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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of various observing systems for the global monitoring of CO_2 surface fluxes" *by* K. Hungershoefer et al.

K. Hungershoefer et al.

katja.hungershoefer@lsce.ipsl.fr

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We would like to thank the reviewer for careful and thorough reading of the manuscript and for the thoughtful comments and constructive suggestions, which help to improve the quality of this manuscript. Our response follows:

1. As a general comment, a lot of important details are not mentioned explicitly (for instance for satellite measurements why are CO2 fluxes inferred from small variations in the column averaged mixing ratio, it is never mentioned before that satellites only measure a CO2 column mixing ratio), I would think the manuscript could be improved by explaining these details more explicitly. Reply: We modified the text on page 18565 and included some additional information. In addition, we think that the other suggestions (see e.g. comment 4) also help to improve the understanding.

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Interactive Discussion

Discussion Paper

Lines 6-8, old: Using a flux inversion or so called top-down approach, the surface fluxes are derived from the spatial and temporal concentration measurements.

Lines 6-8, new: The spatial and temporal gradients of these CO2 concentrations are directly related to the surface carbon fluxes. Through a so-called top-down approach, it is then possible to estimate the spatial and temporal distribution of the fluxes.

Lines 12-15, old: Satellite measurements provide a good spatial coverage but they are challenging because the information about the CO2 sinks and sources located at the Earth's surface must be obtained form small variations in the column averaged mixing ratio.

Lines 12-15, new: Satellites measure the column averaged CO2 mixing ratio with a good spatial coverage but they are challenging because the information about the CO2 sinks and sources located at the Earth's surface must be obtained from small variations in the total column mixing ratio using the top-down approach mentioned above.

2. Page 18563, line 1: "rising" instead of "raising" Reply: Corrected.

3. Page 18564, line 12/13: This is currently debated, see for instance Knorr, GRL, 2010. Reply: We modified the text as follows and included a reference to Knorr: Is the airborne fraction of anthropogenic COemissions increasing?, GRL, 2009:

Old text: In addition, the fraction of CO2 emissions that remains in the atmosphere has increased (Le Quéré et al., 2009). One reason for this is the rapid growth in fossil fuel emissions since 2000 due to the recent growth of the world economies. Another reason is a decline in the efficiency of the natural sinks in absorbing anthropogenic emissions (Canadell et al., 2007).

New text: There are claims that the fraction of CO2 emissions that remains in the atmosphere has recently increased, partly because of a decline in the efficiency of the natural sinks (Canadell et al., 2007; Le Quéré et al., 2009), although there are still significant uncertainties both on the fossil fuel emissions and the ocean and land net sinks (Knorr, 2009).

4. Page 18565, line 2-4: You mentioned FLUXNET here but you do not use these data in your study at all and you also do not explain how direct carbon flux measurements could be used in inversions. Reply: Since we do not use FLUXNET measurements, we did not feel the need to explain how direct measurements are used. We've added a reference where details can be found. The text was slightly modified: "Direct carbon flux measurements coordinated by the FLUXNET project are performed at more than 400 stations in the world (Baldocchi, 2008) and have been directly used in inversions (e.g. Santaren et al., 2007)."

Reference: Santaren, D., P. Peylin, N. Viovy, and P. Ciais (2007), Optimizing a process-based ecosystem model with eddy-covariance flux measurements: A pine forest in southern France, Global Biogeochem.Cycles, 21, GB2013, doi:10.1029/2006GB002834.

5. Page 18565/18566: The potential of the A-SCOPE mission has already been assessed in another study (Kaminski et al, Tellus, 2010), which should be mentioned here as well. Reply: A sentence mentioning the study of Kaminski et al. (2010) was added:

New text: Kaminski et al. (2010) investigated the benefit of A-SCOPE observations in a carbon cycle data assimilation system.

6. Page 18568, last line and page 18569, first line: Could you explain the justification for using an exponentially decreasing error correlation with a decay time of four weeks? How sensitive are the results to this assumptions? Reply: This "assumption" is actually based on the results of Chevallier et al. (2006) (their Figure 6). We added the reference.

New text: At a weekly resolution, errors on any prior fluxes are likely to be correlated in time. A comparison of ORCHIDEE simulated carbon fluxes to ecosystem flux measurements at many stations (Chevallier et al., 2006) showed a strong temporal auto-correlation of the residuals (model minus observed fluxes) with significant values during one month. Based on this study, we thus used an exponentially decreasing error correlation with a decay time of four weeks.

