We thank the reviewer for many insightful comments and good suggestions, especially regarding the point about considering attention the uncertainties of the aircraft data. Our responses to the reviewer's comments are below in blue. Where there were extensive modifications to the text we point to the relevant section rather than including all the modified text in this document. Please note that the reviewer's text appears somewhat oddly formatted (maybe this happened during the conversion to PDF).

Validation of NH3 observations from AIRS and CrIS against aircraft measurements from DISCOVER-AQ and a surface network in the Magic Valley

Overall summary:

I welcome any validation study for ammonia satellite products as, like the authors noted, there are precious few available. Aircraft based measurements provide an almost ideal method for validation which makes this study highly relevant. Even more relevant is the fact that these are, to my knowledge (limited as it is), the first detailed evaluation of CrIS retrieved profiles under inversion conditions. It is also the first study describing the MUSES retrieval and validation. However, some of the approaches taken in the study seem counter intuitive and the manuscript/method will either need some adjustments or at least some further detailing.

Major comments

1. As this seems the first study describing the MUSES retrieval for AIRS and CrIS, that in itself could be given more focus by adding it to the title of the manuscript. Furthermore, illustrate the value by adding a summary to the conclusions on the strengths of the retrieval / improvements over CrIS-FPR.
   MUSES was added to the title, a good suggestion. A thorough comparison of the CFPR and MUSES algorithms was not the goal of this paper, but we have added a short section in the Supplement (see next response), where we also highlight some of the differences in the algorithms.
2. As described in the manuscript there is a clear difference between the CrIS-FPR and CrIS-MUSES retrievals as well as the previous AIRS and MUSES retrievals and it should be more clearly described to be as such. The current title and some of the statements ("Preliminary comparisons have shown excellent agreement between the two algorithms") might make it seems that this validation study is applicable to CFPR as well, which is not the case. To not confuse any future reader, please make it more clear what retrieval is used in the study and leave out any comparisons with other products. An alternative option is of course to include the CrIS-FPR retrieval to this validation study to illustrate the differences.
   A brief section was added in the supplement describing the preliminary results of a comparison between MUSES and CFPR. This was done at the requests of a several of the

co-authors and a colleague, who felt this was important, given that the CFPR has been widely used.

3. On several occasions, throughout the manuscript, the authors stress the importance of the applying the observation operator for a fair comparison to any second set of observed or modelled concentrations and/to reduce the impact of the apriori choice. While its clear, as stated several times, that the retrievals add information beyond any loss during the apriori choice, the information available is still limited and therefor the apriori will have a large influence. Already in the introduction it's made clear that a comparison of in-situ ground based observations is complicated and highly uncertain due to the strong influence of local atmospheric conditions and vertical distribution! (line 166-168). Several of the comparisons however are made without applying the operator (e.g. Figure 3, Figure 7, and any in-situ observation comparison). Any argument such as "there are many end-users who will want to use the data as is in their own analysis and will want to know the corresponding uncertainties. " or "IASI studies also don't use an operator" are no reason to not apply an averaging kernel and reduce the overall quality of the validation study. If anything it should be stressed once more that comparing satellite observations to in-situ data is not trivial. Assumptions can be made for vertical profiles (e.g. based on apriori shape, modelled profiles, or mixing layer height) after which an AVK can be applied. For the two week averaged concentrations an effective averaging kernel can be approximated. The authors strongly agree that applying the AVK is the optimal method for evaluating satellite retrievals. This was done for Figures 5 and 6, and users carrying out assimilations or inversions will use the AVK and the error covariance matrices in their work. But there will be other users who will simply be using the satellite in lieu of or in addition to in situ data, and will need to be reminded that there are major differences in what these different datasets are actually measuring. The authors feel that presenting the direct comparisons demonstrates these differences. We have revised the text in the MUSES and in the DISCOVER-AQ section to emphasize the importance of applying the instrument operator.

4. Validation vs Evaluation: One could argue that the study is not a validation but evaluation of the profiles as the vertical extent by the flights is limited to either 500 or 700hPa (and only surface for the in-situ obs). The further assumption that all concentrations above those levels are zero doesn't help bring the comparison closer to one another. Please change the title to evaluation and/or make a better assumption for the concentrations above 500/700hPa.

