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Biogeosciences Discussions

Interactive comment on "Optimization of the seasonal cycles of simulated CO_2 flux by fitting simulated atmospheric CO_2 to observed vertical profiles" *by* Y. Nakatsuka and S. Maksyutov

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Authors are thankful for referees' comments that pointed out several deficiencies in presenting a material. Following the comments we prepared several changes in the final version of the manuscript, to address basic presentation problems such as a need for more clear statements on:

- what was done before (introduction modified)

- which CASA parameters are optimized (added description to abstracts and text)

- whether the model vertical mixing is important for optimization or not, (discussed in summary and section 3.3)

- what is the main purpose of this study (discussed in summary).

(1) In particular, we rewrote an abstract:

MODIFIED:

An inverse of a combination of atmospheric transport and flux models was used to optimize the Carnegie-Ames-Stanford Approach (CASA) terrestrial ecosystem model properties such as light use efficiency and temperature dependence of the heterotrophic respiration separately for each vegetation type. The method employed in the present study is based on minimizing the differences between the simulated and observed seasonal cycles of CO2 concentrations. In order to compensate for possible vertical mixing biases in a transport model we use airborne observations of CO2 vertical profile aggregated to a partial column instead of surface observations used predominantly in other parameter optimization studies. Effect of the vertical mixing on optimized net ecosystem production (NEP) was evaluated by carrying out 2 sets of inverse calculations: one with partial-column concentration data from 15 locations and another with near-surface CO2 concentration data from the same locations. We confirmed that the simulated growing season net flux (GSNF) and net primary productivity (NPP) are about 14% higher for northern extra-tropical land when optimized with partial column data as compared to the case with near-surface data.

(2) Introduction was changed:

ORIGINAL (p 5936.5-17)

To our knowledge, these studies which used the observed CO2 concentrations to optimize parameters of terrestrial ecosystem model relied upon available CO2 data which are dominated by surface level measurements. However, recent studies have revealed that the accuracies of vertical mixing scheme in transport models vary quite dramatically from a model to model. For example, Stephens et al. (2007) revealed that the

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significant number of transport models compared in the TransCom-3 study (Gurney et al. 2002) do not have sufficient vertical mixing. As a result, exclusive use of CO2 concentration data in boundary layer in the atmospheric inversions can bias the estimated fluxes. Similarly, Yang et al. (2007) used ground-based FTS and aircraft measurements to reveal the weak vertical mixing in a number of the transport models of TransCom-3 both between PBL and free troposphere and within free troposphere, and implied that the use of CO2 column data is more relevant for the reliable optimization of terrestrial ecosystem models.

MODIFIED

To our knowledge, these studies which used the observed CO2 concentrations to optimize parameters of terrestrial ecosystem model relied upon available CO2 data which are dominated by surface level measurements. However, more recent studies have revealed that the vertical mixing biases in transport models result in bias in the optimized fluxes. For example, Stephens et al. (2007) revealed that a number of transport models compared in the TransCom-3 study (Gurney et al. 2002) do have vertical mixing biases which were revealed by comparing optimized concentration fields with observed vertical profiles not used in the inversion. Models with both too step and too shallow vertical gradients were present. Similarly, Yang et al. (2007) used ground-based FTS and aircraft measurements to suggest that use of CO2 concentration data in boundary layer in the atmospheric inversions can bias the estimated fluxes, and pointed to a weak vertical mixing bias on average in a number of the transport models of TransCom-3. They implied that the use of CO2 column data could be more relevant for the reliable optimization of terrestrial ecosystem models. Mean weak mixing bias in TransCom-3 models by (Gurney et al. 2002) can be attributed to using mostly offline models with missing or simplified physical process parameterizations such as shallow and penetrative cloud convection and boundary layer turbulence. Some of more recent transport models, such as compared by Law et al. (2008) involve complete online transport schemes and are expected to do better in vertical mixing.

(3) Section 3.3 was modified

ORIGINAL (5945.4-7)

Furthermore, since the method described in this paper can correct the seasonality of CASA NEP without being much affected by a scheme of vertical mixing in a transport model, it can be used to prepare flux fields of CO2 which can be used as a reference for tuning a scheme of vertical mixing in a transport model.

MODIFIED

Furthermore, since the method described in this paper can correct the seasonality of CASA NEP without being much affected by a scheme of vertical mixing in a transport model, it can be used to prepare flux fields of CO2 which can be used as a reference for tuning vertical mixing processes in a transport model, and could be complementary to other widely used vertical mixing tracers such as radon.

(4) Summary was modified

ORIGINAL (5945.24-26)

Better fit to the column average concentration can potentially improve a fit of the forward model simulations to the observations of the CO2 by ground based and space based instruments.

MODIFIED

Better fit to the partial column average concentration can potentially improve a fit of the forward model simulations to the observations of the CO2 by ground based and space based remote sensing instruments. Transport model tuning is left beyond a scope of this study because the main purpose of producing correct NEP seasonality is achieved by using partial CO2 column observations, although it would be even more efficient to simultaneously tune transport and surface fluxes, that would allow including surface-only observation sites data consistently with vertical profiles.

(5) We made other changes in response to the comments by referee 1.