7. Page 18570, last line: You should explain why night time measurements are not useable. Reply: This derives from the previous sentence. To make it clearer, we added “as a consequence”: "In addition, during the night and early morning, the low atmosphere is generally very stable so that surface fluxes are trapped in the first meters above ground and the measurements are representative of a very small area only. As a consequence nighttime measurements are not useable by current global scale inversions."

8. Page 18581, line 7-16: I think it would be good to display the extended ground-based networks in a figure. Reply: Two plots showing the geographical location of the hypothetical surface networks were added to Figure 2.

9. Page 18572, line 1: Why do you assume that there are large error correlations among the errors? Reply: Errors in the satellite estimates are due in particular to wrong assumptions on the temperature and CO_2 vertical profiles, presence of aerosols, surface reflectance and radiative transfer. All these errors are highly correlated for nearby samples. We made it clearer changing the sentence to: "These observations cannot be considered as independent in the inversion system because of the large correlations among their errors resulting from geophysical assumptions and among the errors of the transport model that simulates them."

10. Page 18572, line 3: How do you define "best" measurement? How do you know that one particular "pseudo" measurement is better than the others? How sensitive are you results against this choice of the best measurement? Reply: For each measurement, we compute an uncertainty as discussed in section 3.3. The “best” measurement is the one with the lowest uncertainty. We made that clear by changing the sentence to "For each satellite orbit, we kept only the lowest uncertainty observation (see Section

3.3) of each model grid box,. . ."

11. Page 18573, line 23-25: I don't think that the measurement error should contain simulation errors as well. Usually, measurement error as referred to as the mismatch between the measured value and the "real" value! Consider the case with the satellite observation and the transport simulation having the same, non-zero error, the result would, however, be a measurement error of zero. Reply: The term 'measurement error' was not appropriate indeed and we replaced it by 'observation error'. The observation error is defined with respect to the assimilation system and therefore combines measurement, model and representativity errors.

12. Page 18575, line 20: Why do you not consider transport model errors? I think this is an important contribution to the overall error assessment. Reply: The issue of transport errors is a key contribution indeed. Its importance made us decide to discuss it separately in the companion paper by Houweling et al. (2010). The text was modified as follows:

Old text: Transport model errors are not considered for the satellite observing systems here. A recent study of Houweling et al. (2010) shows that these model errors are an important factor limiting the accuracy of the determination of CO2 fluxes.

New text: Transport model errors are not considered for the satellite observing systems here. The issue of transport errors is discussed separately in the companion paper by Houweling et al. (2010).

13. Pager 18577, line 21: "analytical" instead of "analytic" Reply: Changed.

14. Section 4.3: The prior errors are missing here. I think it would be good to set the posterior errors in relation to the prior error structure. Reply: We have added in Table 1 the prior annual errors for few critical regions. However, we believe that a new figure would not bring significant information but only overload the paper. The prior flux annual uncertainties are indeed rather large. The prior uncertainties were defined on

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the weekly fields with positive temporal and spatial correlation. For the land, we did not account for the fact that errors during the growing season are likely to be anti-correlated with errors during the non-growing season (i.e., additional plant carbon uptake leads to additional ecosystem respiration). For these reasons, the land prior error budget is rather large (Europe: 89 gC/m2/yr, Siberia: 101 gC/m2/yr] and these large values are necessary to account for the fact that our prior land carbon budget is close to equilibrium (i.e., ecosystem model run at equilibrium).

The following text is added at the beginning of section 4.3: The annual prior uncertainties were defined on the weekly fields with positive temporal and spatial correlation. These prior uncertainties are rather large (e.g. 89 and 10 gC/m2/yr for Europe and Siberia, respectively, see Table 1), because we did not account for the fact that errors during the growing season are likely to be anti-correlated with errors during the non-growing season (i.e., additional plant carbon uptake leads to additional ecosystem respiration) over land.

15. Page 18584, line 21 and in the following: "Representativeness error" sounds awkward; I think the common terminology is "representation error". Reply: Changed.