   We understand the reviewer's concerns but believe this is a validation, in that we are comparing the profiles over the mixed layer, the vertical range where almost all the ammonia in the atmosphere is concentrated. While the question of the how much ammonia is actually present above mixed layer is certainly important, especially over fire plumes, as we discuss in the rewritten section on the columns, it can only be resolved with better in situ instruments flying at higher altitudes under different conditions.

5. Which brings us to: The noise in the observed concentrations… make it hard to trust any of the observed concentrations above a certain level (950 (top figure 2) and 800hpa(bottom figure 2)) and concentration value (>5ppbv). Instruments capable of measuring ammonia at high temporal resolution are prone to large bias/artefacts (Bobrutzki et al., 2010/ Twigg et al., 2022) especially at "low" <10ppbv concentrations(funny standard) . The concentrations during the flights show an overall variation of 10 and 4 ppbv depending on the flights/profile/direction etc. This does not match the assumption of 0 ppbv above the measured profiles. While the variations could simply be instruments noise/artefacts, that does reduce the value of all measurements above a certain altitude (950/800hPa). Is it possible to perform any further QA/QC and provide ancillary information, such as the measurement error and mixing layer height to reduce the overall signs that the instrument is simply measuring noise/offset/artefacts.

   The reviewer's comment prompted us to reach out to the instrument PI, who informed us that the detection limits for NH_3 during DSICOVER-AQ (7.0 ppbv in California and 3.0 in Colorado) were much higher than we had assumed , based on those the PI had published for the VOCs measured during DISCOVER-AQ. Therefore we have rewritten our analysis of the validation stating that it is not possible to make any conclusions about the validity of the satellite data at altitudes where the aircraft data drop below the cited limits. We have added text in in the DISCOVER-AQ introduction and some plots in the Supplement that we hope insight into the variability at each level. We have also pointed out that the aircraft data are binned in layers around each CrIS retrieval level, and that these layers contain hundreds of aircraft measurement, from both ascent and descent.

https://amt.copernicus.org/articles/3/91/2010/

https://amt.copernicus.org/articles/15/6755/2022/

1. Not sure if to place the next comment under minor or major comments:

Line 158-169: A great summary of the pitfalls of previous studies… that we then proceed to walk into in this study. Each of these factors, up to a degree, can be accounted for and improve the validation study:

- Sub-pixel inhomogeneity, a fun point that almost no study does anything with besides mentioning it, some parts from Souri et al., 2022, could help https://amt.copernicus.org/articles/15/41/2022/.

  The authors studied the Souri paper and had several exchanges with the author. After some deliberation we determined that the Souri approach, while interesting, would not add useful information the paper. The best approach for estimating the impact of sub-pixel inhomogeneity is to have data that actually measures the variability of NH3 across co-located satellite pixels. We are doing exactly this with some QC-TILDAS data deployed on an aircraft flown over Colorado last summer during the TRANSAM campaign http://catalog.eol.ucar.edu/trans2am). We are also looking at HYTES data taken over the Imperial Valley. We now mention these activities in the conclusions, and thank the reviewers for prompting us to revisit this issue.

- Time-scales: the mismatch in representativeness of in-situ networks and satellite will result in a bias. Only a rough statement is made (line 554) but its unclear what the exact value is in this case. With a rough lifetime of 4-12 hours the concentrations that CrIS/AIRS observe will be a combination of emissions over the last few hours and not just the overpass. You could argue that CrIS will be more representative of morning/nighttime emissions and not the peak afternoon values. Please make the potential impact more quantitative by adjusting for the impact or adding a rough uncertainty estimate to the observations.
  This is certainly a valid question. We responded with the following text in the manuscript:
  While there are strong diurnal cycles in the NH_3 emitted from the dairy facilities (Leytem et al., 2011, 2013) the average daily emissions and temperatures, which strongly control the emissions, are close to the early afternoon values. Ideally one would use measurements of the daily cycle in NH3 concentrations, to estimate the ratio between the 13:30 and 24 hour mean concentrations, as was done by Pinder et al. (2011), but such data are not available for the Magic Valley site.

