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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aircraft-based inversions quantify the importance of wetlands and livestock for Upper Midwest methane emissions" *by* Xueying Yu et al.

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We thank the reviewer for the thoughtful review. Reviewer comments and our responses are provided below.

1) The authors present an estimate of methane emissions from the upper Midwest in the United States based upon aircraft observations. In general, I think the manuscript is well-written and the analysis is thorough. In a few instances, I would be cautious about not over-interpreting the results of the inverse model or drawing conclusions that are bolder than the constraint afforded by the aircraft observations. Below, I've listed a few specific suggestions and ideas for the authors.

Thank you for the constructive comments. We have revised the draft based on the specific suggestions below.

2) Line 43: There are a bunch of studies that have now looked at trends in methane emissions from the US.

We have now revised the introduction to cite more of the trend studies the reviewer refers to.

3) Line 197: Does the number "0.8" refer to a correlation coefficient or something else? Typically, when people estimate a correlation length, they estimate the length over which the covariances decay by 50% (i.e., analogous to a half-life) or decay to approximately zero. If you're using an exponential covariance model, you'd likely want to estimate the length over which the spatial covariance decays by 50%. If you're using a different covariance model, like a spherical model, you'd want the total decorrelation length.

Thank you for the comment. The value of 0.8 was referring to a correlation coefficient. We have now revised the text to clarify this point as follows:

"The adjoint 4D-Var inversions derive methane emissions at 0.25° x 0.3125° resolution and in this case we use a 200 km length scale (decaying exponentially) to populate the off-diagonal elements of the prior error covariance matrix. Previous methane inversions by Wecht et al. (2014) and Monteil et al. (2013) assumed length scales of 275-500 km to further smooth the solution. In our case the analytical inversions impose strict error correlation by spatial cluster or source sector; thus, the adjoint and analytical analyses together span a wide range of error correlation scenarios. Since the analytical inversions solve for emissions by sector or by aggregated region, we employ diagonal prior errors in those cases."

4) Line 199: What kind of covariance model are you using here?

Exponential. Please see response above.

5) Line 200: I don't agree with the assumption that anthropogenic emissions have no spatial covariance. Across regional or continental scales, agricultural production shows spatial patterns; for example, if there are dairy or cattle farms in one model grid box, it's more likely than not that there are other dairy or cattle farms in the region. Also, this approach seems to contradict the approach in Sect. 2.3.3. If emissions were completely uncorrelated, then presumably each cluster in Sect. 2.3.3 would consist of a single model grid box (at least in regions that are dominated by anthropogenic emissions).

Thank you for the comment. This portion of the text was misleading since in fact the 200km correlation length scale described above was in fact applied to the total methane emissions, including anthropogenic fluxes. The manuscript has been updated accordingly.

6) Sect. 2.5: I think the information in this section shows that the inverse model performs well and indicates how sensitive the inverse model is to different setup parameters. With that said, this section has a lot of technical detail, and you could consider moving some of this information to the SI. Also, this information feels like it belongs better in a results section, but I think the current results section (Sect. 3) has a really nice structure that emphasizes a discussion of the emissions, not the results of technical sensitivity tests. Hence, the SI could be a good place for some of the information in this section.

We have now modified this section, and moved some of the more technical details to SI as suggested.

7) Line 406: The wording here ("We saw above...") feels a little awkward.

We have revised the wording as suggested.

8) Lines 465-474: Do you think the aircraft observations can yield a robust constraint at this level of detail? I can imagine that the model domain is a patchwork of grid

boxes that are dominated by different types of agriculture (e.g., beef, dairy, or hogs), and I wonder if the atmospheric observations provide a sufficiently strong constraint to differentiate among these different grid boxes.

There are several lines of evidence that lend support to this conclusion. First, while the GEM aircraft observations do not constrain emissions for every individual 25 km grid cell in our study domain, results in this section represent statistical averages over many (260-2374) grid cells. We have now added this information to the text and to Table 1. Second, we would like to point out that our use of multiple inversion frameworks is specifically designed to test the robustness of findings such as these; the corresponding uncertainty ranges are given in Table 1. Finally, our core conclusion here (a seasonally dependent bias for dairies + hogs that is not present for enteric-dominant beef facilities, thus implicating errors in manure emissions) is also supported by our domain-average results. Specifically, Figure 3 shows that livestock emissions for the domain as a whole exhibit a seasonal bias that similarly points to manure emission errors, and this finding is consistent across all inversion frameworks. Together, these points provide support for the conclusions in this section, and we have revised Section 4.2 accordingly.

We do agree with the reviewer's comment (and the one below) that more research will be needed to substantiate these findings, and we have therefore also added a statement that "additional research will be needed to confirm the role of manure in driving the top-down/bottom-up livestock discrepancies observed here, and to pinpoint the primary mechanisms involved".

9) Lines 525 - 530: I think that this study provides speculative evidence that manure emissions are driving the discrepancy between top-down and bottom-up emissions, but I'm not convinced that this study makes that link definitively. I think this explanation is an interesting hypothesis that meshes well with a few recent studies. With that said, I think the phrasing in this paragraph is much stronger than what the inverse model actually supports. This statement also goes back to a previous comment about whether the

aircraft data are sufficient to differentiate among different types of agricultural facilities.

We have softened the phrasing in this paragraph as requested:

"We further partition the derived livestock emissions based on county-level animal populations for beef cattle (>95% enteric emissions), dairy cattle (∼60/40% enteric/manure emissions) and hogs (mostly manure emissions). In this way we find that enteric fermentation emissions are well-captured by the GEPA inventory with low overall bias, but that manure emissions are underestimated by as much as 30% in summer and winter, with biased seasonality. While further research is needed to confirm this inferred role for manure in driving inventory errors, conclusions here are consistent with other recent work (e.g., Wolf et al., 2017; Yu et al., 2020; Wiesner et al., 2020). Better representation of manure management (for example, accounting for the timing and rate of field application, and incorporating finely-resolved information on management systems) thus appear to be important priorities for improving bottom-up emission estimates."

Please also see our reply to the previous comment.

10) Lines 548 - 556: I would combine this paragraph with the previous paragraph. I didn't quite understand the purpose of this paragraph or how it related to the research in the manuscript when I started reading it because this paragraph doesn't really have a topic sentence.

We have now reorganized this part of Section 5 for greater clarity.

11) Fig. 5: The change between prior and posterior seems very large for the northwest corner of the domain. There's also a lot of agricultural activity in that region. Do you think the inverse model is correctly attributing the increase in methane fluxes to wetlands (versus agricultural emissions)? The results using the 4-DVAR inverse model in Fig. 7 do not show the same change between prior and posterior across that part of the domain, and that makes me wonder about the results for this region of the model domain.

As the reviewer points out, this derived upward adjustment is stronger in the GMM inversion than in the other optimizations (especially in summer). We attribute this primarily to imperfect separation of the background and wetland signals for this portion of the domain. We have added a discussion of this point to the revised manuscript in section 3.1:

"These spatial patterns are robust across the inversions, but the adjustment magnitudes differ, for example, the GMM inversion yields much stronger upward adjustments in the northwest (Fig. 5-7). We attribute this spread in part to the imperfect wetland-background separation discussed earlier."

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