Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-762-RC3, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "How can mountaintop CO_2 observations be used to constrain regional carbon fluxes?" *by* John C. Lin et al.

Anonymous Referee #2

Received and published: 16 January 2017

Summary

Lin et al. investigate the modelled CO2 concentration time series for June to August 2012 at 3 atmospheric measurements stations with RACCON observations. Carbon fluxes from established inventories and models are used as input for different atmospheric transport model configurations (x,y = 1.3km, 5km, 12km and 1degree) to investigate the impact of their resolution (especially the ability to reproduce the topography of the domain) on the model-data mismatch. Mean daily cycles of CO2 concentrations are analysed as well as the difference in the modelled back-trajectories. The authors results motivate 5 different approaches how to use (or not use) such observations in future studies.

General comments: The paper is well-written and the observational datasets, models

and methods are properly described and referenced. The authors address an urgent question about the future and current use of observations from mountainous areas or within complex terrains more general. Their analysis of mean daily cycle allows an easy interpretation of mean behaviour, but it would be critical to also investigate if there are episodes (meteorological conditions) for which the mismatch between the different transport model configuration is minimized/maximized. I would suggest to add an analysis of the time-dependant offset of each model compared to the observations. In general it would be valuable to have more quantitative results and a discussion how generalizable the findings of this study are for other hilly/mountainous areas. Overall this paper also does not fully address the question of "constraining carbon fluxes", but rather how well different model setups can reproduce the atmospheric concentrations of CO2. To be able to really judge if the models (even the best, 1.3km resolution) are able to e.g. distinguish different prior carbon flux estimates the authors would need to perform a sensitivity study using multiple carbon flux data sets and demonstrate a significant impact at the three sites. Potentially one can consider this study rather a step towards a better use of such data rather than already addressing the question of regional carbon fluxes. After the above and below comments are addressed I would definitely recommend this study for publication in ACP as it will help to better understand limitations of such observations and were future model developments should/could be focussed to eventually be able to constrain carbon fluxes in such regions.

Specific comments: Line 44ff: The claim that nearly 70% of the earth land surface is covered by hills or mountains needs to be better validated. This surely depends on the definition for hill or mountain, which is not given here and the cited publication is hard to access (due to the journal it was published in) and the journal has an impact factor below 1. The authors also mention that carbon fluxes in complex terrain need to be better understood to quantify carbon flux. It seems you are implying that all mountainous or hilly areas are (too) hard to model? Line 193: The authors refer to a previous publication – nonetheless the key parameters e.g. vertical mixing scheme used should be explicitly given in this publication (e.g. by adding a table in this section).

Line 216: Please specify if the system allows for two-way nesting or not Line 277: A “fix” is mentioned, but not explained at all. Please consider giving a brief description here rather than referring to the supplement. It seems the daily cycle has just been shifted or were there any more complicated adjustments performed? Line 304: Please consider refering to table 1 here so the reader can easily find the height CT data was extracted from. Line 503ff: The first question is repeated here “How can mountaintop CO2 observations be used to constrain regional scale carbon fluxes, ...” But the 5 subsections following rather discuss IF such data can be used or how they can be better used. It remains unclear if there is a definitive answer on how to use them. Line 530ff: Choosing the appropriate model layer to extract CO2 does indeed introduce a significant additional degree of freedom. The authors suggest other parameters to avoid creating a fudge factor but do not give specific advice here on which tracers could be useful (222Rn?). Meteorological data is mentioned but looking at table 2 it seems not at all clear that this would be good parameter or what a cut-off would be. Could you suggest how the a suitable proxy could be found? Line 581ff: Here the authors report on the practice of not using mountaintop data but it is unclear how this is linked to this specific study as no suggestion is made how to e.g. better use Schauinsland data. This section should be considered for the introduction to motivate why Approach 1, 3, 4, 5 need to be improved. Line 595ff: When setting up an inversion system it is common (good) practice to assign proper model errors. This seems not specific to this study and the authors fail to give an estimate of the model error for the three sites discussed here. Please consider removing this section or giving quantitative results for the sites and models investigated here. Of course, the model data mismatch calculated here also depends on flux errors, but the authors can surely use this study to give an upper limit of this combined error (and the difference for different model resolutions).

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-762, 2016.