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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Constraining the CO_2 budget of the corn belt: exploring uncertainties from the assumptions in a mesoscale inverse system" *by* T. Lauvaux et al.

Anonymous Referee #2

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Review on the manuscript 'Constraining the CO2 budget of the corn belt: exploring uncertainties from the assumptions in a mesoscale inverse system', submitted for publication in Atmospheric Chemistry & Physics by Thomas Lauvaux et al.

This manuscript presents results from a regional scale atmospheric inverse modeling framework to constrain surface CO2 fluxes in the U.S. corn belt for a 7-month period in 2007. The approach mines an extensive dataset of continuous tower observations of atmospheric CO2 mixing ratios, aircraft data and eddy-covariance flux measurements to optimize surface fluxes, correct for biases in boundary conditions, and evaluate flux fields. To evaluate sensitivities of the findings towards different components of the mod-

eling framework, the authors compare results from various model setups (e.g. using 2 prior flux sources), and definitions of uncertainties.

The paper is well written overall, with all main components of the inverse modeling framework presented at an appropriate level of detail. There are a few inconsistencies concerning the definition of the prior uncertainties and the inclusion of boundary condition biases into the optimization (see details below). All results appear to be plausible and supported by the presented data; however, the strong focus on scenarios and the use of additional data sources to better constrain uncertainties is rather confusing in its present form, making it hard for the reader to separate between the 'main inversion run' (a.k.a. best case scenario) and the sensitivity studies.

Summarizing, this manuscript presents an interesting case study on the potential of atmospheric inverse modeling to constrain carbon budgets in a (relatively) data-rich, regional-scale case study, which should be highly relevant to the readers of Atmospheric Chemistry and Physics. After straightening out some shortcomings commented on in detail below, I therefore recommend accepting this paper for publication after minor revision.

MAJOR COMMENTS

One thing that puzzled me concerning the chosen setup for the presented study is the limitation of the timeframe to 7 months of data, even though it is stated in the methods section that several years of data would be available from these towers. Presentation of carbon budgets and uncertainty estimates for a full year, as well as the description of a full seasonal cycle across at least 12 months, would significantly strengthen this study (multiple years of data would be even better!). So please add results for at least the first 5 months of 2008.

The definition of prior flux uncertainties, both concerning the standard deviation and the spatial correlation structure, seems very arbitrary. Given the way the settings are currently presented, I am not convinced that they have been chosen based on reproducible, objective findings. The authors need to provide sound statistical approaches, e.g. variogram analyses, to back their settings with data. More details below.

In neither methods, results or discussion I found a plausible explanation and/or discussion how you separate your boundary condition biases from surface fluxes in the optimization process. In the discussion, you argue that boundary conditions vary at synoptic timescales, as compared to variations on shorter timescales in the fluxes, but I suppose you can find synoptic patterns in your flux fields as well, based on dominant weather patterns separated by passing fronts. I added several more detailed comments on this aspect below. Please clarify.

The results section leaves the reader with many open questions. The take-home-message seems to be that large parts of the available data are associated with high uncertainties, an independent validation of the fluxes is barely possible, and that there are multiple ways of setting up an inversion, with in part strongly deviating results. The interpretation of the impact of prior flux fields is purely qualitative, which is not sufficient here. The presentation of the scenarios of model framework setup is very confusing in its present form, and should be restructured.

DETAILED COMMENTS

p.2, Abstract: Overall very informative, but in parts too detailed, giving specific information on techniques that is not required here. This should be shortened, concentrating on the major findings.

p.3ff, Introduction: Well written overall! Just the 4th paragraph (p.4) may be shortened, since it is a bit too detailed, focusing on very specific issues.

p.4, 1st paragraph, '.., the performances of terrestrial ecology models ..': This is generally true, but I wouldn't consider it a major problem for inverse modeling studies, since they are dealing with much larger biases in other areas.

p.5, 1st paragraph, 'The apparent atmospheric sink due to the harvest..': Should be

rephrased. The harvest itself doesn't produce an atmospheric sink - it may, however, prevent the decomposition of crop material.

p.6, last paragraph of Section 2.1: The presentation of your dimension numbers is confusing here, since they are not commented on. I agree that it makes sense to compare knowns to unknowns here to set the stage, but you should better explain where e.g. '49x49x2' comes from.

p.7ff, Section 2.3: Good description of the flux model, and of the plans to use different prior flux fields and disturbed versions to test the robustness of the inversion. However, the choices for prior flux uncertainties and their spatial correlation seem arbitrary, this should be improved. See more details below.

p.9, 1st paragraph: The CarbonTracker description might deserve a little more details on the modeling framework setup. I would recommend splitting this paragraph into 2 sections.

p.9, 2nd paragraph: It is not clear what datasets form the references. I suppose you use AmeriFlux datasets – if so, this should be mentioned (and cited). What is the timeframe of comparison (e.g. daily, hourly)? The presented technique to first derive a 'base uncertainty', then scale it with seasonal fluxes, can be described as 'creative', at best, and I am highly doubtful if this is a good representation of your prior errors. If you decide to stick to this approach, you need to make an effort to explain and justify it. Missing aspects are e.g. how you treated spikes (which should play a dominant role for identifying your initial standard deviations), what the uncertainty of your standard deviation definition is, and what impact this has on your inversion results.

