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Biogeosciences Discussions

Interactive comment on "North American CO_2 exchange: intercomparison of modeled estimates with results from a fine-scale atmospheric inversion" *by* S. M. Gourdji et al.

Anonymous Referee #3

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Review on the manuscript titled 'North American CO2 exchange: intercomparison of modeled estimates with results from a fine-scale atmospheric inversion', submitted for publication in BioGeoSciences by Gourdji et al.

This study presents the results of a geostatistical inverse modeling approach to constrain the carbon budget of North America at high spatial resolution (1x1 deg) for the year 2004. Findings are presented at temporal scales ranging from monthly to annual, and at spatial scales ranging from continental to regional (selected biomes/climate zones). The new results are put into perspective using several existing inverse modeling studies and bottom-up biosphere process models as references, discussing differ-

ences both qualitatively and quantitatively. Finally, the authors point out the importance of accurate data for incoming boundary conditions for CO2 in regional inverse modeling studies by comparing 2 different data sources.

Overall, this manuscript is well written and well structured, with methods and results described in a plausible way. Assuming the main objective of this paper is to demonstrate the potential of high-resolution geostatistical inversions at the regional scale, the authors generally succeed in demonstrating the potential of their method, and how it compares to existing carbon budget estimates for the same model domain. However, they place too much confidence in parts of their results, and some of their interpretations on model-model intercomparison are not supported by the findings. Also, they fail to explain what causes the large differences between results using different boundary conditions for CO2. Moreover, the text needs to be shortened significantly to improve readability. Finally, posterior uncertainty estimates are required, which I believe should be doable with the given modeling framework.

Summarizing, I believe that this manuscript provides novel findings that should be of interest to the readers of BioGeoSciences. The overall quality of the study is good, with just minor flaws (listed below) that should be straightforward for the authors to improve. My recommendation is therefore to accept this manuscript for publication after minor revisions.

MAJOR COMMENTS

First of all, this manuscript needs to be streamlined to improve readability. I understand that the authors prefer to describe ALL their methods in detail, and present ALL their results, but this carries the risk of burying the new and important material with less important stuff. I therefore recommend shortening this paper by at least 25%, including all sections. I added some suggestions for sections to reduce/cut further below.

Second, as long as the authors do not use any datasets to evaluate the quality of their results, this is 'only' a model-model comparison, and all interpretations and conclusions should take this fact into account. Since there is no reference data available to validate spatial carbon budgets at the relevant scale, this approach is totally acceptable, and overall I acknowledge the efforts to demonstrate how their results compare against numerous (model) references. However, this approach implies certain limitations in the result interpretation, since you cannot truly evaluate which model is right and which is wrong in case their results differ. There are many passages in the text where the authors use such qualitative evaluations, and most of these statements need to be toned down since they are not supported by the data. See details below.

Third, one thing that puzzled me after finishing to read the manuscript is the rather unbalanced treatment of the boundary conditions issue. In great length, the authors discuss the role of prior settings, spatial and temporal flux patterns and how they relate to the reference models, only to state at the end that the impact of all these factors is dwarfed by the choice of the incoming boundary conditions for atmospheric CO2. They test 2 different options, with significantly different results, but do neither explain what causes these huge differences, nor do they give any indication which one may be the better choice. Consequentially, if the objective of this study was to add a new (geostatistical) number to the existing modeling results to constrain the North American carbon budget, the current manuscript version fails to deliver an answer. Since this issue is obviously so important, the authors need to discuss it in more detail, and they need to provide a guideline which version they favor and why.

Finally, it is hard to put the quantitative results into perspective without posterior uncertainty estimates. These uncertainty ranges need to be provided to allow the reader to evaluate the quality of the findings

RECOMMENDATIONS TO REDUCE MANUSCRIPT LENGTH p. 6777 (abstract), ll. 1-10

p. 6780 (introduction), ll. 1-7

p. 6782f (methods, GIM): Since all of this has been described in detail elsewhere, this

would be a good place to shorten and refer to other publications for more details

p. 6786, ll. 15-23

p.6797, first paragraph in Section 3.2.1: This is a repetition of the figure caption, and should be deleted in any case. Also applies to the relevant sections describing figures 4-6.

p.6799, Section 3.2.2: Overall, this is much too long. Should be reduced by ~50% in length, removing much of the detail on specific BU models and ecozones.

p.6804ff, Section 3.3.1: I do not think this section is necessary at all. You already discussed differences in fine scale spatial patterns on the monthly basis, and there are no new insights here.