- Noise of the in-situ or satellite instruments?
  Satellite instrument noise is specified in section 3.3. This translates to actually low error in the NH3 retrievals (3-23%). The much larger components of the error are the smoothing and systematic errors, as well as sampling issues, as discussed in relation to Figure 6 and the new Figure S3. We have also added the following sentence in section 2.1: *For the retrieved profiles used in this study the measurement error ranged from 3.5% to 23%, the systematic errors mainly from 1% to 60%, with a few cases close to 100%, and the smoothing errors from 24% to 130%.*

As stated the horizontal and vertical distribution of ammonia can have a huge impact on the estimated total columns (e.g. factor 2, van Damme 2014). As stated above make an effort to reduce the potential impact or add a factor of uncertainty to the comparison to account for the potential impact.

- https://acp.copernicus.org/articles/14/2905/2014/acp-14-2905-2014.pdf

  Our rewritten section on the column amounts addresses the question of the vertical distribution. As stated in above in the response to the reviewer's suggestion of using one of the approaches in the Souri paper, and in our conclusions, while the issue of sub-pixel inhomogeneity is important, it requires data collected at the sub-pixel level.

Minor comments

1. Abstract lines 35-37 rewrite needed: The way it's currently written, to me, makes it sound like the validation study only represents a very tiny set of conditions and …not important..
2. Be proud of the study, the highly detailed (smaller set) of observations is a strength!
   Thank you for the praise. Rewritten as: These are small datasets taken over high source regions under very different conditions: winter in California and summer in Colorado

3. Line 35-40 add quantities to the bias/errors.
   Done

4. In several sections of the manuscript there are statements of outcomes of other validation studies but it is not clear which retrieval was validated. There have been several IASI products over recent years with large difference between them (typically updates). Please add the version numbers.
   Done.

5. Line 67: "within the European union" how its currently written makes it sound like the EU regulate US pollutants.
   Rewritten as: Ammonia emissions are regulated by the European Union (EU) and it is a criteria pollutant in Canada, but not yet in the US. However he EPA has published established regulations

Line 85: add reference to "measure accurately", for example Bobrutzki / Twigg, https://amt.copernicus.org/articles/3/91/2010/
We have added a reference to the von Bobrutzki paper and modified the relevant sentence: However, in situ measurements remain a challenge. NH_3 is easy to detect, but it is hard to measure accurately, especially for concentrations below 10 ppbv (von Bobrutzki, 2010)

https://amt.copernicus.org/articles/15/6755/2022/

1. Line 91: there are several instruments measuring via an open-path that are used in measurement networks (e.g. https://amt.copernicus.org/articles/10/4099/2017/amt-10-4099-2017.pdf, https://amt.copernicus.org/articles/16/529/2023/amt-16-529-2023.html)
   Added two references to open path instruments.

2. Line 100-105 add the spatial coverage/footprints of the individual sensors.
   This information is provided for AIRS and CrIS in the section 2.2. In this section the authors feel it is not useful.

3. Line 120: reference? (e.g. https://acp.copernicus.org/articles/22/6595/2022/acp-22-6595-2022.html, https://acp.copernicus.org/articles/22/951/2022/acp-22-951-2022.html)
   Provided a direct reference to the AMoN site.

4. Line 125 onward: add version numbers and retrieval names.
   Done.

5. Line 125 onward: instead of good/high/etc add quantities
6. Line 141 what was the result for IASI(-NN and -LUT)?
   Done.

7. Line 142:144: Incorrect statement. Most of the FTIR sites are located **away** from high source regions, which limits the applicability for high concentration regions. Several of the NDACC sites however (e.g. Hefei, Mexico city, Bremen, Boulder) are within or near regions with high concentrations which makes the complete network quite applicable, and to great interest of the air quality community.
8. Line 143: What is of greatest interest to the air quality community?

