Atmos. Chem. Phys. Discuss., 11, C2351–C2354, 2011
www.atmos-chem-phys-discuss.net/11/C2351/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "High-resolution simulations of atmospheric CO₂ over complex terrain – representing the Ochsenkopf mountain tall tower" *by* D. Pillai et al.

Anonymous Referee #2

Received and published: 27 April 2011

The paper entitled "High resolution simulations of atmospheric CO2 over complex terrain - representing the Ochsenkopf mountain tall tower" evaluates the performances of mesoscale modeling tools to simulate the dynamics and the CO2 variability over mountain areas.

The study is oriented for a future inversion of co2 fluxes over the region, using the Lagrangian model STILT at high resolution. Considering this perspective, it is not clear how these results are going to be used in the future. The transport uncertainties, in an inverse framework, are projected to the observation space, i.e. the CO2 concentration space in this context. How are you going to convert the standard deviation on the wind

speed for example into a CO2 error contribution? What is the impact of a temperature bias on the model performance in the CO2 observation space? Whereas the overall analysis is interesting, there is no real conclusion about the performances of the model for future applications. Especially because the flux uncertainty was not diagnosed carefully. It is very difficult to separate and evaluate the different components of the CO2 concentration mismatch following your results. At this point, you only conclude that higher resolution models show a better agreement with the data compared to a GCM, but you don't clearly show that a mesoscale inversion will give unbiased and reasonable flux estimates (or at least how biased would be the inverse fluxes with your system).

In addition, some components are missing, as the wind direction (only one profile). How do you translate an error of 15 degrees in the wind direction into an observation error? If you run a Lagrangian model with a wrong wind direction, this is not a bias or an error that you bring in the system, it is a misplacement of the flux area contributing to your measurements. How can one handle this issue?

From a general perspective, the influence of the topography on mesoscale circulations is a relatively old and well-documented subject in meteorology (there is actually no references of mountain circulation studies in spite of trace gas applications). Your contribution targets an application of it, i.e. the use of mountain sites in CO2 flux inversions. The critical question is the estimation of the model errors (in the concentration space) in this context. Now, this study remains limited to a general meteorological study, including some CO2 concentration data, but little information on how to use them in an inverse framework.

I also agree with the first reviewer on the fact that a rigorous assessment of the "acceptable errors" is required. If you consider the actual CO2 mismatch and the overall transport errors, can you improve the fluxes with the present system? "relatively well" might not be enough.

Your discussion includes lots of descriptions (orography effects, seasonality) and potential causes for the underestimation of the peaks. Key questions for the readers are not really discussed. For example, you conclude pp6896-5 that STILT has "remarkable similarities". Does it mean that running WRF at 2km is not required? What is the minimum resolution that one should use? What is impact of TM3 boundary conditions on your CO2 concentrations? Is the vertical resolution a key component for mountain sites? The long descriptions of gravity waves is interesting but doesn't really discuss your results. You could estimate the performance of the model for several cases. Do you capture the dynamics of gravity waves systematically, or do you misrepresent some events? An inversion over several months or years will have to capture these events many times.

I recommend additional analyses of your results with a more rigorous assessment of the transport errors in terms of CO2, and clear conclusions considering the perspective of transport errors in an inversion framework.

Technical comments:

6877-12: add references

6878-4: define "scale of representativeness". Observations include also large scale signals which can affect measurements during synoptic events in particular, as you show in figure 8.

6885-9-15: did you estimate the monthly mean of the mismatch or the mismatch of the monthly means? The mismatch of the monthly means is not what is used in the inversion.

6886-7-10: The wind direction is a key element for the Lagrangian model. An additional figure showing the wind direction mismatch would be very informative and add value to your analysis.

6887-23: Do the fluxes used in TM3 have a diurnal cycle? If not, it might explain why

you observe no diurnal cycles in the modeled concentrations.

3.2.1: The VPRM fluxes are crucial in this section. The simplified equations governing the flux calculation in VPRM might be critical. Did you use the initial parameters of VPRM, or did you modify them for your region? The CO2 mismatch is depending a lot on the flux errors too, including the temporal variability. A synoptic event impacts also the CO2 fluxes (by temperature, incoming radiation,...). Could you separate the importance of the fluxes from the atmospheric dynamics?

6888-5: Which level did you use for TM3?

4.2: Whereas the description of the meteorology due to the topography is long, your conclusions are very general and don't really bring much. You could have the same conclusions just looking at concentration data.

6898-1-5: already documented in the literature. This section should include your results and conclusions.

6898-17: Do you need to run STILT at high resolution if the present version of STILT is good enough compared to WRF? What do you mean by "high resolution"?

Figure 4: The titles of the axes are misplaced or missing. "Altitude" is the y-axis while CO2 and q are on the x-axis.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 6875, 2011.