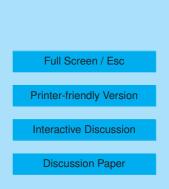**Atmospheric
Chemistry
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Discussions**

Interactive comment on "High-resolution simulations of atmospheric CO$_2$ over complex terrain – representing the Ochsenkopf mountain tall tower" *by* D. Pillai et al.

Anonymous Referee #1

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

Pillai et al. argue that high accuracy measurements made in tall towers are often not used in global atmospheric inversion studies, because the atmosphere around the tall towers is often influenced by (meso-scale) meteorological phenomena, which cannot be accounted for in the coarser global inversion models. Not using measurement data from these tall towers results in the loss of valuable information of source/sink distributions at the surface. In an effort to address this problem and examine possible solutions, Pillai et al. use two different meso-scale resolution models (STILT and WRF)

to simulate the atmospheric transport of tracers such as CO2 around the Ochsenkopf tall tower in Germany and compare these to measurements collected at different levels in the tall tower, and to coarse inverse model results (TM3). The latter two represent data taken at two extreme resolutions: local and global (4 x 5 deg). The objective of the study is to investigate what level of improvement the meso-scale resolution models can bring, compared to the coarse global inverse models. To answer this research questions, the authors focus on five occurrences of specific scales: a cold front passage (synoptic scale), a mountain-valley circulation (meso-scale), gravity waves (meso-scale), seasonal variability and vertical gradients. They compare model results with tall tower observations, as well as wind profiler data, and aircraft data. The study concludes that meso-scale models are able to represent much of the spatial and temporal variability of tracer concentrations observed at the tall tower at different levels, and during a suite of meteorological phenomena.

Whereas the conclusion, that meso-scale models are better capable to reproduce high resolution spatial and temporal variability, may not be stunning, this paper has several merits: 1) it combines available data (meteorological and tracer data, ground, aircraft based, and model data) in a logical way within one framework, 2) it addresses several rather different atmospheric phenomena. To my knowledge a meso-scale model evaluation of atmospheric CO2 transport has not been performed earlier on this scale. At the same time, such model systems are highly needed to explain and attribute the variability of the high accuracy observations taken at tall towers and flasks collected in the last decade(s). Unfortunately, the large extent of the study also puts limits to the depth at which the underlying processes can be studied. As a result, the study does not relate model-data mismatches to specific processes or model configurations, nor does it result in suggestions on how to improve the models. I consider this as a drawback of the integral method chosen, although I do suggest that the authors include a paragraph in section 5 to discuss potential improvements.

A more serious concern with the paper is the lack of a discussion of how good is good

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enough. On quite a number of occasions, the authors state that 'WRF or STILT captures XX "relatively well"', even though the difference between model and observations is sometimes up to a few degrees C / g/kg/ ppm. Prior to assessing whether the model does good or not, the paper needs to discuss what the model is supposed to do best for its particular purpose, and how that can be tested. In this case, the model is supposed to do CO2 transport good at high spatial and temporal resolution, so it needs to do vertical mixing, meso-scale circulations, pbl height and surface fluxes well. What are acceptable margins (e.g. compare to TM3 and measurement accuracy)? How important is a bias compared to amplitude? Only with a definition of the model requirements can statements about performance be made.

Below are a few minor comments. Because the paper investigates the performance of a highly needed model application, and does that in an integrated and comprehensive way, and because the suggested changes can be performed without additional model experiments, I recommend that the paper can be published with minor revisions.

Specific comments: Page 6880, line 4-9: this sentence is too long

Page 6880, last paragraph: it remains unclear, at this point, what method you will apply to address the objectives. It may help to better explain the methods here or just before.

Page 6881, section 2: information about the region (topography, land use, industrial activity, etc.) is missing, as well as the location of the tower, and the distance between the tower and the wind profiler.

Page 6883, line 8: The WRF domain of 500 x 500 km seems somewhat small, resulting in a large part of the model domain being influenced by the boundary conditions. Please comment on this.

Page 6883, line 24: The Pillai et al, 2011 is not submitted yet, so please do not refer to it.

Page 6884, section 3: Why don't you evaluate the model with respect to boundary

layer height and/or turbulence characteristics (u^*, tke, sigma_u, sigma_w, ...)? And, in the whisker plots, why don't you make a distinction between data taken below and above the top of the boundary layer, because humidity, temperature, and CO_2 often change considerably across the bl top. This may explain the large variability in specific humidity/relative humidity. You may even consider comparing average boundary layer measurements and model data when the bl is well mixed.

Page 6884, first paragraph, you discuss wind direction, but you show wind speed (Fig 2).

Page 6885, line 6: so why did you chose to compare this time slot?

Page 6885, lines 9-15: it is more conventional to display model – observations, because then an overestimation becomes a positive difference.

Page 6885, lines 19 and 21: change 'parameters' to 'variables'

Page 6886, line 3: why do you compare moisture in terms of RH and not in terms of q, because RH depends also on T

Page 6887, line 5: 'a slight underestimation': the difference looks quite large to me

Page 6887, first paragraph: it is interesting that WRF and STILT capture the daytime minimum CO_2 much better than the nighttime maximum, and that WRF and STILT both tend to underestimate the nighttime maximum. Can you explain this?

Page 6888, line 11: 'remarkably well': 1) what sort of temporal variability do you mean, 2) bias and standard deviation are indeed smaller than in TM3 and R2 larger, but they are still an order of magnitude larger than the measurement precision. Therefore I do not think the label 'remarkably well' is suitable.

Page 6888, line 23: 3rd reason: q has sinks (precipitation) in the atmosphere, where CO_2 has not.

Page 6889, line 23: 'a decrease' in spec. hum, not an increase?

Page 6890: how are the observed/modelled peaks defined in terms of spatial and temporal averages? Do differences in averaging explain part of the mismatch?

Page 6892, section 4.2.2 (Mountain wave activity). I do not see any waves in fig. 11, except in 11d, where w seems to correspond to U x dz_sfc/dx. I am not an expert in the field of gravity waves, therefore please explain where you see evidence of waves. To me Fig. 11 looks more like katabatic flow than like gravity waves.

Page 6894, line 9 'unrealistic gradient': I would say that WRF's gradient looks more like the observed gradient than STILT's.

Table 2: sd is short for standard deviation? Or standard error?

Figure 1: it is somewhat unclear whether the blue rectangles represent nested grids in STILT or WRF or both. Please re-formulate the caption

Figure 5: it is unclear what 'dimona' means (in the colorbar text)

Figures 10 and 12: the arrows show the wind direction. Which components do they show? Certainly not U and W?

Figure 13: Why do you show only 20 hours, and not 24?

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