

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

Anonymous Referee #2

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We thank the reviewer for his/her constructive review. In what follows, the comments of the reviewer are in italic and our reply in normal face.

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 ^{222}Rn 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 presentation 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.

We have significantly reduced the number of sensitivity experiments shown in the main paper: For the TM5 boundary layer heights we show now in the revised version only the boundary layers heights evaluated with the InGOS definition (consistent with the definition used for the IGRA radiosondes), evaluated both at the InGOS stations and the adjacent IGRA stations (see Section 3.2 in the revised version). The additional evaluations of the BLH are now shown only in the supplementary material. For ^{222}Rn activity concentrations, we show now only 3 cases (FC_CT, FI_CT, FI_CU; see Section 3.4 in the revised version) in the main Figures. Also the abstract has been significantly shortened.

In Figures 8 and 9 a clear improvement with the revised ^{222}Rn 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.

We have condensed the presentation of the various convection/advection parameterizations, and show now in the revised version only the simulations with the combined 'revised slopes scheme' and ECMWF ERA-Interim convection. Furthermore, we removed the paragraph on this issue from the abstract

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.

We had deliberately chosen to show the monthly means and would like to keep this presentation, since it gives more detailed information (more precise representation of the seasonal evolution) than the seasonal means. As already explained above, to render more readable the different graphs, we show now in the revised version only the more relevant model experimental settings in these Figures.

Also the authors put large emphasis on the improvement in the comparison to ^{222}Rn 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.

Although the evaluation of the new ^{222}Rn flux map is not the primary objective of this paper, realistic ^{222}Rn emissions are an essential prerequisite for the model validation.

We prefer to keep the presentation of the statistics per station (Figure 11, now Figure 8 in the revised version), because of (1) considerable differences also among the non-coastal sites, and (2) the limited number of stations.

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 ^{222}Rn flux map.

We have condensed the abstract significantly and deleted the paragraph on the different convection/advection parameterizations.

Detailed comments

Abstract

See our reply above

Please consider to condense especially lines 3-12.

We shortened this part of the abstract.

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.

We think that this conclusion is important because realistic ^{222}Rn emissions are an essential prerequisite for the model validation. Therefore, we would like to keep this conclusion in the abstract.

Consider re-formulation of sentence on l. 21-24, which is difficult to grasp.

The sentence has been slightly rephrased

Also lines 37-42 read a bit confusing: while ECMWF convection results in much lower ^{222}Rn 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.

We have deleted this paragraph from the abstract.

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?

We have included the suggested reference Locatelli et al. (2015).

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.

We have moved Figure 2 (submitted version) to the Supplement. The scatter plots of Cabauw ceilometer BLHs at 00 and 12 UTC are now shown in a single Figure S1 in the Supplement (and the figure caption updated accordingly).

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?

The temporal resolution of the ECMWF ERA-Interim convective fields is 3 hours. However, in the TM5 version used in this study, 6 hourly 3D meteo fields were applied (See Section 3.1).

P 8, l 14: ‘Noah soil moisture data’ : Do you have a reference here?

The following reference of Rodell et al. (2004) has been added:

Rodell, M., P. R. Houser, U. Jambor, J. Gottschalck, K. Mitchell, C.-J. Meng, K. Arsenault, B. Cosgrove, J. Radakovich, M. Bosilovich, J. K. Entin, J. P. Walker, D. Lohmann, and D. Toll, 2004. The Global Land Data Assimilation System, Bulletin of the American Meteorological Society, 85(3): 381-394

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 ^{222}Rn atmospheric model simulations, or?

Karstens et al. (2015) recommended the use of the new emission maps derived from the Noah reanalysis. The authors found that “comparison with observations suggests that the flux estimates based on the GLDAS Noah soil moisture model on average better represent observed fluxes”. We included the conclusion from Karstens et al. (2015) in the text to explain our choice of the Noah data based ^{222}Rn flux map.

Furthermore, we simulated the ^{222}Rn activity concentrations also using the ERA-Interim based ^{222}Rn flux map (not shown). These additional sensitivity runs showed overall poorer agreement with ^{222}Rn observations than the Noah data based ^{222}Rn simulations, confirming the conclusion of Karstens et al. (2015).

