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Interactive Comment

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

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General comments

This paper makes some general recommendations on strategies for CO_2 inversions at regional scales. Much of the presentation summarizes previous work from the authors. The paper presents what purports to be a new result about the dominant role of the near field (see specific comments). The paper does not group its recommendations succinctly in the conclusions but I draw three main suggestions from it:

1. Improved modelling of the near-field

2. Improved transport modelling

3. The use of a joint modelling framework for top-down and bottom-up estimates (often called Carbon-Cycle Data Assimilation or Model-data Fusion).

The first two of these are unarguable; how can anyone disagree with suggestions for improving the tools we use to solve a problem? I am also unlikely to disagree with the third point, having advocated the approach for a decade. That said, I believe the paper offers little that is new and I find myself disagreeing with most of the arguments used to support these recommendations. The paper also leaves an impression that the task of regional inversion is nearly impossible. Of course any paper targeting strategies to improve a research tool will catalogue the weaknesses in that tool and so leave a negative impression. also, any scientific community needs its balance of brash optimists and prudent pessimists. 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.

In my general comments below I will try to distinguish carefully between things I disagree with and a few statements in the paper which are demonstrably wrong or dangerously unclear. I may get this distinction wrong of course and I beg the authors' indulgence, remember that this is a discussion not yet a review. 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.

Specific comments

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0.0.1 Near Field Contribution

By running an essentially forward simulation, the authors purport to show that the nearest 20km radius to their chosen observing site contributes as much to their observed signal as the rest of the domain. the first thing to say about this “near field singularity” is that it isn’t new. It was shown to be a rather general property of high-resolution inversions coupled with high-resolution transport by Bocquet, Nonlinear Processes in Geophysics (2005). Bocquet (2005) encompasses not only the problem raised here but a more general problem that even the increments in an inversion will become more and more focused as resolution becomes finer. This is the counterweight to the solution of igh resolution usually taken to avoid the aggregation error of Kaminski et al., JGR (2001). It is a problem with which the field is yet to come to grips. 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. The key, I believe, lies in the bottom half of P1324 where the authors discuss the influence from different distances on their signal. The question is which signal? Let me start with the most obvious counterexample, the long-term trend. The long-term trend of about 2ppm/yr is equivalent to a source of about 8gC/m^2 for the globe. It is obvious that if this source was applied to a 20km radius circle from the tower or from the rest of the globe that the trends would be very different indeed. This is obviously not the case we have here but it does raise the general issue that the problem is absolutely dependent on which signals are being interpreted. In their total signal case, what the authors really see is a weak advection of the diurnal cycle coupled to a fall-off in the footprint. 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 although the refinement of the result with the higher resolution here remains interesting. What the authors actually show is that if one changes the flux by 10% in the near-field (less than 20km) this has the same effect as a 10% change in the far-field provided one scales both fluxes without changing their patterns and provided one keeps a sufficiently short time horizon. 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. In summary, I think this problem exists but is overstated here.

0.0.2 treatment of Aggregation Problems

This point chiefly touches the discussion on P1328 starting at line 10. I think the discussion here is based on either a misunderstanding of Kaminski et al., JGR, (2001) or a miscategorisation of the types of error. The classical agregation error developed by Kaminski et al., was describing the context where the resolution of the underlying transport model (and hence the sources which can be explicitly represented) is finer than that of the increments to the fluxes. this is now a much less serious problem than it was as we wrote that paper since the practice now is to invert fluxes at transport model resolution. there is an interesting issue left of the use of correlation lengths in inversions which we should now revisit but there is no doubt the available spatial degrees of freedom in an inversion in 2009 is vastly larger than in 2001. If one does accept there is still a problem then the two cases quoted don't seem to be coherent with the consequences described. 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? Perhaps they mean adding uncertainty to fluxes in the near-field. This also does not fit the underlying statistical approach. The prior flux uncertainty should represent our statistical knowledge of the difference between our prior and the truth. It should not take account of how this will project onto the data space. (See Tarantola, 2004 ch. 1 for an explanation of the underlying statistics here). The recommendation of case B, "use a better prior" is unarguable of course. Recall though, that the problems arise not

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from the quality of the prior but from the spatiaotemporal forms of the increments. The "intermediate" approach of specifying more detailed covariance structures is nothing other than a spatial filter for these increments. To see this, we can write the standard Bayesian form for the flux inversion as:

$$\vec{s} = \vec{s}_0 + C(\vec{s}_0)J^T \left[JC(\vec{s}_0)J^T + C(\vec{d})\right]^{-1} (\vec{d} - J\vec{s}_0)$$

where $\vec{s}$ represents sources, the subscript 0 indicating a prior, $\vec{d}$ observed concentrations, J the Jacobian (set of all flux footprints), the superscript T the transpose and $C(\vec{x})$ the covariance matrix of a vector quantity x. If we follow the last term (the flux increment) from the right we see that we start by calculating a mismatch between the data and prior simulation, weight it by a complicated matrix, project it back into source space through the transpose of the Jacobian then finally multiply it by the prior covariance. This has the effect that patterns not captured in the prior covariance cannot appear in the increments. In fact one can take advantage of this computationally to carry out the inversion in the eigen-space of the prior covariance (e.g. Chevallier et al., JGR 2005 and succeeding papers). 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.

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. We have a set of data to interpret,the question is whether this data can be more successfully interpretted by high-resolution models than low-resolution. That point has been pretty much settled by the studies of Geels et al., (2007), Law et al., (2008) and Patra et al. (2008) and I doubt the authors would disagree with the conclusion. High-resolution modelling usually demands limiting the domain for computational reasons. This is another of the sentences that gives the paper its overall negative tone.

P1320 (bottom) I don’t know whether the authors are trying to give the impression that

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MDF is in particular need of correct statistics (e.g. more than a direct flux inversion) but if so they need to verify it. Instinctively it looks unlikely.

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.

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.

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 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

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-

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mode data) remains a wise choice.

Technical corrections

P1318L11 "Much stronger" should be "much more strongly"

P1318L12 "since more" should be "for more"

P1322L19 "further on" should be "henceforth"

P1323L13 "strongly" should be "rapidly"

P1326 (2nd last line) "are completely" should be "will be completely"

P1332L6 "invest into" should be "invest in"

P1333L27 "permits to" should be "permits one to"

Interactive comment on Biogeosciences Discuss., 6, 1317, 2009.

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