Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2016-48-RC2, 2016

Geoscientific Model Development Discussions

Interactive comment on "Evaluation of the boundary layer dynamics of the TM5 model" *by* E. N. Koffi et al.

Anonymous Referee #2

Received and published: 2 May 2016

General comments

This study reports on a thorough evaluation of TM5 to describe the boundary layer dynamics, comparing various parameterization settings of the BL and extraction methods height to radiosonde, lidar and ceilometer observations. Furthermore simulations of 222Rn using two different emissions and various settings for advection and convection in TM5 are compared. The study draws potentially important conclusions regarding uncertainties due to convection parameterization in TM5, relevant for GHG emission studies, and is therefore well suited for publication in GMD.

While this study is certainly thorough, in its current shape the manuscript is merely a report on the numerous sensitivity runs that have been executed. A more rigorous selection of sensitivity experiments to be presented, along with a more selective pre-

sentation of observational data could largely improve the readability of the manuscript. Also the abstract is currently too elongated.

For instance, both presenting 'TM5' and 'TM5-IGRA' and likewise 'TM5-INGOS' and 'TM5-INGOS-INGRA' in figures 4-7 seems not necessary, as 'differences are usually very small' (p.10, l.13)' , and furthermore such differences cannot be explained in terms of sensitivity of the parameterization, but rather reflect a representation error. Therefore I believe these simulation results are even a bit confusing and should be removed from the figures.

In Figures 8 and 9 a clear improvement with the revised 222Rn emission map is visible, but differences between various convection/advection parameterizations is less obvious, which makes me wonder if presentation of all these results could not be more condensed, or moved to the supplementary material.

I believe the figures 4-9 benefit from presenting only seasonal mean statistics, rather than monthly means: The same messages can be conveyed with much condensed use of figures.

Also the authors put large emphasis on the improvement in the comparison to 222Rn observations when using the new flux map. However, the purpose of this paper is rather the evaluation of the boundary layer dynamics in TM5, by performing sensitivity runs. While many figures are presented, in the end it remains unclear to me how the parameterizations quantitatively compare, presented preferably in a Table. E.g. the statistics of the analyses given in Figures 11 and 12 could be averaged over the different stations, while excluding coastal stations hampered by representation errors and excluding the results obtained with the simplified flux map.

On the abstract, I believe the authors should condense this strongly, by reporting only the key findings of this study, which I believe are the performance of TM5 to represent BLH (l.14-l.17), and the achievements and limitations of the comparison against the new 222Rn flux map.

Detailed comments

Abstract

Please consider to condense especially lines 3-12. Also I suggest to remove the conclusion regarding the improvement with the new Karstens et al. emissions from the abstract because, even though interesting, it is not essential to the subject of this manuscript. Consider re-formulation of sentence on l. 21-24, which is difficult to grasp. Also lines 37-42 read a bit confusing: while ECMWF convection results in much lower 222Rn activity than TM5 the authors cannot conclude if this is an improvement or not, which in its current formulation, does not appear a useful finding.

Introduction

I expect a few more references to studies to previous work that have considered the relevance of boundary layer dynamics for trace gas distributions, (and inversions), e.g. Locatelli et al., GMD 2015. How does this new work relate to that study?

Section 2

Page 5, l. 14: You introduce a figure where you compare the Cabauw ceilometer BL with IGRA data. I expect some discussion and interpretation of this result at this point.

Section 3

Here, and at several points throughout the manuscript, you mention the issues associated to the resolution of TM5 (1x1 horizontally over Europe, 25 vertical layers, 3-hourly surface meteo data, 6-hourly 3D fields). Considering it's apparent relevance it would have been interesting to see a sensitivity study at different model resolutions. Could you specify the temporal resolution of the ECMWF convection fields in your sensitivity study? Is this 6 hour?

P 8, l 14: 'Noah soil moisture data' : Do you have a reference here? Considering it's apparent sensitivity to soil moisture, why didn't you consider use of the ERA-Interim

reanalysis? This would be more consistent with the 222Rn atmospheric model simulations, or?

Section 4

"We extract…": Are TM5 simulations of 222Rn collocated in time and space with respect to the observations? Please be more specific here on horizontal, vertical an time interpolation.

Section 5.

Pp 10, l 14 – l19: So do you have any indication that ECMWF treatment of the BLH is better than the one currently used in TM5, based on this? Please provide more quantitative conclusions.

Pp 11, l 38: "The mismatch (…) cannot be explained by the modelled BLH". This statement seems inconsistent with Figure 12, where 'potential shortcomings of TM5 to correctly simulate the vertical 222Rn activity concentration gradients' are illustrated. Please explain this apparent inconsistency.

P12, l3-l7: Karstens et al. pointed out that the uncertainty averaged over the footprint might be smaller than 50

P12, l22: the authors suggest that the GHG-emissions derived in inverse modeling change by the same order of magnitude as 222Rn, i.e. 10-30

P12, l35-39: Please provide a short interpretation of this sensitivity analysis.

P12, l43: "tower height of 20m is within the first model layer 200m is within layer 3.": Considering it's sensitivity, how did you treat the model sampling? Did you apply vertical interpolation? Do you expect any sensitivity to vertical model resolution?

P13, l11: In this section, and in Figure 13, I miss results from the FI-CE run using the ECMWF meteo. Or are differences marginal? Please comment.

Figure 11, right panels and Figure 12: Which parameterizations are used for the computation of the TM5 boundary layer height? Standard TM5 or ECMWF convection? Or is the difference in BLH for the two parameterizations marginal?

Conclusions

P 14, l17 “The updated slopes treatment”: This is jargon. Please reformulate to something more generic, e.g. “the revised advection parameterization”. Could you indicate the importance of this study for GHG inversions based on TM5? Is this study a ground for replacing the convection treatment in TM5, or is it merely useful in providing a constraint on the uncertainty estimate of the GHG emission inversions?

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2016-48, 2016.