Section 4

“We extract. . .”: Are TM5 simulations of ^{222}Rn collocated in time and space with respect to the observations? Please be more specific here on horizontal, vertical and time interpolation.

We apply 3 dimensional interpolation (i.e., horizontal and vertical interpolation) using the ^{222}Rn activity concentrations of the neighboring grid cells. The model output are hourly averaged concentrations (which are directly compared to the hourly averaged observations)

Section 5.

Pp 10, l 14 – 119: 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.

No. We only stated that the differences we sometimes observe between TM5 BLHs and ECMWF BLHs at some coastal sites may be attributed to i) the relatively finer spatial resolution of ECMWF (~80 km in horizontal on 60 levels) and ii) to the different treatment of BLH in the

two models. The ECMWF BLHs are not discussed anymore in the text, hence this sentence has been deleted

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 ^{222}Rn activity concentration gradients’ are illustrated. Please explain this apparent inconsistency.

This is maybe confusing. No, Figure 12 (submitted version; Figure 10 in the revised version) shows the ratios of boundary layer heights (modelled BLH versus observed BLH) at noon along with the ratios of ^{222}Rn activity concentrations (observed versus simulated) at 12, 13, 14, 15 LT for different seasons. We found that at most of the studied stations, the modelled BLHs compare well with observed BLHs, while the differences between the simulated ^{222}Rn activity concentrations and observed ones can be larger. This result points to potential shortcomings of TM5 to correctly simulate the vertical mixing of ^{222}Rn activity concentrations within the boundary layer. The text has been updated.

P12, l3-17: Karstens et al. pointed out that the uncertainty averaged over the footprint might be smaller than 50

The uncertainty averaged over the footprint could be smaller. However, as discussed in the paper, the uncertainties of neighboring pixels in the ^{222}Rn flux map are likely strongly correlated, and therefore the reduction of the relative uncertainty (integrated over a typical footprint on the order of 50-200km) is probably relatively small.

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

Yes, this is correct. As mentioned in the paper, this has also been confirmed by first GHG inversions with the new ECMWF based convection (not shown).

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

We analyzed the ratios of both boundary layer heights and ^{222}Rn activity concentrations as shown in Figure 12 (submitted version; Figure 10 in the revised version) for the 3 main stability regimes (stable, neutral, unstable or fair). We used the modelled Richardson number obtained at the first level of the model to discriminate between the 3 stability regimes. Results for the three stability regimes are similar and similar to those obtained when considering sample covering all the stability regimes shown in Figure 10 (revised version). A limitation of this exercise was that for both stable and neutral stability regimes, we had at most stations, only few cases by seasons. The text has been revised

P12, 143: “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?

As mentioned above, ^{222}Rn activity concentrations are 3-D interpolated, i.e. including vertical interpolation. Yes, we expect some dependence on the vertical resolution of the model. However, this has not yet been analyzed in detail. The 3-D interpolation is now stated (see Section 3.2)

P13, 111: 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.

We now present the simulations of ^{222}Rn activity concentrations by using convection scheme based on ECMWF reanalysis (FI-CE) combined with the “revised slopes scheme (FI_CS). The differences between FI-CT and FI-CS are marginal (Figures S14-S24 in the revised version of the Supplement), hence the differences between FI_CT and FI_CU are dominated by FI-CE

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?

In Figure 11 (submitted version), the TM5 default boundary layer was shown in the submitted version. We now show the boundary layer height extract at the closest IGRA station associated to the InGOS measurement sites (acronym TM5_INGOS_IGRA; Figure 8 in the revised version)

Conclusions

P 14, 117 “The updated slopes treatment”: This is jargon. Please reformulate to something more generic, e.g. “the revised advection parameterization”.

We use now the term 'revised slopes scheme' throughout the paper.

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?

Since we did not find a significant difference / improvement of the ^{222}Rn simulations with the new ECMWF convection, this study does not provide enough evidence, which would justify the replacement of the convection scheme. Further studies are currently performed within the TM5 modelling community (including the use of further tracers), however at this stage no clear conclusion can be drawn.