

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

T. Lauvaux et al.

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(Christoph Gerbig): 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.

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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.

(The Authors): The virtual flights experiment was used to optimize the altitude of the aircraft trajectory which could affect the spatial extension of the surface signal. Considering the independence of the signal with the flight altitude, we concluded that the strong mixing in the PBL homogeneize the surface signal at any measurement altitudes. But the horizontal virtual trajectories were just realised to seperate clearly the flights within the boundary layer or in the free troposphere. We added in the result chapter a more precise conclusion: "The three different measures are constant in the boundary layer, and decrease quickly above it, with almost the same shape. The vertical mixing is strongly dominant compared to the horizontal mixing, implying that any flight measurement at one location within the boundary layer will be influenced by the similar surface area."

(C. G.): 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

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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.

(T. A.): We agree on the fact that the uncertainty reduction is a relative value which ignores the model uncertainty here. The relatively small tower of Marmande is more dependent on the surface processes, which are more difficult to model especially by night. But, for the modelling point of view, a high tower won't bring any information during night time. Also, aircraft measurements have a limited interest looking at the error reduction, much smaller due to the limited period of flight. Finally, even if small towers show higher uncertainties in the models, they remain an interesting way to study surface processes.

(C. G.): 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 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.

(T. A.): For the resolution of the boundary conditions the important question for the paper is how much of the information content in the measurements must be sacrificed to avoid bias from incorrect boundary conditions. Again the question of the resolution is important, this times the spatial resolution. The boundaries of the domain are either over the ocean or some distance from the measurement sites. Over the ocean we

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expect atmospheric CO₂ structures to be quite smooth so are entitled to use relatively low spatial resolution to describe them. Over the land this may not be true but even here Linn et al. (2004) suggest the correlation length of 100km is reasonable. Note also that, the further away the boundary is from a measurement site, the less the detail of the boundary condition will matter.

(C.G.): 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.

(T.A.): We changed the word as suggested in a previous answer as follows: "Imperfect knowledge of boundary conditions does not significantly impact the error reduction for surface fluxes."

Pg 10440 In 20: The fact that half of the emitted CO₂ stays in the atmosphere wasn't

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really discovered in 2007, I agree with my Coreviewer in that I recommend using more reasonable references.

(T. A.): We used more adequate references as described in the answer to Wouter Peters.

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.

(T. A.): we changed the reference.

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

(T.A.): We changed as follows: "We simulated the 27th of May at 2km resolution which is an intensive period of flights, and included it in the longer simulation from the 23 to the 27 May at 8 km resolution."

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

(T.A.): We corrected it.

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.

(T.A.): We agree on this point in the PBL, but concerning aircraft modelling in the free

troposphere, the mean wind w was the main issue to retrieve a good estimate of the surface influence.

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?

(T.A.): We precised after this sentence the exact meaning. A small error on the vertical wind w at 2500m high has more impact than in the PBL considering the surface signal. Increasing the resolution improves the estimation of the meso scale processes also, such as sea breeze.

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?

(T.A.): See the previous answer.

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

(T.A.): We corrected this in a previous comment as follows: "solves most of the problems of non-linearity in the advection term"

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

(T.A.): As the modelled concentration is the final result of the coupling between boundary conditions, surface fluxes, and the transport, the direct comparison between model to measurement provides a reasonable estimate of the observation uncertainty. There are statistical conditions which must be met on the comparison of concentrations produced by the prior fluxes and the observations (See Michalak et al., JGR, 2005, doi:10.1029/2005JD005970). This comparison also includes the impact of prior uncertainties on the uncertainty in simulated concentrations. The direct comparison we have made, therefore, is a more rigorous test of uncertainty hence a more conservative assessment of data impact.

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

(T.A.): Considering prior studies at other scales (Chevallier et al, 2006), we expect it.

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. (T.A.): This is correct although a good replacement term is not obvious. We have changed the references to "virtual tall tower" to "potential tall tower".

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.

(T.A.): We have replaced the sentence with: "A time interval of 3 minutes produced the best compromise between the need for sufficient particles to gather good statistics and the need to limit the spatial extent of the receptor."

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.

(T.A.): We have replaced this with: "This result defines the maximum backward integration time for LPDM. Beyond this (earlier in time) there are no more particles in the

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domain hence no fluxes that affect this observation."

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