Authors' Response: 'High-resolution simulations of atmospheric CO2 over complex terrain- representing the Ochsenkopf mountain tall tower' by Pillai et al.

We would like to thank anonymous referee for his/her valuable comments. Authors' responses to these comments are as follows. The referee's comments are in black color fonts and the authors' responses are in blue color.

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?

The paper is now modified to include targeted error reduction in the fluxes after inversion and the acceptable error margins for the transport uncertainties (please see the response to first referee). The evaluation of meteorological variables gives a quality assessment of the atmospheric transport predicted by the models. This is important in case of CO_2 (as these meteorological variables can drive atmospheric CO_2 variability over a region) to diagnose the potential reasons for the model-data mismatch.

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).

We agree that "CO_2 concentrations are based on a combination of transport and flux modeling and both of which are associated with uncertainties" (this statement is now included in the paper). The paper aims at reducing transport uncertainty in a consistent way to improve the flux estimations, i.e. it shows how well the high-resolution transport model can represent the spatial and temporal variability of atmospheric CO_2 concentration over a complex area in order to minimize the transport uncertainty (or in other terms model-data mismatch) in the inverse modeling framework. However it is not within the scope of this paper to perform mesoscale inversion and to assess the actual reduction in flux uncertainties when using high-resolution models. This is already indicated in the paper (Sect. 5)

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?

We disagree with this. The models running with biased wind direction brings transport uncertainty to the system. In fact, all misrepresentations in transport, be it in advection or mixing, cause misplacement of the potential flux area contributing to the measurements, or of the strength of the contribution. As there is no perfect transport model, this has to be treated as an uncertainty.

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.

We disagree with the statement that this study remains limited to a general meteorological study. The questions which we address are 1) whether the observed CO_2 concentration around a mountain site is influenced by the mesoscale transport phenomena which cannot be accounted in the coarse models and 2) If yes, can the high-resolution models represent these associated variations and minimize the model-data mismatch in order to reduce the flux uncertainty. These are the important concerns to utilize the data from the mountain sites. Regarding the usability of data in the inverse system, we proposed the high-resolution nests in global models (see Sect. 5)

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.

- modified (please see the response to first referee)

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?

It does not mean that running WRF at high-resolution is not needed, as STILT is driven by the high-resolution meteorological fields generated from WRF.

What is the minimum resolution that one should use?

We see similarity between model simulations at 2 and 6 km when is driven by WRF meteorology at respective horizontal resolutions. It is expected that there will be degradation in model performance while running at coarser resolution (say 25 km). This can be tested by running our modeling system at coarser resolution; however needs additional time.

What is impact of TM3 boundary conditions on your CO2 concentrations? Is the vertical resolution a key component for mountain sites?
The impact of TM3 (coarse resolution) boundary conditions is not so important as atmospheric dispersion smears out the effect of unresolved features in the 3D field.

We have not studied the impact of increased vertical resolution alone. It is however expected to be significant in the mountainous regions.

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.
We disagree with the first statement. The effect of the gravity waves on the measured CO_2 concentration can be seen in Fig. 11 and the result was supported with Fig 12. Please note that the caption for Fig. 12 is now modified for the better interpretation (please see the response to first referee).

Reg. representation of the gravity waves: As already indicated in the text, the mountain wave is much more complex in reality and is difficult to predict as well as to interpret its effects on measurements. We cannot afford the high-resolution simulations (such as 2 km x 2 km) over periods of years to examine whether model captures the gravity waves systematically.

Technical comments:
6877-12: add references
-modified
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.
-modified to
"..thus reducing their horizontal scale of representativeness (the scale at which the measurement represents the underlying process) to about 100 km"

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.
It is mismatch of monthly mean to show how the model performs in general (on a monthly time scale) regarding meteorology.

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.

Please note that the plot already shows the wind components.

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.
yes

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?

The VPRM parameters are adapted to the European domain by optimizing with observations from different flux sites. Also please see the response above.

6888-5: Which level did you use for TM3?
- the pressure level close to OXK

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.
We disagree with this. Only looking at the concentration data would not have identified the mesoscale effects (in other words, the causes of the variability). Hence it is key to represent those in the model.

6898-1-5: already documented in the literature. This section should include your results and conclusions.
We disagree with this. These are the new results for OXK from our analysis.

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"?

The high-resolution in this context refers to a spatial resolution of 2 to 10 km. It does not mean that STILT has to be run at higher resolution than the present model configuration. The paper is modified accordingly to:
"Our future work will focus on regional inversions using STILT-VPRM at high-resolution (e.g. 10 km x 10 km) with a nested option."

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.
-modified