   The reviewer is correct, this statement is not right and was deleted. Note that only seven of the NDACC sites were used in the Dammer analysis, and that the CrIS Mexico City data were suspect. The following sentence was added to provide more context. Correlations at the individual sites ranged from 0.28 (Mexico City) to 0.86 (Bremen).

9. Line 158: Move a part of the sub-pixel bit (169-180) above this section for better readability.
   Done

10. Line 180-182: again makes it sound like the study is not that relevant, while it is!

    Rewrote as : Aircraft campaigns are valuable in that they profile the vertical distribution of NH_3, allowing us to evaluate the performance of retrieval algorithms and to provide models with more realistic profiles; however they are by nature limited in their temporal coverage

11. Chapter 2 & 3.3 integrate together into a MUSES chapter
    Done.

12. Line 199: Can you add an example of the apriori profiles and typical surface values.
    A priori profiles have been added as Figure S1. Figure 2 provides good examples of the range of typical values. We also added the following in the introduction: (surface values can range from less than 0.1 ppbv to 200 ppbv or more).

13. Line 214-218: either add a section comparing the two retrievals, provide a source for these results or remove this section.
14. Line 216: specifically "excellent agreement", while I understand the comparison is beyond this paper at least specify where anyone can find the comparison.
    The authors have revised this statement (see also the authors' response to the reviewer's second major comment).
15. Line 256:261: please add some quantities (ppbv/%) to what can be expected for each of the error sources. Show that III in particular is essential as it seems the part that's new within the MUSES retrieval.
    As stated above, the following was added to section 2.1:
    For the retrieved profiles used in this study the measurement error ranged from 3.5% to 23%, the systematic errors mainly from 1% to 60%, with a few cases close to 100%, and the smoothing errors from 24% to 130%. Example retrieved profiles and corresponding errors are shown in Figure S3.
    We also added the following at the end of the paragraph: Note that the estimated error cannot account for sampling errors, i.e., differences between the air masses sampled by the satellite and by the in situ instruments.

16. Line 284-287: Either remove, or add a few lines/statement to the discussion/conclusions that this study indicates the potential hazards of, and large levels of uncertainty in, simply using the CrIS retrieved surface concentrations.
    Please see response to third major comment.

17. Line 290: After the whole description, simply truncating the averaging kernel seem counter-intuitive. Of the limited information contained in each observed profile quite a bit will be above the 500/700hPa level. Smoothing etc will have an effect on the profile/column comparison. Please show that this effect is minimal or (better) redo the comparison without truncating and assume a value for the levels above 500/700hPa.
    We followed the reviewer's suggestion and extended the aircraft profiles by blending in the MUSES NH_3 prior above the top of the aircraft data and redid the profile comparisons, statistics table and error plots, then adjusted the text accordingly. The differences between applying the full AK to the extended profiles and applying the truncated AK to the aircraft only data were quite small.

18. Line ~295: add the dates/period to California 2013 and Colorado 2014.
    Dates were added, and the text was reorganized.

19. Line 310-311, why not bin the observations the CrIS and AIRS retrieval intervals?

    The values in Figure 1 were binned for clarity in the plot, which is just an illustration. For the comparisons with AIRS and CrIS, the aircraft observations were binned on the retrieval intervals, as stated in section 4.

20. Line 311: add the detection limit, and concentration interval that the 35% is representative of.
21. Line 314: "higher detection limit" how much higher?
22. Line 315: same, what amounts are we talking about?

After consulting with the PTR-MS instrument PI this section was rewritten as:

Note that the PTR-MS instrument samples the atmosphere at 1Hz but the data in Figure 1 were binned over 100 m to improve visibility. The estimated instrument uncertainty is 35% (Müller et al., 2014). However, the PTR-MS NH_3 data were a side product of the PTR-MS measurements during DISCOVER-AQ, which were designed to obtain data on volatile organic compounds (VOCs). NH_3 is sticky and accumulates in the instrument inlet, slowing the instrument response This effect leads to biases if the NH_3 amounts are changing rapidly (Sun et al., 2015);when the aircraft is leaving the boundary layer on upward spirals the instrument does not respond quickly enough to the sharp decrease in NH_3 and overestimates the NH_3 concentration; similarly, when entering the boundary layer on downward spirals, the response to the increase in NH_3 is slow, and NH_3 is underestimated (see Figure 9 in Guo2021). Furthermore, the detection limits for NH_3 were much higher than for the VOCs that were the primary target of the PTR-MS measurements: 7.0 ppbv in California and 3.0 ppbv in Colorado (Armin Wisthaler, personal communication). These limits imply that any aircraft observations below these values are effectively noise.