p.10, 2nd paragraph: The definition of spatial correlations in flux error is, again, rather creative. The good part is that many potential biases and uncertainties are discussed, but the final decision is not well explained. This part could well benefit from a solid statistical procedure (why not a variogram analysis?) to justify the chosen settings.

p.11, Section 2.4.1: This kind of setup documentation usually works better in a table. One missing aspect is nudging. Any FDDA used for that? Or overlapping daily runs?

p.11f, Section 2.4.2: Has there been a sensitivity study concerning the influence of the aggregation to 20k at the end? More importantly, what is the input data source here? RAMS?

p.12, Section 2.4.3: This Section is too detailed, e.g. concerning the reference gas target concentrations, for example. However, it would be important to know how many profiles were available, and at what frequency they were obtained.

p.12, Section 2.4.4: The first paragraph should be deleted, since the brief outline of the methods is only confusing without further details.

p.13, last paragraph: It is not clear why the authors state first that temporal correlation is important, but then decide to ignore it. I would suggest leaving out this part completely. The passages concerning the temporal correlations read OK.

p.14, Section 2.5.1: You may want to move the first part of this section into Section 2.5, since it is not truly focusing on aircraft data.

p.16f, Section 2.5.3: It is not clear to me how you manage to distinguish biases in the boundary conditions from biases in the surface fluxes in the optimization, without specific datasets that allow you to separate their influences on your mixing ratio observations. So this Section might need a bit of polishing to clarify your setup.

p.18, Section 2.6: I appreciate the open discussion on the serious limitations to this approach. However, it is not clear what sites exactly were used. For example, there are 3 Mead sites, all with different irrigation practices. Which one did you use here?

p.19, Section 3.1: This paragraph leaves many open questions. It appears that you compare a number of aircraft profiles with WRF/Chem data to assess uncertainties. The first question is if you only used those 6 flights shown in Figure 3 for this, or if you used the entire flight database. Second, it seems that your uncertainty analysis is

solely based on mismatches in boundary layer heights between data and simulations. If so, please clearly state that. Third, it is unclear what base uncertainty you use to add the BL-height errors. Do you take your entire model-data-mismatch (without BL error), then add 30-50% to consider the BL height uncertainties? Also, how was this range of values (e.g. the 30-50%) applied? Did you apply a gradient from early morning to mid morning, or did you pick values randomly?

p.19f, Section 3.2: You should mention that your results are highly sensitive to the spatiotemporal variability in flux fields. You are actually not evaluating the quality of your transport fields, but rather how differences in transport methods lead to mixing ratio changes. To give an extreme example, assuming you had totally homogeneous flux fields in all directions, your transport fields could be extremely different from each other, but your method wouldn't reveal this since the resulting mixing ratios would be the same anyway. Please include this aspect into the discussion.

p.20, Figure 3: This figure needs a clear comparison between boundary layer height estimates from both time series! So please (a) display BL heights, and (b) combine these into a single graph (might be a third column on the right).

p.22f, Section 3.4: I like the fact that the authors try to evaluate their fluxes with an independent data source. However, the findings in their present form are not truly convincing. In the first place, the figures do not clarify whether or not the posterior fluxes are superior to the prior ones. Next, you mention 'correlation' several times, but do not present any numbers to back your claims of 'good' or 'better' correlation. Also, it remains unclear how exactly you derived the eddy-covariance data uncertainty. Does this include uncertainty estimates from each individual site, or do you simply use across-site uncertainties? Also, your definition of 'homogeneous' is very loose, I would recommend using a different term for patches with >40% of a given biome type.

p.24f, Section 3.5: It is paramount that you put your findings into numbers! If you want to present a convincing analysis of the impact of your prior flux fields, use some kind of correlation analysis to demonstrate how the prior and posterior fields differ between methods.

p.25, Figure 5: It would definitely help to have the prior and posterior flux estimates side-by-side in a single figure!

p.26ff, Section 3.6: The description of test case scenarios is very confusing in its present form. Please use some kind of list, and assign names to each scenario in the text, to help the reader find the way through!

p.29, Section 4.2: As mentioned above, it is not clear what data can be used to clearly differentiate between BL and surface fluxes. The described shifts appear arbitrary.

p.30ff, Section 4.3: I do not agree with your interpretation here. First of all, you have much less data in winter, compared to summer, for your 7-month inversion, so it shouldn't be surprising that the relative weight of your priors is stronger there, and corrections are less pronounced. Next, when you present your arbitrary drought stress case, the quality of the findings should be mainly dependent on the setup of your prior uncertainties, so these results indicated that you obviously chose appropriate uncertainties for this test case. However, you should tone down your interpretation a bit, since with the incorrect spatial patterns you will get plausible answers, but for the wrong reasons. Last, I am not sure what the paragraph on pp.31/32 is good for. The conclusion that transport models need to be improved particularly during stable stratification is obvious.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 20855, 2011.