs broadcasting, particularly if without incorporation) in March and June, with differences in fertilizer amounts in March and June potentially contributing in cases 3 and/or 4.

   And it's important to remember that BDSNP is going to be heavily—and it seems inappropriately—biased towards having a fertilizer-induced emission peak *only* at planting/green-up. But you do have good correspondence between BDSNP and OMI in 2020 (Fig 3), which is a bit of a mystery to me (and a real limitation of having only a single year of simulations).

If it is not possible to fully resolve this issue in this manuscript, in future work, it might be interesting to include a dynamic bi-directional NH_3 scheme along with BDSNP to explore the fertilizer pulsing question.

2. **NOx titration discussion**: In general, I'd really like to see more discussion of this and better contextualizing--it's a very interesting result (15.6 $\mu g\, m^{-3}$ reductions) that needs more interrogation. Attributing NOx titration occurring in March but not in the summer to solar insolation is not convincing as currently argued. I would have thought NOx titration of ozone to be more related to VOC:NOx ratios than insolation, and I wouldn't expect daytime insolation in March to be all that different in summer at 33N-40N, and the argument being made here is that the insolation is different enough that instead of causing a 15.6 $\mu g\, m^{-3}$ reduction in daytime ozone in March, NOx emissions in the NCP cause an *increase* in daytime ozone concentrations in summer. Left out of the discussion is how large the increase in VOC emissions is between March (when it is presumably negligible) and summer (when it is presumably at its annual peak), and whether that's enough to shift the ozone regime—to me that sounds like a more likely culprit than insolation. The model could help answer these questions. Patterns in ozone measurements at the surface or from space that support these dynamics would also be helpful for supporting and understanding the model results.

   Huang et al. 2023 (https://doi.org/10.5194/acp-23-14919-2023) might be a good place to start (but not end) the discussion: it includes evidence for soil NOx leading to ozone reduction over the NCP in section 3.3, e.g., "With a 25 % reduction in soil NO emissions, there was a widespread small decrease in monthly average MDA8 ozone concentration (ΔMDA8: $-1.5 \pm 0.9\ \mu g\, m^{-3}$), except across the NCP, where ozone showed a slight increase (up to 1.3 $\mu g\, m^{-3}$) in the Shandong and Henan provinces." I think that reference also describes the NCP as VOC limited, which is important to point out.

   Purely optional, but it may also be interesting to discuss a bit more the implications of agricultural emissions for ozone and air quality goals in urban areas, especially as fossil fuel NOx emissions decrease.

3. **BDSNP default fertilizer assumptions**: In my original point 6, I raised some questions about fertilizer applications. Thank you for clarifying that the default BDSNP implementation was used. I think some additional text is needed to acknowledge important limitations of that implementation.

   "*Though the 75/25 treatment is the most typical global farming practice (Matson et al., 1998), it may probably introduce extra biases in a specific region.*"

   While it is true that Hudman et al. included this statement in their 2012 paper, it is incorrect. 75/25 is not typical global farming practice. Matson et al. 1998 is not an appropriate reference to support this statement; it is a paper focused on a site in Mexico and makes no statements about global fertilization applications; the 75/25 split is an estimate for local farmers in Mexico in 1994. Typical practice would place more fertilizer in a topdressing application, and less at planting—e.g., GGCMI, the largest global crop modeling exercise, uses a 20/80 split (see 7th paragraph under "GGCMI phase 3 crop modelling protocol" in the Methods of https://doi.org/10.1038/s43016-021-00400-y). I did a quick search on fertilizer splits, and found this statement (but I expect if you simply contact an agricultural extension agent you can get an answer): " 'One base and one topdressing' mode is currently the most common form of fertilization for maize spring in Northeast China[24,25,26], with 40% of nitrogen fertilizer is applied as a base fertilizer, and 60% is applied as top dressing during the growing period." https://doi.org/10.1038/s41598-023-38724-3. I would also report what the fertilizer split was at Fengqiu Station.

Also it's worth noting that BDSNP treats the topdressing not as a single application, but is spread out over a period of weeks or months, which is also perhaps not an ideal assumption that will affect results.

I think the best path forward for the authors is to simply state that they used the default BDSNP fertilizer & global emission assumptions, and that these assumptions (in this case, primarily the 75/25 split and not applying the topdressing in a single application) may not accurately the fertilizer applications in China. As the authors note in part in lines 159-165, the offline fertilizer files and emissions tuning factor end up being fine (though not necessarily for the right reason). The authors could also argue after line 165 that the BDSNP tuning, which was done for year 2000 agricultural emissions estimated by Stehfest & Bouwman, is not so far off from recent estimates of global emissions, possibly on the high side (e.g., Table 2 in DOI: 10.1111/gcb.16193, and https://egusphere.copernicus.org/preprints/2025/egusphere-2025-1416/egusphere-2025-1416.pdf argues that current-day estimates range between 0.84 and 2.2 Tg N yr-1, though note this manuscript has not been peer reviewed).

But the 75/25 split (including the assumption that the 25% is applied evenly throughout the remainder of the growing season) is something that needs to be acknowledged – you could simply state something like 'using a 20/80 split in fertilizer applications, as is commonly used in crop modeling (Jägermeyer et al. 2021 https://doi.org/10.1038/s43016-021-00400-y), or a 40/60 split, as has been reported to be common in Northeast China (https://doi.org/10.1038/s41598-023-38724-3) would be expected to result in differences in the magnitude and timing of emissions compared to the default BDSNP scheme. Among other impacts, at planting there is no canopy interception of emitted soil NOx, which would result in substantially larger emissions to the atmosphere under the default BDSNP 75/25 split than in a 20/80 split. In addition, because BDSNP applies the 25% topdressing application evenly over the growing season following the 75% basal application, it is less likely to produce sizable pulses of emissions.'