23. Line 323: technology and protocols: add reference.
24. Line 325 type of sampler? Quite the quality differences.
25. Line 346: add LSTs.

    Added.

26. Line 351: if CrIS-JPSS-1 is not used leave it out of the manuscript.

    We respectfully disagree on this point and have listed both JPSS-1 and JPSS-2, since NH_3 from CrIS on these platforms will greatly extend the NH_3 record.

27. Line 361: add some information on why 60 minutes and 15km are used (especially for low(<5kmph) and high (>30kmph) wind speeds differences in observed air mass are possible.
28. Line 362: summarizes the approach of Guo2021.
29. Line 363-364: stricter for a reason, other studies (e.g. Tournadre, 2020 for NH3) showed some information on the limits, reference.

https://amt.copernicus.org/articles/13/3923/2020/amt-13-3923-2020.pdf

This section was rewritten as shown below. The authors hope this addresses the reviewer's concerns.

The DISCOVER-AQ campaigns in California and Colorado provide the most comprehensive set of in situ NH_3 profile data (as opposed to retrievals from FTIR instruments) available. Both locations have many strong sources and each campaign carried out multiple flight days over a two month period. These datasets demonstrate the strengths and limitations of satellite data in areas of great interest to the air quality community; additionally, they allow for the evaluation of the accuracy of the retrieval estimated error, as calculated from Equation 3. During each flight the aircraft flew multiple up and down spirals. The satellite profiles were co-located with aircraft profiles taken within one hour of the satellite overpass time and 15 km of the pixel center, the same criteria used by Guo2021.This co-location criterion is much stricter than is usual for satellite validation (see Hegarty et al., 2022 for an example with CO retrievals from AIRS, who used nine hours and 50 km) but is necessary given the short lifetime of NH3 (on the order of hours to days) due its high reactivity and fast deposition. Tournadre et al. (2020) used an even stricter time requirement of 30 minutes for comparing FTIR and IASI NH3 retrievals over Paris, but we found that such a limited time window drastically reduced the available data. Given the chosen criteria, each CrIS or AIRS profile was compared with data from at most two spirals

1. Line 365-367: add values for future reproducibility
   Done.
2. Line 369: Why median and not mean? Did the data have strong outlier values / not well distributed?
   Yes, roughly 25% of the profiles had very long tails. Please see Figures S4 and S5.
3. Line 372-377: move up to line 347 as its appropriate to already mention the differences there.
   Done
4. Line 382: "amounts as high as 100ppbv …" add during a (weak) inversion?
   NH3 amounts downwind from CAFOS routinely reach values above 100 ppbv, even when there are no inversions (see Nowak et al., 2012).
5. Line 385:390: excellent case-study no negativity/toning down needed, adjust text.
   We thank the reviewer for this suggestion and have rewritten this section as: There were thermal inversions over the entire period (Figure 2, upper right), which lead to increased uncertainties in the retrieval, as they effectively create an emission layer above the surface, i.e., a layer that is warmer than the surface and therefore emits more than it absorbs. Inversions also limit the vertical extent of the boundary layer, with consequently lower NH_3 concentrations at altitudes where the retrieval has greater

sensitivity. Nevertheless, evaluating the AIRS and CrIS NH_3 profiles against the aircraft data is a useful exercise, as the combination of inversions and strong sources is not a rare occurrence, and this analysis demonstrates both the capabilities and limitations of retrievals under these conditions.

6. Line 391: "However, when averages over long periods and/or broad regions are desired, it would be reasonable to exclude cases with inversions", I have to disagree and argue the opposite, and ground-based measurement will also measure during these inversions, for an accurate comparison you'll need to include such events into the satellite mean, else its not representative of the situation on the ground.
   This is an excellent point and we have removed this sentence.