16. Page 18587, line 24 to Page 18588 line 20: This "Land-Vegetation dynamics" section is a bit misleading. Vegetation dynamics is usually understood as the slowly changing components of the terrestrial biosphere such as mortality, succession, and competition and not processes, which operate on a weekly time scale. In fact, it is not clear to me which processes you actually refer to here. Processes regulating the terrestrial CO_2 exchange fluxes operate on a much higher temporal time scale. They vary on a synoptic time scale. Reply: We agree that the title “Land-Vegetation dynamics” is a bit misleading as we do not discuss slowly changing components which would require detecting long term mean carbon fluxes (i.e., over several decades). We rather focus on short term (synoptic) to seasonal carbon flux variations. The processes that are involved include 1) slow varying response of photosynthesis to weekly/seasonal changes in climate forcing, and 2) response of soil organic matter decomposition to

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variations in soil water content and temperature. For photosynthesis, although the controlling processes operate on shorter time scale (i.e., hourly), the weekly to seasonal variations that can be monitored from space, provide integrated constraints to validate the different processes. We have changed the title of this section to “Land-Vegetation C-fluxes: synoptic to seasonal scales”. We also modified the text describing its content: “Measurements of the carbon fluxes ... over large spatial scale areas, at least for the short (synoptic) to medium (seasonal) scales.” We modify the sentence “The temporal scale .... (threshold).” by : “Although the processes controlling photosynthesis and respiration at the ecosystem level operate at high temporal scale (i.e., hourly) we consider the weekly (target) and monthly (threshold) time scales which are more directly compatible with global remote sensing products.

17. Page 18588, line 25/26: Could you provide a reference for this. Reply: The reference Gurney et al. 2002 (science paper for Transcom) was added.

Reference: K. Gurney, R. M. Law, A. S. Denning, P. J. Rayner, D. Baker, P. Bousquet, L. Bruhwiler, Y. Chen, P. Ciais, S. Fan, I. Y. Fung, M. Gloor, M. Heimann, K. Higuchi, J. John, T. S. Maksyutov, K. Masarie, P. Peylin, M. Prather, B. C. Pak, J. Randerson, J. Sarmiento, S. Taguchi, T. Takahashi, C. Yuen, Towards robust regional estimates of annual mean CO2 sources and sinks, Nature, 415, 2002.

18. Page 18590: Kyoto protocol verification is very demanding indeed. But how about verifying UNFCCC reporting which considers total emissions and not anthropogenic and natural emissions separately. Reply: Although it is not said explicitly in the text, the UNFCCC reporting is addressed by the “Vegetation Feedback to climate change” section.

19. Page 18590, line 25-27: Could you elaborate on what kind of systems your are thinking of here. Reply: Part of this paragraph was rewritten to illustrate the types of information available.

Old text: Additional and complementary information will be needed, in particular to

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better constrain the ocean fluxes or to monitor the anthropogenic emissions as needed in the context of international treaties. Of course there is a wealth of such information available and so a clear outcome of this analysis is the need to build systems that can integrate streams of information with the atmospheric data studied here.

New text: Additional and complementary information will be needed. There is a wealth of such information available, e.g. spatial patterns of vegetation activity (Maignan et al., 2008), patterns of human settlement and energy consumption (Oda and Maksyutov, 2010; Rayner et al., 2010) or oceanic partial pressure CO2 measurements available from the Surface Ocean CO2 Atlas (SOCAT) (IOCCP Report No. 7, 2007). A clear outcome of this analysis is the need to build systems that can integrate such complementary of information with the atmospheric data studied here.

The new references are: IOCCP Report No. 7, Surface Ocean CO2 Variability and Vulnerabilities Workshop, UNESCO Paris, France April 11-14, 2007.

Maignan, F., F.-M. Bréon, C. Bacour, J. Demarty and A. Poirson: Interannual vegetation phenology estimates from global AVHRR measurements: Comparison with in situ data and applications, Remote Sensing of Environment, Volume 112, Issue 2, Pages 496-505, 2008.

Oda, T. and Maksyutov, S.: A very high-resolution global fossil fuel CO2 emission inventory derived using a point source database and satellite observations of nighttime lights, 1980–2007, Atmos. Chem. Phys. Discuss., 10, 16307-16344, doi:10.5194/acpd-10-16307-2010, 2010.

Rayner, P.J., M. R. Raupach, M. Paget, P. Peylin and E. Koffi: A new global gridded dataset of CO2 emissions from fossil fuel combustion: 1: Methodology and evaluation, J. Geophys. Res., accepted Apr 2010.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 18561, 2010.