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Biogeosciences Discussions

Interactive comment on "On observational and modelling strategies targeted at regional carbon exchange over continents" *by* C. Gerbig et al.

C. Gerbig et al.

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First of all we thank Peter Rayner, the reviewer, for the perceptive comments to our paper. There are many comments that seem to indicate that some of our statements were not formulated clearly, and we take this opportunity to try to improve this. The reviewer is quite critical towards the general message of the paper, which we take as an opportunity for an open discussion on some of the points. In the following reply we try to address each of the comments the reviewer made.

Replies to general comments

Reviewer: "The paper also leaves an impression that the task of regional inversion is nearly impossible. ... also, any scientific community needs its balance of brash optimists and prudent pessimists."

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Principally there is no guarantee that the targeted information (regional scale carbon budgets that goes beyond "detecting" only the largest anomalies or the largest errors in the a priori flux fields) can actually be extracted from the existing and planned networks measuring atmospheric CO2. Therefore we think it requires an honest assessment of the information provided by the different network components in combination with the modeling tools. In this context, we regard it as essential to first tackle the largest obstacles first, and next to transport uncertainties we regard the near field dominance for CO2 and the associated strong spatial variability of CO2 discussed in this paper as one if the big obstacles.

Referee: "The fault with the paper is a failure to quantify the current state of the art so that we don't know which problems are more important or more tractable. For this the authors will need to overcome their reliance on their own previous work and draw some inferences from, rather than cataloguing, work like that from Sarrat et al. and Lauvaux et al. (directly relevant to the campaign of this special issue) or from the Denning group at CSU."

We agree that the paper could have attempted to draw some inferences from the recent work in mesoscale forward and inverse modeling of CO2, and we agree to include a more detailed discussion of these papers within the introduction.

The paper by Sarrat et al., 2008 BGD (same special issue as our manuscript) basically shows that when using a high resolution forward modeling combined with a land surface model that includes CO2, atmospheric CO2 during a short term campaign can be better reproduced when using more realistic values for LAI. It clearly shows potential of atmospheric observations at least qualitatively, but it also shows that there is still a lot of work to be done to get down to processes that we don't know yet, or to reducing uncertainty in bottom-up estimates within the domain. Lauvaux et al. 2008 (ACP) attempted an inversion using pseudo data for two tall towers and aircraft transect within the CERES domain, solving for daily fluxes for each 8 km pixel for a 4 day period. The authors find an uncertainty reduction larger than 30% for a region of about

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200km x 100 km, using all measurements from an intensive observational period during CERES 2005 (two towers and 10 aircraft transects), and using an a priori uncertainty of 2gC/day/m2 (about 100% of the daily uptake during the growing season). Although the results were presented quite optimistically, they in fact indicate that there is a strong impact (i.e. strong reduction of uncertainty) mostly confined to the near-field of the observations, which we look into in a systematic way in our paper. In a subsequent paper by Lavaux et al. (submitted to GRL) the method was applied to real data, and it was shown that the fluxes retrieved with the inversion improved the prior fluxes towards the independent flux observations by airborne and ground based eddy covariance. Also the methodological work by Zupanski et al. 2007 on flux optimization shows that when using dense atmospheric networks such as the ring of towers in northern Wisconsin, multiplicative biases in flux components can principally be determined.

In another paper, Lauvaux et al. (this issue), attempted to derive transport uncertainty from ensemble simulations with a mesoscale model (11 ensemble members of the global model were used to generate different mesoscale simulations). The paper targets important shortcomings of inverse modeling, but the approach is still in its infancies. There is indication that there is some potential in deriving error covariances, but variances (i.e. the magnitude of the transport error) could not yet be derived since the dominant source of transport uncertainty is not likely to come from the large scale assimilation, but more likely from mesoscale model error including representation of boundary layer mixing.

In summarizing, these papers indeed show that a) the community has started to look into mesoscale inversions and b) that this is a non-trivial task with mesoscale circulation tending to complicate the interpretation of observations in many cases as we argue in the introduction. So far the regional scale inverse results only allow detecting and reducing gross errors in prior fluxes related to e.g. phenological misrepresentations. The community is still far from quantitatively using the information from continental observations related to long term regional scale budgets of C, and we think it is important

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to honestly assess the reasons for this.

Reviewer: "I also repeat the suggestion to group a set of recommendations at the end of the paper, even better if they suggest which of the problems the authors identify should come top of the list."

Answer: In the conclusion of the manuscript we call for

-specific model-data-fusion approach that combines top-down and bottom-up approaches. In this approach it is vitally important to reduce the errors that are associated with current low-resolution transport models used in the inversions. The suggested way forward is to develop nested modelling systems that optimally take into account the need for high resolution models near observation sites.

- that the near field of towers should receive special attention with additional information provided by flux towers, allowing for atmospheric constraints to have more impact on the large scales inaccessible to direct flux measurements.

- measurements of vertical profiles and/or meteorological measurements allowing the determination of mixing heights are highly recommended at the atmospheric monitoring sites to help reduce the uncertainties in simulated vertical mixing.

