

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

**Anonymous Referee #1**

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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**

*I think this paper could be significantly enhanced by including some further discussion or even recommendations on estimating model transport errors based on the model-observation comparisons of  $^{222}\text{Rn}$  and BLHs. As the authors already point out, transport errors are a substantial source of uncertainty in the fluxes estimated in atmospheric inversions. There are already a number of groups using TM5 in atmospheric inversions, but such recommendations need not be limited only to TM5 but in general the use of the new  $^{222}\text{Rn}$  emission map and the IGRA BLH dataset for assessing model transport errors.*

We have added several recommendations at the end of the conclusions.

*The paper includes many detailed figures of the comparison of BLHs and  $^{222}\text{Rn}$  but I think a couple of figures that summarize (i.e. give a more immediate indication of) the comparison between the model and observations and of the seasonal and diurnal cycles could be very helpful. Then some of the detailed figures could be moved into the supplement.*

We have reduced the number of figures, and reduced the number of scenarios shown both for the comparison of the BLH and  $^{222}\text{Rn}$  activity concentrations (as described in more detail in reply to reviewers #2 and #3).

**Specific comments**

*P3, L21: Here the authors mention only surface monitoring stations in regional inversions but not aircraft data, which are often used (e.g. the Kort et al. 2008 study cited here). Model representation of aircraft observations will be also affected by errors in BLH and simulations of boundary layer dynamics. Perhaps this should be mentioned.*

We have modified the text, mentioning explicitly the use of aircraft data in the study of Kort et al. (2008). Furthermore, we added a reference (Miller et al., 2013), which also use aircraft data for their flux inversion.

*P10, L26-27: It is interesting that the modelled nocturnal BLHs tend to be higher than observed in summer but that this is not the case in winter? Can the authors comment on this?*

Obviously, the model has in particular difficulties to simulate the very shallow nocturnal BLH, which is often observed at continental stations in summer. This is partly due to the fixed lower limit of 100m for the BLH in the model (see Figures S12 and S13 in the revised version of the Supplement).

*P10, L41: The authors do not discuss comparison of the modelled and observed (at IGRA sites) nighttime BLHs for Cabauw or Trainou.*

We had not discussed in detail the comparison of modelled and observed nighttime BLHs at Cabauw and Trainou due to the limitations of the ceilometer / LIDAR measurements during night (see section 2.1.2).

*P11, L7: Please give a quantitative estimate of “better agreement” either stating the improvement in the RMSE or correlation.*

The statement refers primarily to Figures 8 and 9 (Figures 6 and 7 the revised version), which shows the seasonal variation of observed and simulated  $^{222}\text{Rn}$  activity concentration. In addition, the improvement is also clearly visible in the overall statistics, shown in Figure 11 (Figure 8 in the revised version), which shows the improvement both in the RMSE values and correlation coefficients. This is briefly discussed later (in Section 4.2 of the revised manuscript).

*P11, L15: Please delete “apparently” – either the InGOS 222Rn flux maps give better agreement or they don’t, so “apparently” is not appropriate here.*

Deleted as suggested

*P11, L38-39: The authors state that the mismatch between the observed and modelled 222Rn activity concentrations cannot be due to the modelled BLH because this matches the observed BLH well. However, I understand that the modelled BLH is determined by vertical interpolation, therefore, I wonder if the vertical resolution in TM5 may be a possible reason for the mismatch?*

The dependence of the TM5 BLH on the vertical resolution has not been investigated. However, we note that the TM5 BLH (evaluated in the model version with 25 vertical layers) is in general very close to the ECMWF ERA-Interim BLH (60 vertical layers).

*P13, L1-11: I think this section should be expanded to discuss the influence of compensating errors in the 222Rn fluxes (in the constant versus InGOS flux maps) and in the BLHs and how this might explain the fact that the simulations with the constant fluxes lead to a better comparison with the observations.*

This is a good point. We have added a short statement that this could point to partially compensating systematic errors (See Section 4.2 in the revised version).

*Technical comments*

*P4, L46: “as” should be replaced by “compared to”*

We have slightly modified the sentence to: "attribute the height of the residual layer of aerosol ... as height of the real mixed layer". The suggested "compared to" would change the content of the sentence

*P10, L36: delete “also” after “In addition”.*

Deleted as suggested