(As an aside, something that may be of interest to the authors: it may be that as a consequence of having unvarying fertilizer applications and constraining global fertilizer emissions to 1.8 Tg N, BDSNP is unable to reproduce historical trends or much in the way of interannual variability in emissions: https://egusphere.copernicus.org/preprints/2025/egusphere-2025-1416/egusphere-2025-1416.pdf)

4. Regarding the row anomaly: the issue is that in the L3 product, the number of pixels changes over time because the pixels affected by the row anomaly (and excluded from the L3 product) changes over the OMI lifetime. This is the issue that I was asking be acknowledged/addressed. All that really needs to be done here is to acknowledge that the number of pixels included in NO2 retrievals changes over time because of the increase in the number of pixels affected by the row anomaly, making the data unsuitable for trend analysis and possibly introducing uncertainty in seasonal averages.

Specific comments:

Line 85-86 "incorporating high fertilization rates according to the solar terms with excessive N fertilization." What is meant by 'according to the solar terms' will not be understood by the majority of readers (including me) and will need to be explained. Perhaps it would be better to use the growing season or calendar year as context?

Line 87: "Thus, this region is primarily responsible for agricultural N-fertilizer consumption" This line is not clear as currently phrased. Should it be "This region is the largest consumer of agricultural fertilizer N in China"?

Line 95: I still think this pulse is not unexpected: e.g., fertilizer pulses have been seen by OMI, fertilizer pulses are typically observed in field measurements, and BDSNP is designed in a way that would result in fertilizer pulses of some duration (Hudman et al. 2012)

Line 196: "the trajectory NH3 from IASI is integrated into each 0.125° × 0.125° grid cell with the average during 2007-2021" This description sounds inaccurate. I don't think "trajectory" is the correct word here; is vertical column density intended? And I also think "integrated" is incorrect (at least, I do not understand what is meant by "integrated" here). Since these were L2 data, the method of regridding should be specified, including the specific screening criteria (i.e., % cloud cover, retrieval error, and whether exceptions to the retrieval error criteria were made for low concentrations). In addition, "the average during 2007-2021" is stating that the dataset is a single map of a long-term mean, but a time series is presented in the SI figures. Finally, the 0.125 x 0.125 resolution seems much too fine for daily IASI retrievals. This all actually sounds like the description for an oversampled dataset, not the time series presented in the SI.

Line 209-212: In my previous comment, I was asking whether these measurements were made continuously, hourly, daily, or at some other interval.

Lines 260-262: I don't think this statement is accurate—the NH3 peaks are in June, not in March. Please see my first general comment above.

Lines 275-278: Same comment as for 260-262

Lines 298-301: I think it's important to note here that BDSNP is going to be heavily (and arguably inappropriately) biased towards having a fertilizer-induced emission peak *only* at planting/green-up.

Line 329: move the definition of IOA from line 361 to here.

Line 332 and following: I would add discussion here about the discrepancies between the IASI NH3 peaks (which occur in June) and the OMI NO2 peaks in March.

Line 339-341: "*Though the 75/25 treatment is the most typical global farming practice (Matson et al., 1998)*" Also discussed in a general comment. While it is true that Hudman et al. included a statement like this in their 2012 paper, it is not correct. Matson et al. 1998 is not an appropriate reference to support this statement; it is a paper focused on a site in Mexico and makes no statements about global fertilization applications; the 75/25 split is an estimate for local farmers in 1994. I do not think it's possible to defend a 75/25 split globally, but I think you can simply acknowledge this as an issue and discuss how it may affect results (i.e., the point that BDSNP is going to be heavily (and arguably inappropriately) biased towards having a fertilizer-induced NOx emission peak *only* at planting/green-up).

Line 385-386: Change to "experiment that excludes soil sources of NOx and NH3 in the study domain" if that's correct.

Line 404-405 (Figure 4 caption): change "surface NO2 and NH3 during March 2020 over the NCP. (a and b)" to "a) surface NO2 and b) NH3 during March 2020 over the NCP."

Line 421: Li et al. 2017a only discusses NOx titration at night. The interesting result presented in this manuscript is the daytime titration and the large reductions in daytime ozone concentrations. Huang 2023, discussed in general comment 2 above, seems like a better place to start.

Line 435-440: I think just make it explicitly clear that this experiment doesn't include any changes in HONO emissions. Maybe "We note that soil nitrous acid (HONO) emission, which are not included in these modeling experiments,…".

Line 464: See my comment to line 421--I believe Huang et al. 2023 actually includes evidence for soil NOx leading to ozone reduction over the NCP.

Line 515: Change "Changes in surface NO2 and . . . " to "Simulated changes in surface NO2 and. . . "

Figure S2: It took me a while to understand that the focus of these figures is the grey bars. I would change the title of the figure to "A re-presentation of the long-term NO2 [tropospheric] column time series shown in Figure 2, but with June and October pulses highlighted using grey bars." Also, the 'short bars' are in Figure 2, not Figure 1, and you need to explicitly define here what the red bars represent in each panel—i.e., what fertilizer event they represent--since that is different from Figure 2.

Figures S4 & following: the figures are not properly numbered, starting with the second Figure S4.