We agree that it would be ideal to be able to rank these, but even a simple ranking in terms of which of these contributes most to reducing overall uncertainties they would probably require implementing these suggestions and checking how the approach improved. This clearly goes beyond the scope of our paper.

Replies to specific comments

0.0.1 Near Field Contribution

Reviewer: "the first thing to say about this "near field singularity" is that it isn't new."

Answer: We agree, it is not new; in fact, already the analysis of Kaminski et al. 1996 of the sensitivity of the seasonal cycle in CO2 mixing ratios to seasonal biosphere fluxes

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showed the strong local influence by using the transport adjoint. However, unlike the Bocquet 2005 analysis referred to by the reviewer our intention was by no means to solve the problem of any singularity, but to apply a high resolution representation of transport to the specific problem of biosphere-atmosphere exchange of CO2 with its strong diurnal cycle in vegetated areas. What is new is that the strong diurnal cycle with flux reversion between day and night causes a much stronger near-field effect for CO2 than for other tracers without strong diurnal flux variations.

Referee: "I don't believe that the demonstration given in this paper is a good example of the general problem or that the authors properly interpret the results. "

Answer: We took this example, since it is exactly tailored to the problem of regional scale inverse modeling of CO2.

Referee: "bottom half of P1324 ... The question is which signal?"

Answer: It is quite clear from the text and the equations that the signal is the one caused by biosphere-atmosphere exchange within the model domain. The reviewer obviously addresses not the spatial composition of the signal, but its spectral composition, such as the decomposition into diurnal, seasonal and long term changes. There is no simple translation between the spatial and spectral composition, with far-field corresponding to long-term and the near-field corresponding to short-term changes. What one can say is that near-field contributions can cause high frequency variations, and that large-scale far-field contributions are unlikely to cause high frequency variations. However, it is not true that near-field contributions can not contribute to low frequency variations. This is obvious when time-averaging the contributions from various distances, as we have done in Fig. 2, where the strongest monthly averaged contributions are from the near-field. We simply argue that time averaging does remove the near-field dominance. However, to make the limitation to a modeling domain of a specific size clearer we agree to reformulate the following sections:

P1322 line 18: We added the sentence "Since the model is limited to a regional domain

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(North America), the term "far field" here is limited to distances smaller than 5000 km."

P1332, line 13 (Conclusions): We reformulate the sentence "We show that for a single tall tower during the growing season the fluxes in the nearest 20-60km contribute as much as the fluxes from all other areas combined."

to

"We show that for a single tall tower during the growing season the fluxes in the nearest 20-60km contribute as much to the mixing ratios as the fluxes from all other areas within the model domain combined."

Referee: "This weak advection of the diurnal cycle also is not new, it was demonstrated for a larger-scale model by Law et al., GBC, 2004"

Answer: Law et al., GBC, 2004 showed that an inversion using continental sites benefits from solving for mean daytime fluxes (constant during daylight hours, zero during night) in addition to monthly mean fluxes, while when using only ocean sites the inversion does not benefit. This finding was also not surprising, given that the observations at most ocean sites do not even exhibit a diurnal cycle. However, what we targeted was the behavior of the diurnal cycle in fluxes on observations over the continent, where mixing ratios of CO2 themselves do show a strong diurnal cycle. The coarse horizontal resolution of the transport model used in the Law et al., GBC, 2004 study (5.6° x 2.8° lon x lat, or ~ 600 km x 330 km) did not allow investigating how the diurnal cycle smears out over such short distances as was found in our study. We regard our findings not just an interesting detail of a "refinement" of the Law et al. result, rather it is an important issue that it has major implications for regional scale inverse modeling.

Reviewer: "The results with respiration already show a spreading of influence as the diurnal cycle problem is reduced. Further, if we changed the near-field flux by 10% and did the same with the total far-field flux, the difference in trend would eventually emerge from any spatial offset given by the near-field change."

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Answer: There seems to be a significant misunderstanding. Our intention is to investigate the relative contribution from fluxes within the near-field and the far-field (but still within the model domain) to the CO2 mixing ratio at a measurement location, and the resulting impact from small scale (but not necessarily short-term) bias errors in a priori fluxes. This problem can principally not be solved by simple time averaging, since it is caused by the combination of strong spatial variations of the footprint (near field contributions are always larger than far-field contributions) and potential bias errors in near-field fluxes (our example of a misclassified vegetation cover).

Reviewer: "In summary, I think this problem exists but is overstated here."

Answer: We apply rather simple arithmetic to results from a high resolution transport model coupled to a biosphere model with a diurnal cycle, From this we argue that the strong variability of surface influence and of surface fluxes needs to be represented with minimal bias or it will result in false flux estimates.. We do not think we overstate this problem, rather it is more our impression that this issue of near-field variability has been understated in the literature.

0.0.2 Treatment of aggregation problems

Reviewer: "P1328 starting at line 10 ... In case A the authors speak of adding an extra uncertainty to deal with uncertainties of unresolved inhomogeneities. Do they mean to the data or the near-field fluxes? If the data then this was the recipe described by Kaminski et al. The authors claim that under this approach the inversion will choose to concentrate all its improved information in the near-field. Do the authors have evidence for this?"

