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Biogeosciences Discussions

Interactive comment on "A regional high-resolution carbon flux inversion of North America for 2004" *by* A. E. Schuh et al.

Anonymous Referee #1

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This paper describes the design and application of a new system to estimate the CO2 balance of the terrestrial biosphere of North America from observations of atmospheric CO2 mole fractions. The system combines many state-of-the-art components including the SIB biosphere model, the RAMS meso-scale atmospheric model, a Lagrangian particle model, and a Kalman filter optimization scheme. Novel in this approach is the separate estimation of ecosystem respiration and photosynthesis for each grid box of the domain. The authors carefully assess the sensitivity of their result to some of the typical inversion related assumptions. This leads to an interesting new way to assess a component of spread around the mean flux results using a jack-knifing approach with the observational dataset. The amount of work done by these authors is impressive and this paper is worth attention from the wider community of readers with an interest in surface CO2 balances. I recommend publication in Biogeosciences, but only after

the following points are addressed thoroughly.

An important point of attention is that from the material presented, I found it hard to judge how well this system really works and what robust features of the North American carbon balance it can detect. This is unfortunate because, especially in the opening paragraphs, previous studies are described with important shortcomings while this paper is touted to overcome them. As an example, the abstract mentions the limitations of optimizing with a coarse biome-dependent covariance structure compared to the high resolution biome independent approach taken here. But the paper does not indicate if there really are extra degrees of freedom given the large spatial correlations imposed, how these extra degrees of freedom help the final solution, and if respiration and photosynthesis bias factors are independently retrieved in the posterior solution. Are the mentioned "subtle differences" between east and west coast forested regions now resolved? I ask you to devote a new section to such questions, in which also flux results are post-aggregated to carbon relevant sub-continental areas (such as biomes). These can then be compared to inventories, other models such as carbontracker, and to the biome specific SOCCR results.

Also, I would like to see more information on the obtained match to the observations to assess the realism of the inversion: are CO2 mole fraction residuals indeed Gaussian and is the balance between assumed uncertainty (fluxes+data) and attained skill good (chi-squared)? Are seasonal biases in simulated mole fractions left after optimization of the bias factors? Does the separate optimization of GPP and R match the full diurnal cycle of observed CO2 mole fractions and observed CO2 flux, or only the daytime when observations were introduced? This is a second new analysis/paragraph that I ask the authors to write.

Since the paper is quite lengthy already, space for these extra analyses should be created by removing some redundant sections (as indicated below) and by taking another critical look at the sometimes very long, interjected, sentences used. In addition to being hard to read, they also gave me the impression of being lectured sometimes on topics that could be assumed familiar to the readers. I realize this is a matter of personal style though and I do not mean to offend by suggesting this, as the paper is generally very well written.

In addition to the points above, which I think are critical before consideration for publication in Biogeosciences, I have listed detailed questions and suggestions below.

Abstract:

line 6: As far as I can tell there is only one inversion based on biome-similar large regions (carbotracker), unless you count the CCDAS from Rayner et al., (2005). If you agree, either make this statement explicitly, or reword the sentence to be more general.
line 8: The example of east and west coast forests is an extreme example of when the biome-approach breaks down and seems specifically chosen to make readers doubt the logic behind such a choice. The authors know of course that a similar extreme example can be chosen for other approaches (such as isotropic distance correlations). Since I feel that this paper presents an alternative to previous approaches and not clearly (unless more evidence is given) an improvement, I would ask the authors to reconsider this line of reasoning in the abstract, and perhaps add a more balanced description of the differences between the assumptions in this paper vs earlier work in the main text.
line 15: "...provide insights...", please be specific and quantitative. What insights are you referring to? List them, including a number describing the effect.
line 29: "... interesting hypotheses..." again be specific: What do you hypothesize after this study? What can the reader expect to learn?

p10198, line 15: I believe this statement is not true. Very few current inversions use coarseness of the estimated fluxes to save computational power. Rather, it is an explicit way to regularize the problem given a small number of observations.

p10199, 1st paragraph: The discussion of grid box numbers versus biome regions is incomplete without some statement of degrees of freedom. For instance, NWP models use millions of unknowns and operate at 20x20 km global grids, but the number of degrees of freedom in the problem is much smaller because of the detailed covariance structure. Similarly, an inversion using a grid of 200km with an isotropic distance correlation of 1000km has 18 degrees of freedom. Not 540 (=30x18). This should be mentioned here already, and later in the paper (methods) explicitly calculated for this problem. I already mentioned this in the first review round, but I don't think carbontracker has only 17 ecosystems represented. The Peters et al (2007) paper gives the number 25. This suggests that your system (2x18 degrees of freedom, minus the propagated part of the covariances) is not so different from other inversions.
p10199, line 11: What is simple about your filter, and how does that allow you to work with all portions (what are these?) of your inversion. Be explicit.
p10200, line 18: "estimate true fluxes of...", you mean 'measure' I presume?
p10202, line 14:"global biosphere-transport model", you mean a coupled terrestrial biosphere and atmospheric transport model?
p10203, 2nd paragraph: To my opinion, this section is long and unnecessary and could be removed to create space for the requested analyses.
p10204, line 1: "... which provides an estimate": You mean to say that the carbotracker optimized CO2 concentration field includes the effect of sources and sinks outside your inversion domain. It now reads as though carbotracker gives you source/sink estimates.
p10204, line 6: "half-hourly", bring to start of sentence, it now reads as though someone sent you observations every half an hour.
p10204, line 12: "low quality modeled measurements" please rephrase to state more clearly what you mean.
p10204, line 14:"diurnally influenced CO2" please rephrase more exactly.
p10204, line 15-19: This explanation is very unsatisfying. What "kind of systematic late venting" do you mean and what causes it? Is there some special boundary layer