MINOR COMMENTS p. 6780f, last paragraph of introduction: this should be aligned better with the abstract. For example, you fail to mention the boundary condition CO2

p.6781, Section 2.1: You should clearly state at this point what data year you are going to work on

p.6784, Section 2.3.1: Since the authors raise the issue themselves, they should try harder to explain why they base their study on this relatively data-poor year. Especially for hi-res inversions, more towers should make a big difference.

p.6786f, last paragraph of Section 2.3.3: this last paragraph is confusing - I got the message after reading it several times, but you may think about rewording to clarify what kind of aggregation and model comparison you are talking about.

p.6787, Section 2.4: not clear how temporal covariances in Q were treated in the end in this study. The specific paragraph needs clarification.

p/6787, l.24 (and throughout the text): I do not think 'biome' is the right term for your selection of sub-regions. You are talking about a climate zone that is dominated by a certain plant functional type, which are in no way homogeneous biomes. An alternative suggestion could be ‘ecozone’, or something like that.

p.6788, Section 2.5.1: I think this needs more information for readers who are not familiar with the ’model of the trend’ approach. You may want to give an example, e.g. how correlation with temperature may link your flux fields to seasonal cycles, etc.

p.6789, ll. 6ff: related to the previous comment, to allow a correct interpretation of your results, you need to provide more detail on how exactly your auxiliary variables are used to produce the final flux fields. In particular, you need to explain how you can set up an inversion with just this one driver that can obviously only explain parts of the fluxes, as compared to the more comprehensive use of a set of NARR variables below.

p.6789, ll. 15ff: Add spatial resolution of the NARR dataset

p.6790, Section 2.5.2: please provide spatial resolution for VULCAN and datasets used in the other regions

p.6791, Section 2.6: You should include some general statements here on the value of model-model intercomparisons, and the interpretation of differences in the results. For example, you should mention that, in principle, even good agreement with other models wouldn’t mean that this model works well.

p.6793, Section 3.1.1: These findings need some more clarification, parts of which may be covered by some more background info on the model of the trend concept. I do not fully agree that a beta of 1 is the desired outcome here in any case. What about some incidental spatiotemporal correlation between anthropogenic and biogenic flux fields? In that case, wouldn’t the fossil fuel fluxes be scaled to cover parts of the biosphere signal?

p.6794, ll. 20ff: I have some problems with this part of the X interpretation. specific humidity should be a driver for GPP rather than for RH. The way you present the role of specific humidity makes it sound more like an artifact - if you’ve got precipitation at different timescales included, you should have the soil moisture well covered for RH.

So adding a temperature measure should do a better job to complete the picture for RH.

p.6795, ll. 22ff: The last section of this paragraph is confusing. Explain better!

p.6797, ll. 5ff: I do not agree with this statement! In both cases, there could be the same type of systematic error that causes biases, so better agreement between classes of models doesn’t necessarily mean that these models are more accurate.

p.6798, ll. 7ff: You should mention seasonal biases in inverse modeling results here as well. For example, what about the role of boundary layer height dynamics across seasonal timescales?

p.6799, ll.24ff: these findings may be artifacts - they depend a lot on the setup of the aggregation regions, and how they correspond to spatial patterns in the NARR variables.

p.6801, ll. 12ff: I think this is an over-simplification of the whole matter - the results depend on the balance between Q and R matrix settings - if too much confidence is placed on Q, it will always bias the results, or rather influence it strongly.

p.6802, ll. 22ff: This is the kind of statement that isn’t supported by your results, since you don’t have reference data to prove which model is ‘right’. there’s no reason to believe that the inversion works better than any other model, as long as you prove it with data!

p.6803, ll. 4ff: Same as above

p.6803, ll. 10ff: some of these conclusions need to be put into perspective. #1 is largely dependent on the setup of Q, so in this form it isn’t generally valid. #4 should be toned down, noting that the inversions might be as wrong as the bottom-up models.

p.6803f, first paragraph of Section 3.3: maybe you should note here that the annual fluxes, though subject to large errors, are the one number that people want to see! So even though they may be uncertain, this is the one aspect that should deserve most attention for future improvements!

p.6805, ll. 14ff: please clarify how exactly you handled the fossil fuel fluxes for the bottom-up models.

p.6806f, ll. 27ff: not sure if this discussion is helpful at all!

p.6807, conclusions section: As mentioned above, many of your statements here should be toned down since your results do not support qualitative statements on specific model performances. Particularly the last paragraph on p.6809 is largely speculative!

p.6810, ll. 3ff: I don't think you can use the observed differences between Simple and NARR to conclude that grid scale inversions are impossible to date. These differences should be obvious, given the database differences between both. The main problem is rather the availability of atmospheric observations.

p.6829, Figure 3: The color scales should be uniform for all figures

p.6831, Figure 5: explain which boundary condition is used for which row of figures!

Interactive comment on Biogeosciences Discuss., 8, 6775, 2011.