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Atmospheric Chemistry and Physics Discussions

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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1) Concerning particles released from towers at higher altitudes, they affect more the boundaries during night time, but the final influence remains small compared to surface influence. In more, the spread of the particles reaching the boundaries smoothes the effect of this increase. As the tall tower is affected more by boundary conditions during the night, the advection is small and the diffusion enlarges the influence area on the vertical boundaries, In the study of J. Vila-Guerau et al., 2004, the free troposphere influence dominates the surface influence during the morning, but not during the afternoon or the nighttime. In our study, the average influence during the CERES campaign, with a weak synoptic forcing, is dominated by the surface distribution for the towers. But we agree that during strong mixing period with the ventilation of the boundary layer, or during synoptic events, boundary conditions influence much more

the concentrations than the surface.

2) This is a question of style of explanation. We believe separating the explanation of the choices we made from their specification would complicate the task of reading the article. Furthermore, this is a new field so a detailed explanation of the choices seems important for researchers who will use the same methods.

3) This is a paper to help planning campaigns. Thus the error reduction is the most important thing. We are also setting the stage for a later inversion but the necessary forward comparisons are in Sarrat et al., 2007. In the later paper we will need to check simulation of residuals with LPDM but that is best done after a real inversion.

Minor comments:

>- Abstract: define LPDM and NH

As LPDM means "lagrangian particle dispersion model" and NH "non-hydrostatic", we didn't explain the acronyms not to repeat in the text.

>- Abstract: "The noise contributed by imperfect knowledge of boundary inflows does not significantly impair the resolution". I do not understand this sentence: 'noise' from boundary conditions is the same as 'signal' from distant regions and one cannot expect a non-specialist to understand the word 'noise' in this respect. Also, 'resolution' of what? The model? The domain? The observations?

We rewrite as: Imperfect knowledge of boundary conditions does not significantly impact the error reduction for surface fluxes.

>- Introduction: the citation of previous work attempting to "downscale top-down methods" is somewhat random and does not do justice to many other efforts. Please cite a more comprehensive list or cite something more specific from these studies that made them relevant to this work.

We have cited a more specific list in the text.

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>- Description of CERES: The description of the observations is chaotic: the observations are taken at 1Hz, averaged to three minutes, then averaged to half an hour, and available to this study at hourly resolution? Please give the surface elevation and tower inlet height as separate numbers, and give an indication of the height of the canopy around the towers. What instruments were used and how were they calibrated? Where is this data available or who can be contacted?

This is not a description of the campaign, that is already covered in Dolman et al, 2006. We have given the link to the campaign web-site.

>- Models, first sentence: 'coupled' what is the nature of this coupling? one-way, two-way, offline?

Precised as "offline".

>- Models: 'multiple backward simulations', a non-inversion specialist would not know what practice you refer to here, please clarify

This is explained below.

>- Models: '...meso-scale models improve the simulation of observations...'. One would hope so, but it is stated with too much certainty here. At least give one or two references that show the superiority of meso-scale models in this field. Also, what observations are you referring to here? Boundary layer heights? Temperatures? CO2?

sarrat et al., 2007, geels et al, 2007

>- p10444, line 3: This paragraph is really hard to understand even for someone who knows these methods. The general comments on static and dynamic receptors serve no purpose in this work, just describe what you did and give facts. No one will understand what you mean when "particles are integrated over time depending on the frequency of the fluxes": fluxes have all frequencies from milliseconds to hundreds of years.

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We have clarified this by: "For a tower, particles are integrated over time depending on the signal-noise of the observations."

>- p10444, line 24: 'mean' refers to what time/space interval?

In fact this is the explicitly resolved wind and we have changed the sentence accordingly.

>- p10445, line 1: 'solves most of the problems...' What are the problems with non-linearity you refer to and how are they solved by your coupling with an LPDM?

We precised the sentence as follows: 'solves most of the problems of non-linearity in the advection term'

>- p10446, line 6: What aspect of the surface signal did you assess when testing the compromise in time-averaging? Its size? magnitude? pattern?

In fact it is the stability of the particle distributions which become unreliable for very small numbers of particles. We have rephrased accordingly.

>- p10450, line 18: 'reduced', you mean 'smaller' I assume as one measurement does not influence the other.

>- p10451, line 5: "particle touchdowns' , can you think of a better way to say this?

This seems a good image for what actually happens in the model.

>- p10451, line 11: 'globally reduced' is not a good term to use in a regional study

corrected.

>- p10451, line 13: I do not understand the difference between the two results yet. Where did the particles go that did not reach the surface? They must have hit the boundaries?

Addressed in the general comment I think.

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>- p10451, line 19: It is too optimistic to say that your results will allow optimization of surface fluxes. What you have shown is that there is some surface signal to be exploited, but whether you can do that depends crucially on your model and the errors in it. This would be an excellent place to discuss some of those and especially the systematic ones (biases) that are so far not touched upon in this work, even though they might be the largest concern to your readers.

In fact it suggests such an optimisation and we have rephrased accordingly. We believe that discussion of biases is best left to a real inversion (see general comment above).

>- p10452: line 14: Please discuss the possible influence of upper BCs here as well, as I still have a hard time believing they do not influence your diurnal CO2 signal strongly through entrainment.

See general comment above.

>- p10453, line 2: Please define 'high altitude observations' in this sentence.

Done.

>- p10453, line 26: the reference to the water cycle is out of place here as it has not been mentioned anywhere in this work, nor demonstrated that a framework like yours >has anything to say about it.

There is a significant body of work showing the importance of coupling between the carbon and water cycles, we have added one of the key references.

>- p10545, line 9: The loss of particles to the free troposphere in enhanced mixing conditions again points to a substantial role for entrainment and thereby influence from the upper boundaries as you describe.

See general comment above.

>- p10454, line 20: I don't believe 'demonstrator' is a proper term except to indicate a person taking part in a protest. Please change.

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Changed.

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