Answer: We agree to modify the manuscript by mentioning that the Kaminski et al. 2001 method can likely be used to reduce the impact of this uncertainty by about a factor 2.

With respect to the concentration of the information in the near field, we regard it as

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clear that if one includes an additional uncertainty for the lack of the inversion to represent the near field, this is where at least a certain fraction of the information has to go. We have no other proof for this other than to say the additional uncertainty should be implemented in exactly that way. There might be an argument as to how much information will go into the near field, but the order of magnitude can be derived from the simple fact that at least on diurnal to monthly timescales the mixing ratios are clearly dominated by the near-field fluxes (see fig. 2).

Reviewer: "The point here is that the "intermediate" approach isn't really intermediate and that the key point is not the quality of the prior but the selection, by whatever means, of the available increments.

Answer: We agree to this, and will modify the text accordingly.

Reviewer: "P1320L7 This sentence suggests that regional modelling may be harder than global. This may or may not be true but it isn't the right question. ... High-resolution modeling usually demands limiting the domain for computational reasons. This is another of the sentences that gives the paper its overall negative tone."

Answer: What we intended to say is that regional modeling presents a new challenge compared to global modeling, as a new class of issues arises that deal with mesoscale processes that can safely be ignored at global scale, at least for transport modelling. It is not just a computational issue. We suggest to change the sentence as follows and hope this takes away some of the perceived negativism:

"The challenges in accurately representing atmospheric transport are not simpler, but rather, different at regional scales compared to global transport models, as has also been shown from the mesoscale tracer simulations by Sarrat et al. (this issue) and Ahmadov et al (this issue)."

Reviewer: "P1320 (bottom) I don't know whether the authors are trying to give the impression that MDF is in particular need of correct statistics (e.g. more than a direct

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flux inversion) but if so they need to verify it. Instinctively it looks unlikely."

Answer: We agree that both, MDF and direct flux inversions, need the correct statistics. We wanted to emphasize that, given the number and diversity of data streams entering the optimization in a model-data-fusion system, a correct weighting of each stream highly important.

Reviewer: P1326L5 "The claim that "measurement errors are often treated as constant in time and space" rather misrepresents the efforts of the inversion community. One can argue about "often" but most important inversion studies since Bousquet et al., (Science (2000) have used structured data uncertainties, as did the series of Transcom-3 studies, the data assimilation experiments of Kaminski et al., GBC, (2002) and Rayner et al., GBC (2005), the series of inversions from Rodenbeck and colleagues etc. It suffices for the authors' purposes to say that data uncertainty isn't fully developed in atmospheric inversions without misrepresenting the literature."

Answer: we agree, and will modify the manuscript accordingly.

Reviewer: "P1327L20-25 The statement that aggregation is needed for regularisation is incorrect. The Bayesian methodology is inherently regularised. The source of aggregation error lies in computational constraints on transport model grids not limitations of the Bayesian method."

Answer: The reviewer is correct in that regularization is inherent in the Bayesian methodology, where it is provided by through the use of priors. We suggest to reformulate the section to "A certain spatial and temporal aggregation of the fluxes to be optimized in the inversions is required for computational reasons. This also helps counteracting that the lack of information in the measurements leads to an under-constrained problem."

Reviewer: "P1329L10-15 The authors seem to change their minds within a few lines here. First top-down and bottom-up comparisons are impractical then they can be done

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on certain agreed scales then a full comparison is impossible. The authors don't specify on which side (top-down or bottom-up) the covariance matrices become prohibitive. If the point is that we should use bottom-up models to specify not only the priors but their covariances then we are already in the domain of carbon cycle data assimilation"

Answer: We do not see a change of mind here: we argue that a full comparison is not possible because it is impractical, and that comparisons therefore need to be done on certain agreed scales. A comparison taking into account the full error covariance matrices is prohibitive on both sides, since the same dimensions will be involved in both. Regarding carbon cycle data assimilation: that is exactly what we argue for in P1329L21 ff.

Reviewer: "P1333 (bottom half) The authors do suggest the two alternatives here but we should take note of the long experience of operational data assimilation from the NWP community who usually keep back subsets of data for validation. The authors are correct that the Bayesian framework that underlies the data assimilation approach does allow proper statistical assessment of the results when all data are included but this makes tremendous demands on the various input statistics. Keeping data back, particularly data that doesn't itself have great value as an constraint (such as much campaign-mode data) remains a wise choice."

Answer: We agree with the reviewer, and will modify the statement "However, bringing together the different data streams in one model-data-fusion system, allows in fact for a much better validation of the system as compared to the classical top-down and bottom-up comparison: ... Alternatively, the model-data-fusion system permits to leave out individual constraints and compare the resulting fluxes on specific spatiotemporal scales." to "However, bringing together the different data streams in one model-data-fusion system, allows in principle for a much better validation of the system as compared to the classical top-down and bottom-up comparison: ... Alternatively, and probably more practical, the model-data-fusion system permits to leave out individual constraints and compare the resulting fluxes on specific spatiotemporal scales."

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