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Geoscientific Model Development Discussions

Interactive comment on "CTDAS-Lagrange v1.0: A high-resolution data assimilation system for regional carbon dioxide observations" *by* Wei He et al.

Anonymous Referee #1

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The manuscript describes a regional scale inversion system or data assimilation system to derive biosphere-atmosphere fluxes of CO2 for North America. The regional system is largely based on CarbonTracker, but the paper describes a number of experiments to specifically assess the uncertainties in the regional flux estimates. The paper is well written, however a few aspects need to be addressed before I can recommend accepting the manuscript for publication.

Main comments:

1. The derived posterior fluxes are extremely variable at sub-seasonal time scales; in P12 L30 it is mentioned that these fluctuations may be related to "artifacts that are

caused by the sparseness of the observations". It should be investigated as to whether there might be some temporal averaging or aggregation required to reduce the noise.

2. Why are the footprints aggregated to 1x1 degrees, given the 10x10 km spatial resolution of WRF resolution, and given the spatial variability of fluxes (including anthropogenic emissions) combined with rather small footprint areas within the proximity of the atmospheric observations? Has any sensitivity analysis been performed to assess the impact of using these rather coarse footprints? Furthermore, it should be clarified if SiBCASA uses only dominant vegetation types at the 1x1 degree resolution, or whether a tile approach is used to also include other vegetation types within a given grid cell?

3. The shortest length scales used for the prior error covariance are rather large compared to those suggested e.g. by Chevallier et al. (2012) or by Kountouris et al. (2015). Given that the change in the annual net biosphere fluxes with correlation length scale seems to become larger with shorter length scales (Fig. 10), also short correlation scales should be investigated. Furthermore, fig. 10 should include the prior flux and the prior and posterior uncertainties (which might change with correlation scale depending on the setup).

Detailed comments:

P4 L7: Table 1 lists 12 rather than six PFP aircraft sites, also P4 L28 mentions 12 sites

P6 L18: In equation (1) beta_i, W_i and S should also have a dependence on observation time t_r and location X_r.

P6 L23-25: This is unclear. Particles leaving the domain below 3000 m are not considered, does that mean they get a zero boundary condition value for the mole fraction? Even though the influence from surface fluxes is strong, the lateral boundary condition for those should be quite different from zero. What about those particles that did not leave the domain within the 10 days?

P7 L24: Why use 3 hourly mean fluxes, as footprints are available at hourly time steps? Was the associated aggregation error quantified? Similar with fossil fuel emissions (P8 L5)

P8 L26: “multi-model prior suite of inversion” this is unclear, may be reformulate?

P9 L12: Lateral boundary condition EMP: references only describe LBCs for gases other than CO2. A more detailed description of how EMP CO2 fields were derived is needed. How does the EMP boundary condition differ from the one described in Gerbig et al., 2003?

P10 L18, Table 2: Cases BX1-BX5 should be included in Table 2 (there only BX2 is included, but the values for the different columns are identical to those for the Base run). Also, the run B2’ (included in Table 2) should be described in the text. Similarly, the multiplicative flux adjustment run should be included in Table 2.

Table 4: the values in brackets need to be explained in the table caption. Also “BC adjustment” should be given as a mean and a range, and the range should be explained in the caption.

Table 5: the values in brackets need to be explained in the table caption, I assume those are the uncertainties.

P14 L20: what is meant by “consistent”? Given the uncertainties all flux estimates are statistically indistinguishable.

P14 L23-29: unclear, how the averaging was done. Did the prior fluxes in the CT2013B-avg case have any diurnal variations? The reason for using 10-day averages is also not clear to me. CTDAS-Lagrange uses 10-day backward calculations, but the resulting footprint values change strongly with backward time (time before measurement time).

P15 L20: OSSEs can be set up in very different ways, allowing also for differences in transport (using different transport models) or structural differences in biospheric fluxes. This should be reformulated.

P15 L23, Fig. 8, Fig. 9: Why are the prior uncertainties in the additive method so much larger than in the multiplicative method? For a clear comparison between the two methods, the underlying uncertainties should be better matched. SCALE at which matching is needed?

P16 L22: This seems inconsistent with the numbers given in Table 2 and with the description in P11 L22

References: Chevallier, F., Wang, T., Ciais, P., Maignan, F., Bocquet, M., Altaf Arain, M., Cescatti, A., CHEN, J., Dolman, A. J., Law, B. E., Margolis, H. A., Montagnani, L. and Moors, E. J.: What eddy-covariance measurements tell us about prior land flux errors in CO 2-flux inversion schemes, Global Biogeochem. Cycles, 26(1), doi:10.1029/2010GB003974, 2012.

Kountouris, P., Gerbig, C., Totsche, K. U., Dolman, A. J., Meesters, A. G. C. A., Broquet, G., Maignan, F., Gioli, B., Montagnani, L. and Helfter, C.: An objective prior error quantification for regional atmospheric inverse applications, Biogeosciences, 12(24), 7403–7421, doi:10.5194/bg-12-7403-2015, 2015.

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