7. Line 395-407, to be honest this whole section could be removed. From previous studies and your summary in the introduction its already clear that there are large bias (or representation errors) to be expected from not applying the averaging kernel or spatial-heterogeneity, its not needed to show it here.
   Please see our response to the reviewer's third major comment.

8. Line 426: Please add the percentage that the 1.0ppbv represents compared to the total observed concentrations or add those to the text.
   Added the following to the text: roughly 7% to 10 % at the surface, increasing to 30% at 750 hPa

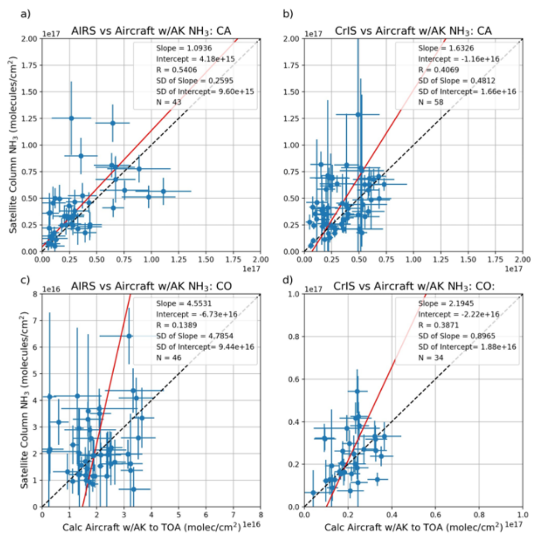
9. Line 427-429: Importance was already stated in the introduction. If you want to leave this section in, add some colouring of the profiles (fig3,fig5) based on the apriori profile concentrations. A percentage based plot would also help put the values into perspective.
   We are not sure exactly the importance of what the reviewer is referring to, but we have taken the suggestion of coloring the profiles and the column amount by the choice of the a priori profile.

10. Line 436: …has been argued…add a refence.
    We apologize; this statement was made to us at several conferences, but never in a paper. We have removed the sentence.

11. Line 451: similar like stated above, add some values on what order of uncertainty/error/bias we can expect for the individual errors. A plot like Fig. A2/A3 in Dammers et al., 2017, comes to mind. https://amt.copernicus.org/articles/10/2645/2017/
    A plot following Dammers Figure A2/A3 would be interesting but would require extensive analysis and is beyond the scope of this paper.

12. Line 453-455: "The measured uncertainties range from 5%-50% …. point to the need for averaging…" why is that the case? Most in situ instruments/measurements observe with comparable levels of uncertainty, similarly the uncertainties in the emissions can be up to several orders (Factor 2.5, higher values also mentioned within Van Damme 2018, stated in introduction / Dammers 2019 also in introduction).
    We agree with the reviewer that this statement is incorrect and have removed it.

13. Line 459:461: Again not an argument to make the same (incorrect) comparison here. Either replace entirely with, or add a comparison including the application of the averaging kernel and show the impact on the comparison.
We have experimented extensively with different approaches for carrying out the total column comparisons: using all the aircraft data, extending the aircraft data to TOA, as was done for the reworked Figure 5, applying and not applying the AK, and every case yielded significantly worse results for the AIRS Colorado comparisons and lower correlations for all cases. The figure below shows what happens when we integrated the reworked profiles from Figure 5 (aircraft profiles extended to TOA with the AK applied).



We leave it to the reviewers to decide if this plot is useful. Instead we truncated the AIRS/CrIS profiles to the MLH and compared those to the aircraft columns integrated to the MLH (new Figure 8). We also extensively reworked the text on the column comparisons for both California and Colorado, discussing the role the choice of the a priori profile. We feel comparing the total and partial columns, and considering the effect of the a priori choice and the aircraft uncertainties, along with the fact mentioned by the reviewer that July and August are fire season in Colorado, has provided some useful insights.