Comment: “It would be helpful to state more clearly the findings of previous research in the introduction (especially p. 5935 and 5936) to, critically, better describe how this study represents an improvement to previous work.”

Edits: introduction was modified (see above)

Comment: “The change that would improve the paper the most would be a full discussion of improvements to vertical mixing models given that the NIES model used was co-developed by the co-author. See e.g. page 5944: it says mostly that the mixing model is insufficient; more valuable would be suggestions for how this can be improved. The readers have been convinced that vertical mixing models represent a substantial problem for quantifying the carbon cycle (e.g. Stephens et al. 2007), but a proactive discussion would detail the necessary improvements. Why is winter northern mid-latitude vertical mixing a particular problem? Erroneous sensible heat flux estimates for vegetation covering snow?

Reply: we added short discussion in the introduction, however it is important to stress that we are not trying to fix a vertical mixing in the model in this particular study, because by using lower and mid-tropospheric partial column averages we hope to overcome lower tropospheric mixing biases when surface flux seasonality is concerned. Referees are right that we are in good position to fix a vertical mixing as well, but for the purpose of this study which is a terrestrial biospheric flux model optimization it is not compulsory with a proposed approach.

Comment: “Methods: A brief CASA description would be forthcoming so as not to continuously refer to future sections. Include a couple of sentences on the model and what it does.

Reply: We change order of sections 2.1 (inverse modeling) and 2.2 (CASA).

Comment: “Is CASA really designed to have zero NEP on the grid-cell basis (p. 5942, 21)”

Reply: Following a number of CASA-based studies by J.Randerson and coauthors we use climatological temperature, precipitation and NDVI to spin-up the model for about 2000 years (with each parameters set) so that the annual mean NEP approaches to zero. We checked that the mean NEP signal in transport model is small and insignificant to disturb the seasonality.

Comment: “The formulation of equations 6 and 7 and equations like it have always concerned me; they don’t follow Leibig’s Law and there is little mechanistic reason to assume, for example, that light use efficiency is sensitive to the product of temperature and water stress.”

Reply: The use of notation such as Ts (g, t) and W(g, t) could be misleading and we replaced those by Ft (g, t) Fw(g, t) . . . “where factors Ft and Fw are dependent on temperature and soil moisture and account for stresses induced by temperature and soil water availability”. As said in CASA model description papers, those factors reach maximum value of 1.0 at optimal temperature and sufficient soil water content. We checked CASA Matlab code and found no inconsistencies with model papers.

Comment: “Is subtracting 30 the correct formulation in equation 8? (Depends on the reference temperature)”

Reply: Model code uses value of 30. Randerson et al GBC 1997 refer to Parton et al 1995 for base decomposition rates at 30 degrees, so it looks reasonable.

Comment: “I like the approach to optimize on Emax given that it has (likely) been assumed constant in previous CASA studies, but there is far more literature on this important issue (e.g.(Zhu et al. 2005)) and there are more models than CASA. Please expand the scope of the discussion on Emax to enhance its applicability of studies using CASA and/or other models.”

Reply: We did not go for comparison with other estimates of Emax, because we don’t

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feel confident about discussing Emax values and comparing to flux observations. Our Emax are not independent from Q10 and show strong covariance with Q10 in the inversion output, so we are more confident in performance of the combination of Q10 and Emax than in each separate value. Before comparing to other Emax estimates we need to add stronger independent constraint on Q10.

(6) Response to comments by referee 2.

Referee suggested to rewrite and resubmit the manuscript due to severe deficiencies in the present version, so we got impression that the purpose and essence of our study was not fully understandable in that version and made several modifications outlined above.

Comment: “Foremost, it lacks a rigorous description of the methodology, and the conclusions are not supported by the results. Specifically, it remained unclear to me what parameters were optimized, and how the ingestion of the observations was linked to improving the vertical mixing in the model. To my understanding, no modifications have been done to the model to improve the mixing scheme. “

Reply: We admit that we failed to make clear impression that our intention was to optimize flux seasonality without going through tuning vertical mixing in the model. In the modified version we shifted focus from discussion of vertical mixing effects to producing optimized flux which is independent of the mixing biases even with biased transport model. Main point is that if we use partial columns extending to mid and upper troposphere we get significant reduction of the optimized flux sensitivity to possible transport model biases. The abstract, introduction, discussion and summary were modified to reflect our position.

Comment: ”The authors should also make a more careful review of the available literature on inverse modeling.”

Reply: Although there could be some incompleteness in historical prospective, we

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hope our paper cites most relevant papers which are directly related to the subject of using inverse modeling for optimizing seasonality of the terrestrial biosphere fluxes by most active groups (e.g. I. Fung, J. Randerson, P. Rayner and M. Scholze), so it covers the subject although somewhat briefly.

Comment:" The paper would definitely benefit from a thorough reorganization of its sections. I cannot see how the conclusions on the vertical mixing scheme are connected to the results. I recommend rejecting the current version of the manuscript for publication in Biogeosciences, while encouraging the authors to submit a more carefully prepared and complete manuscript."

Reply: We reordered sections on model components, and made changes to discussion on vertical mixing. We hope that a modified manuscript will not make confusing impression as in first version and will be acceptable for publication.

For the authors

Shamil Maksyutov

Interactive comment on Biogeosciences Discuss., 6, 5933, 2009.