Atmos. Chem. Phys. Discuss., 7, S3353–S3357, 2007
www.atmos-chem-phys-discuss.net/7/S3353/2007/

Atmospheric Chemistry and Physics Discussions

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

W. Peters (Referee)

Wouter.Peters@noaa.gov

Received and published: 24 July 2007

This paper presents a framework for the quantitative interpretation of high-frequency observations taken during an intensive in southern France. The focus is on the technical implementation of this framework that consists of a Eulerian meso-scale transport model, a Lagrangian particle dispersion model, and a Bayesian minimization method. The authors have successfully coupled these elements to address the potential reduction of Gaussian uncertainties of a-priori surface fluxes. The uncertainty reduction is assessed using different sets of the available observations.

Impressive about this work is the number of components that have been brought together in order to interpret observations. Very few studies before have achieved this with the eye for detail displayed here. Weaker points in this work is the lack of actual

model-data comparisons for the reader to assess the skill of the system, especially since the analysis focuses almost exclusively on random error components which are not necessarily the most relevant ones in the framework presented. Considering that designing and building a framework for analysis is a difficult but necessary first step towards more detailed analysis, this paper represents a substantial achievement that merits publication.

In addition to the list of minor comments below, I would like the authors to address the following three major issues:

1) The interpretation of the results is not fully satisfactory with respect to the utility of small towers, tall towers, and aircraft. Experiment 3 addresses the difference between tall and small towers but it remains unclear what the lesson learned really is. We are told that fewer particles touch the surface in the vicinity of the tower, but also that the effects on the boundary conditions is quite small. Where do these particles go for the tall tower if they don't affect the BCs? What about the upper boundary conditions (free troposphere)? This BC must be optimized too and can have quite an influence on the daytime concentration at a tower (see J. Vila-Guerau et al., 2004)? I have read a few times now that the influence of BCs on the observations is small compared to the surface flux influence, but do not feel that this analysis has convinced me of that fact. A more quantitative analysis (rather than just figure 6) is needed to show the relative contribution of lateral BCs, vertical BCs (!), and surface fluxes and should contrast these numbers for small towers, tall towers, and aircraft.

2) The description of the different components has to improve substantially. Please stick to important information and facts about the components, use a table to summarize the many time-scales of averaging, simulations, particle release, etc. The many digressions into potential benefits and drawbacks of your choices inserted now distract from the information and belong in the discussion (if anywhere at all).

3) Where are the observations? I believe that it is crucial for this paper to show some

ACPD

7, S3353–S3357, 2007

of the time series of observed CO2 that you plan to interpret. For instance, the weak influence of BCs depends very strongly on the synoptic situation: if a front moves through a drop in CO2 mixing ratios of 20 ppm is entirely due to B's and not to local fluxes. Since most of your arguments are based on strongly 'filtered' metrics such as "uncertainty reduction in a Bayesian framework" (with all its possibilities to influence the result through the assumed covariances), and "number of particles touching the surface", showing observed CO2 time series would allow the reader to assess your conclusions from a more traditional perspective: the diurnal cycle, synoptic variations, and the presence of other gradients in CO2.

Minor comments:

- Abstract: define LPDM and NH

- 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?

- 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.

- 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?

ACPD
7, S3353–S3357, 2007

EGU

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

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

- 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?

- 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.

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

- 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?

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

- 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?

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

- 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

ACPD

7, S3353–S3357, 2007

boundaries?

- 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.

- 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.

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

- 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.

- 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.

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

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