14. Line 461: incorrect statement, Dammers et al., 2017 (and 2016) did apply the averaging kernel to IASI profiles. The IASI retrieval uses a profile assumption and profiles can be derived from the columns. https://acp.copernicus.org/articles/16/10351/2016/
The IASI retrieval product consists of columns. Profiles can be derived from columns in a post-processing step, as Dammers et al., 2016 and van Damme et al., 2017 have done, but in both papers just two fixed profiles were used to convert from column to profile. The authors believe that this process adds uncertainty to a comparison with already large inherent uncertainties. But we expanded our statement by adding: *though Dammers et al., (2017) estimated IASI columns by using two fixed vertical profiles to convert column amounts to profiles.*

15. Line 469: "are assumed to be zero" As stated above this is quite an assumption and the impact should be quantified. Alternative choices such as using the values from the apriori profile or scaling the apriori profile with the observed values at the top of the spiral are also viable. The lifetime is of the order of hours – days which means there should be a non-insignificant amount of ammonia above the mixing layer and in the upper

troposphere. The July-August measurements in Colorado coincide with the fire season, long-distance high-altitude plumes could occur during this period and interfere in the comparison (e.g. Lutsch et al., 2016; https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016GL070114).

16. Line 486:487 + beyond: At the altitudes where the measured concentrations seem valid enough there is no indication of a missing error source. If the values of the aircraft are not representative of concentrations observed >500/700 hPa how can we still conclude anything about potentially missing error sources?

    See our response to Major Comment 5. Based on our revised understanding of the validity of the data the reviewer is entirely correct that there does not appear to be any unknown error source in the Colorado dataset.

17. Line 515: …water vapour retrieval errors… Add a bit of discussion on this outcome as the sub-pixel retrievals of water vapour etc were one of the addition of MUSES over the old retrievals (unless I am mistaken, always possible). If this only increases the bias/uncertainty is it smart to keep doing the retrievals as such?
    The authors do not consider the water vapor retrieval an addition to the process. Any optimal estimation retrieval algorithm for NH_3 (or any other minor trace gas) requires information on the atmospheric state. The CFPR and MUSES algorithms simply obtain the atmospheric state in different ways. The CFPR algorithm uses the NUCAPS water vapor and temperature derived from cloud cleared CrIS radiances (on the coarser nine pixel resolution). The MUSES algorithm starts from the single pixel radiances, and over multiple steps, retrieves surface properties, cloud optical depth, temperature and water vapor, as well a number of different trace gases (CO,CH_4,O_3) besides NH_3. This process ensures that the atmospheric state is derived using the same forward model and radiance data that are used in the NH_3 retrieval, reducing possible sources of error. The last statement has been added to the MUSES description.

18. Line 545: biased high should be biased low. Puchalski et al also gave a range of +18 to -32%.
    We thank the reviewer for pointing out this error and have corrected it.

19. Line 554: see earlier statement on using emissions for concentration representativity.
20. Line 559: "possibly" missing a closing bracket
21. Figure 9: Excellent example of the temporal variability picked up by the satellite instrument However, accounting for the apriori effects could bring these comparison a lot closer.

    Yes, maybe. However, this would require introducing a great deal of extra data (profiles above the surface, averaging kernels), which we suspect would just muddy the comparisons, as we found when we extended the DISCOVER-AQ profiles and applied the AK.

22. Figure 11: colorbar values are missing
    Corrected.
23. Line 616: add within the "United States" or something similar, recent EU emission databases are typically at distributed based on livestock numbers at each farm, with some inventories (e.g. the Netherlands) even at facility level that get aggregated to 1x1km^2
    Done.

24. Line 621: oddly specific, leave out, or add reference? (Herrera, 2022, https://acp.copernicus.org/articles/22/14119/2022/acp-22-14119-2022.html)
    We thank the reviewer for reminding us to cite the paper we were thinking of.
25. Line 624-626: Be proud! You're reducing the relevance of this paper, while it definitely is relevant!
    We believe we have addressed this issue throughout the paper and thank the reviewer for the excellent suggestion.

26. Conclusions: update statements following the above comments.
    Conclusions have been updated.

27. Line 624: show the shape and values in the retrieval section.
    We are not sure here what the reviewer referring to here.