dynamics going on at this tower? If you do not know the cause, then simply state that you are using a shifted time interval without further reference to this unknown phenomenon.
p10205, top: Did you assume four independent observations each day at each site? Then mention that there is likely significant auto-correlation in these observations, that they do not inform independently on your biases, and that the 5.5 ppm should be scaled by a factor of sqrt(4) to compare to other typical inversion setups.
p10205, line 18: "inversion" please replace by "biosphere model" or "SIB3".
p10205, Section 2.4: The paper would be accessible by a larger community of data assimilation experts if the recommendations for notation of Ide et al (1999) were followed. Please consider this.
p10206, eq 4: Can you please state these assumptions in words as well.
p10208, eq 10: This would be a good place to discuss the degrees of freedom in your system based on the spatially explicit correlations in your grand prior.
p10208, line 15: I do not understand your explanation of the parameter alpha. It seems like an extra control (in addition to H0) on the correlation, but I couldn't find further reference to it in the paper.
p10209, 1st par: This first section is too much lecturing for my liking, please consider deleting it as all the arguments can be found elsewhere again.
p10209, line 12: The statement that regional inversions have been shown to be very sensitive to inflow contrasts with you earlier statement (p10199,l16) suggesting that you are the first to investigate this. Please fix this.
p10209, line 20: Is the number of alternatives used in the jack-knifing procedure 100? I read later on that the results were based on 45 inversions?? Please clarify.
p10210, line 19: Since the matrices are unitless and the sigmas carry the true values, please add units to the quantities here.
p10211, line 12: This is the only statement on posterior correlations and it suggests that the bias parameters were not at all independently retrieved. Hence my request to discuss this in much more detail.

p10211, last par: I could not figure out the relation of this section to that on page 10209. Are you describing the same procedure again or is this a different test? I'm sure this is due to my lack of understanding, but likely other readers might also not follow the story here.
p10212, line 4: The suggestion that the posterior error is too small is not really relevant to me: this number is so strongly method dependent that a comparison to Gurney and Peters does not say much. What is much more important (coming back to my main points above) is that the posterior errors are in good balance with the obtained skill (CO2 residuals, flux residuals). If a comparison to other inversions is presented it would perhaps be more informative to know the uncertainty reduction.
p10212, line 11: I assume you summed the posterior covariances of your filter, not just the variances?
p10212, line 23: This paragraph again repeats earlier statements and could be removed or shortened.
p10215, line 6: Did you also experiment with alpha0?
p10215, section 3.4: I really enjoyed reading this section, and the analysis presented. The idea to use some performance weighted measure of multiple inverse realizations is interesting and could be mentioned in the abstract as novel.
p10216, line 26: A third explanation for the prominence of prior patterns in posterior flux fields is that the prior was quite good to begin with. After all, the size of the adjustments is as large as the flux itself in some locations suggesting that priors were not too tight, and data helped change fluxes. Whether your system was too rigid or not can of course be diagnosed from the filter statistics.
p10217, section 3.6: The promised comparison to Ameriflux data now only entails a figure that shows improvements at ARM. What about the rest of the dozens of sites in Ameriflux? To pre-empt (other peoples') idea that you're cherry-picking I suggest to include a table or figure showing statistics for as many sites as possible in the network. Without this, your promise of a comparison to Ameriflux level-4 data is overstated and should be revised.

p10218, line 3: remove "approximately", your numbers are exact.
p10218, line 5: the carbontracker website currently shows a number of -0.69±0.51 PgC/yr for 2004 while it encompasses a larger area than your domain. Did you match up the domains to get to 0.9PgC/yr or is this simply a wrong number quoted? Or has carbontracker revised its numbers down since the Peters et al (2007) paper and if so, why?
p10219, line 13: "given its late diurnal venting", what was first speculation has now become a given, but I still object to this without some more detailed explanation.
p10219, line 18: The answer whether there indeed is a correlation to cause this behavior is in your covariance matrix, please investigate this.
p10220, last paragraph: your discussion of crop harvest removal and its relation to the estimated NEE is in contrast with the Peters et al (2007) notion that a sink is seen but a source not. Please explicitly state this contrast.
p10221, line 10: "sources" should be "forests" or "ecosystems"?
p10222, line 1: I'm not sure I understand your low NEE resulting from a lack of constraints oh high GPP regions. The Intex data was not shown here but if it really holds the key to some problems in your estimated NEE it should be shown and discussed.
p10222, line 15: A 30

Figures:

Figure 1 is to me quite uninformative and could be removed in favor of some more substantial results as suggested by my first two points. The IGBP soil class map can then be referenced through a URL or in supplementary material.
Figure 2 could follow this recipe.
Figure 3: I would like to see a histogram of these differences, preferably by season. Labels need to be added with mean differences and the prescribed model-data mismatch.
Figure 4: 100 inversions? Sorry for my confusion.

Figure 6: Can you add histograms of the differences, or seasonal/monthly means? The systematic differences peaking at 10 ppm are large and support my idea that the flux differences they induce are large, not small.
Figure 10: The adjustments in both systems do not look ecoregion shaped, or isotropic distance shaped liked the covariance matrices suggest. Can such patterns be seen in the beta-factors themselves, but they are wiped out by the flux patterns in RE and GPP?
Figure 11: Could you make these figures wider than tall instead of taller than wide?
Figure 12: This could follow figure 1 and 2 to make room for my requested additions.

Interactive comment on Biogeosciences Discuss., 6, 10195, 2009.