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Interactive comment on "Relating atmospheric N_2O concentration to N_2O emission strength in the U. S. Corn Belt" *by* Congsheng Fu et al.

Anonymous Referee #2

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The authors estimate N2O emissions for the US Corn Belt using a top-down approach. The authors have made a valiant effort, but I think the paper still has a long way to go. Below I have included a number of broad, high level suggestions and line-specific suggestions.

High-level suggestions: - In the introduction, I would be sure to emphasize the new or novel scientific questions that you want to tackle in the article. These questions should be different from existing N2O studies. In other words, I would focus on the scientific gaps that these existing studies have not been able to answer.

- Make sure the motivations and scientific questions in the introduction connect with the questions that you answer in the discussion/conclusions. For example, there is a lot of text in the intro about Eulerian versus Lagrangian models and the new perspective

that an Eulerian model would provide. However, it's not clear from the discussion and conclusion sections what we learned from this Eulerian model that we could not have learned from a Lagrangian model.

- I felt that the methods were convoluted, particularly given the simplicity of the top-down approach. For example, the paper appears to use both preset scaling factors and an empirically derived scaling factor (unless I misunderstood the text). This combination of both pre-set and empirical scaling factors feels too complicated, given that the top-down strategy in the paper is ultimately very simple. See the specific suggestions below for more detail on this point.

- Try to use technically specific phrases whenever possible and try to avoid broad generalizations. For example, the phrase "model accuracy" appears multiple times in the text, but it's not clear what aspect of the model framework that phrase refers to.

Specific suggestions:

Pg 1, Line 16: "evaluated" might be a better word choice here than "validated"

Pg 1, Line 20: You compare your flux numbers against EDGAR in the abstract. You could also consider putting these numbers in the context of existing top-down studies of US N2O emissions (of which there are several). Do you get similar or different numbers? In the latter case, what factors might explain these differences?

Pg 1, Line 33: "emissions are". I think it's more standard to use the plural ("emissions") and not the singular ("emission"). This suggestion applies to multiple instances of the word throughout the text.

Pg 1, Line 36: I would specify whether these methods are used by your study or by existing studies. I.e., the topic sentence doesn't make it clear whether are you are about to do a literature review or whether you are about to describe the current study. Consider using active voice here instead of passive voice.

Pg 2, line 19 "is usually simulated": I think yo could be a bit more specific here. E.g., "All of the top-down studies described here used STILT ....". Alternately, "All but one of the top-down studies ....".

Pg 2, line 21: "computational"

Pg 2, line 23: Why is it important to quantify the "spatial characteristics of atmospheric N2O mixing ratios"? I would argue that a model like STILT could be used for this task by running a large number of receptors or footprints. However, I don't think there has ever been a need to do so in the context of in situ greenhouse gas observations.

Pg 2, line 25: Again, why is it important to explore "the relationship between the spatial characteristics of surface emissions and the atmospheric N2O mixing ratio at the regional scale".

Pg 2, line 26: Why would an Eulerian model be any better at quantifying turbulence than a model like WRF-STILT? If you keep this motivation in the introduction, I would expand the discussion to answer this question.

Pg 2, line 34: What do you mean by "force agreement"? In what way are you forcing this agreement? Instead of using a phrase like this one, I would instead indicate whether your simple inverse model is a scaling factor inversion, a grid-scale inversion, a geostatistical inversion, etc.

Pg 2, lines 36-40: This study sounds very similar to Chen et al. I would try to delineate as clearly as possible what lingering science questions you want to answer in this study. For example, you mention analyzing the "influences of monitoring height on the inverse analysis results." You could elaborate on this point and explain how that analysis would benefit the community or answer important science questions.

Pg 3, line 20: Why use Niwot Ridge as the background in this study? In theory, one could use any number of different sites in the NOAA network as the background. Alternately, one could also use Arlyn Andrews' empirical boundary curtain product.

Pg 3, paragraph starting with line 25: In many STILT studies, the meteorological model

(e.g., WRF) has a different spatial resolution than the STILT footprint. This paragraph mixes and matches the resolution of the meteorology model and the resolution of the footprints. I would clarify which of these two cases you are referring to.

Pg 4, line 25: Why use these pre-set multipliers?

Pg 4, paragraph starting with line 28: I'm confused by the methodology here. Why use set multiplier values (as described in the previous paragraph) and then fit a coefficient ("a") to a simulation that has already been scaled by some pre-set multiplier? Instead, I would fit modeled concentrations (using some inventory) to the observations with a simple regression. I'm not sure what additional leverage one gets by using the more complicated setup described here.

Pg 5, line 5-7: I disagree with the statement here. One could generate footprints or sensitivities either using an Eulerian model adjoint or using a model like STILT. That procedure would circumvent the need to run a model in iterative fashion, like what the authors describe here. Also, this statement clashes with the introduction. In the introduction, the authors argue that Eulerian models provide a vital perspective that Lagrangian models cannot provide. But here, the authors point out a big shortfall of their Eulerian approach that would not be true of a Lagrangian approach. I would tone down the language in the introduction accordingly. I would also be more specific in lines 5-7. For example, if WRF-Chem does not have a readily-available adjoint, that argument would be more compelling.

General comment: The material in section 2.3 also relates to inverse analysis. I would merge sections 2.3 and 2.4.

Page 5, beginning of section 3: You may want to give you reader a road map/outline of this section, telling your reader what information to expect.

Page 6, line 25: This statement feels too broad or general to me. What kind of model performance are you referring to here? Transport model performance? The performance of your estimated emissions?

Page 7, paragraph beginning with line 4: I think that analysis here is complicated by the experimental setup that has the pre-set scaling factors. I.e., the low bias is the result of the particular scaling factors that you chose. Again, I would use a simple regression in sections 2.3 and 2.4 and focus on the results that lead to direct scientific conclusions.

Page 7, line 14: What kind of "model accuracy" are you referring to here? Atmospheric transport, accuracy of the emissions, spatial/temporal aggregation errors, or something else?

Section 4.1: This section is mostly devoted to validating your atmospheric modeling setup. It feels like this material is more well-suited to a supplement, not to the beginning of the discussion section. I.e., this section does not tell us much about N2O but rather explains why you think your approach is a valid one.

Page 8, line 25: I wouldn't use nighttime measurements at either 32m or 100m. You could potentially use nighttime measurements at 185m; that inlet height may lie above the nocturnal boundary layer. But I think even using nighttime measurements at 185m is really challenging/ill-advised.

Page 9, lines 21-25: I disagree with this statement, at least in part. The argument here might be valid for Kort et al. 2008. They used aircraft data from across the US and did estimate a single scaling factor. Miller et a. 2012, by contrast, used a grid-scale inversion (not just a single scaling factor), and they used tall tower data from the central US agricultural belt. Also, I don't think it's guaranteed that Kort et al.'s scaling factor would have been larger had they used data specific to the corn belt. I think it's a possibility but not a definitive explanation as presented here.

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