

Interactive comment on “Mesoscale inversion: first results from the CERES campaign with synthetic data” by T. Lauvaux et al.

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Received and published: 5 September 2007

The paper introduces an inverse modelling system to retrieve surface-atmosphere exchange fluxes at high resolution. The focus is on the technical setup of the system and on preliminary inversions with pseudo data to assess the capability to derive surface flux estimates with different measurement strategies.

General comments:

I congratulate the authors on setting up this mesoscale inversion system, they have combined quite a number of different components to a working overall system. Certainly the implementation of the minimization of the cost function and the treatment of prior and observational uncertainties is impressive.

However, the paper nearly completely ignores the fact that the transport as simulated by models are far from perfect. In fact, airborne measurement campaigns such as performed during CERES are targeted at assessing the capabilities of the modelling systems. The measurements usually reveal weaknesses in the representation of transport processes such as vertical mixing in the planetary boundary layer or vertical transport by convective processes. Further they help in assessing the spatial representativeness of a measurement network consisting of a few tall towers. The airborne campaigns are not primarily targeted at providing flux constraints for a short period of time. For the assessment of the inversion system presented in the paper this means two things: a) Ignoring for example the uncertainty in simulated mixing heights, the authors find that the ideal aircraft path for concentration measurements is a constant altitude within the boundary layer. Knowing that there is considerable uncertainty, we usually make sure to not only sample horizontal gradients, but also include many vertical profiles to allow assessing and improving the models capability to capture the PBL dynamics. b) The uncertainty reduction for the different strategies are strongly impacted by the capability of the model to represent a given measurement. Short towers such as the Marmade tower with 20 m height are hard to represent due to near surface processes, while tall towers with 300 m height are easier to represent. Aircraft columns from the surface to above the top of the boundary layer are easiest to represent, to first order they compensate for the lack of vertical mixing within the column. This is the most important difference for the different measurement strategies, not their somewhat different footprint. In this sense I think the assessment of the uncertainty reduction for the different measurement strategies is not appropriate.

The discretization of lateral boundary condition relative to surface fluxes seems somewhat arbitrary. Surface fluxes are resolved with 8 km, while the lateral boundary condition is gridded with 1x1 degree. Of course this has a strong impact on the uncertainty reduction for the unknown boundary fluxes. The discussion on the contribution from the boundary is somewhat unclear, the number a particle touches the surface before it leaves the domain doesn't really mean anything. What matters is the relative strength

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of influence the particles get from the different boundaries (surface vs. lateral). The suggestion that one could use an offset with an uncertainty reduces the numbers of degrees of freedom for the boundary values to one, which seems even more unrealistic. This should be better discussed in the context of expected variability near the boundaries vs. expected capability of the global model used to generate this boundary in the future.

Somewhere in the paper it should be explained what the inversion system will estimate in the future. Are the fluxes, for which uncertainty reductions are assessed, meant as small corrections to a bottom up model that provides good prior estimates, or may be correction of parameters within the bottom up model, or are they meant as complete fluxes between surface and atmosphere? This has large implications on the spatial and temporal scales required to represent them. Fluxes are a lot more variable than parameters controlling them.

The discussion on the vertical mixing (beginning of pg. 10454) is not fully clear. Enhancing the vertical mixing in an offline model should not lead to smaller error reduction (which means smaller surface influence) due to loss of particles to the free troposphere. Also, it is unclear how a better PBL scheme is supposed to fix this. What is probably required is a better implementation of vertical turbulence within the LPDM that fully avoids un-mixing from turbulent to less turbulent areas, see (Lin et al., 2003). Lin, J.C., Gerbig, C., Wofsy, S.C. et al.: A near-field tool for simulating the upstream influence of atmospheric observations: The Stochastic Time-Inverted Lagrangian Transport (STILT) model. *J. Geophys. Res.-Atmos.*, 108(D16), 4493, doi:10.1029/2002JD003161, 2003.

Specific comments:

Pg 10440 In 13: Instead of “noise” I suggest using the term uncertainty.

Pg 10440 In 20: The fact that half of the emitted CO₂ stays in the atmosphere wasn't really discovered in 2007, I agree with my Coreviewer in that I recommend using more reasonable references.

Pg 10442 In 12: The cited paper for COBRA was written before the mission; there are references discussing the mission and the data that I think should be used (Gerbig et al., 2003; Lin et al., 2004). Gerbig, C., Lin, J.C., Wofsy, S.C. et al.: Toward constraining regional-scale fluxes of CO₂ with atmospheric observations over a continent: 1. Observed spatial variability from airborne platforms. *J. Geophys. Res.-Atmos.*, 108(D24), 4756, doi:10.1029/2002JD003018, 2003. Lin, J.C., Gerbig, C., Wofsy, S.C. et al.: Measuring fluxes of trace gases at regional scales by Lagrangian observations: Application to the CO₂ Budget and Rectification Airborne (COBRA) study. *J. Geophys. Res.-Atmos.*, 109, D15304, doi:10.1029/2004JD004754, 2004.

Pg 10443 In 19: “intensive aircraft flight time period the 27 May” please reword.

Pg 10443 In 22: The authors probably mean analysed fields rather than reanalyzed.

Pg 10443 In 25: Not clear why the description of vertical transport is better at 2 km. Turbulent mixing and convection are still either parameterized, or not resolved at that resolution. The only part that does improve is orographically induced transport, which is as much horizontal as it is vertical.

Pg 10444 In 13: “aircraft require a shorter period of particle integration, and a better description of the vertical motion.” This is unclear. Do the authors mean the path needs to be better resolved due to vertical motion of the aircraft?

Pg 10444 In 14: “The second difficulty concerns the vertical motion in the free troposphere” I don’t really understand this. Vertical motion during the fair weather situations encountered during CERES intensive observing periods was limited to subsidence, which is a large scale phenomenon. Why is this a difficulty?

Pg 10445 In 2: “non-linearity Æ non-linear” What is meant by this non linearity? A non-linear relationship between fluxes and concentrations?

Pg 10445 In 15: “Æ hourly concentration Æ from each tower Æ each 3 minutes from each flight” I assume the reason to treat tower-hours like 3 aircraft minutes is supposed to

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account for some error covariance in representing the respective measurement in the model. I suggest estimating the different error components and their covariance length scales to some degree so that this can be justified, see e.g. in (Gerbig et al., 2003).

Pg 10446 In 15: It is unclear how a comparison between measurement and model (with unknown boundary conditions at the side and the surface) can provide reasonable estimates of the observation uncertainty.

Pg 10446 In 18: “lack of temporal correlations” this is unclear. Has this been tested statistically or is this expected?

Pg 10451 In 10: The term “virtual tall tower” is misleading in this context, since it is used to describe the concept of using short towers to extrapolate to higher heights.

Technical corrections:

Pg 10446 In 5: “A time interval of three minutes showed the best compromise between the signal at the surface, and the spatial extension of the corresponding receptor” This sentence is unclear.

Pg 10449 In 22: “This result sets the maximum time integration required because flux increments only depend on observations in this temporal window.” This is unclear.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 10439, 2007.

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