

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

**Anonymous Referee #3**

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

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*The paper attempts to evaluate the performance of TM5 to simulate boundary layer heights and surface radon concentrations. Some biases are found that the authors link to some weaknesses in TM5. Overall, the paper is fairly well written but it is obvious that many people were involved in the analysis of data and model output which makes the paper appear ‘fragmented’ and, at times, unstructured and disorganized. Provided below are major and minor comments which also include some suggestions to improve the paper.*

**Major comments**

*1) Is Geosc. Model Dev. An appropriate journal for this type of paper? This paper addresses the evaluation of a model, not the development. A journal such as Atmospheric Chemistry and Physics or Boundary Layer Meteorology seems more appropriate to me.*

Yes, we believe that GMD is appropriate:

(1) GMD lists under 'Aims and scope' explicitly 'full evaluations of previously published models', see:

[http://www.geoscientific-model-development.net/about/aims\\_and\\_scope.html](http://www.geoscientific-model-development.net/about/aims_and_scope.html)

(2) Moreover, the referee #2 explicitly stated that the paper is "well suited for publication in GMD".

*2) The title is too broad and should be made more focused on those aspects that are actually studied in the paper, i.e. daytime and nocturnal boundary depths and  $^{222}\text{Rn}$ -concentration. Boundary layer dynamics include the study of thermodynamical and dynamical processes in the boundary layer including e.g. winds, stability, entrainment, etc. These processes are not studied in this paper and the title is therefore misleading. The title should reflect that the analysis is only made over Europe.*

The paper investigates the boundary layer heights as well as the processes in the boundary layer (including dynamic and thermodynamic, etc...). When simulating the  $^{222}\text{Rn}$  activity concentration in TM5, all the thermodynamic and dynamic processes in the model are relevant. Thus, the evaluation of the model simulations of  $^{222}\text{Rn}$  activity concentrations implicitly includes the evaluation of the whole boundary layer dynamics.

We agree with the second statement regarding the focus of the paper on Europe and have updated the title accordingly: “Evaluation of the boundary layer dynamics of the TM5 model over Europe”

*3) The difficulty of a coarse model to represent a coastal zone has not only to do with the coarseness of the model, but also the horizontal spatial variability. Also in high resolution models the largest spatial variability for fluxes can be found in these regions. For CO<sub>2</sub> this has been addressed by Pillai et al.(2010).*

We agree that also spatial variability of <sup>222</sup>Rn fluxes close to the coast may also play a role for the simulation of <sup>222</sup>Rn activity concentrations at stations close to coast. For the <sup>222</sup>Rn fluxes, the largest effect should be related to the variability / gradient of the water table close to the coast. In principle, this should be covered by the <sup>222</sup>Rn flux map (within the horizontal resolution of the input data sets)

*4) There are some problems in the structure of the paper, and titles of sections are sometimes inappropriate/misleading. Also the introduction of some figures in the text is sometimes a bit strange. For example. Figure 2 is introduced very early, but is only discussed very late (much later than the discussion of Figs. 4 and 5). This must be resolved by either putting the discussion in the section where it is introduced, or before the model output is compared to observations. As for an example eof a misleading section titles, consider e.g. Section 4 which is entitled ‘simulation setup’. Section 4.1 only addresses extraction of model output and no aspects of the simulation setup. These misleading/inappropriate titles should be corrected. It would be good to give subsection with appropriate titles in the Result section.*

We have revised the structure of the paper. Figure 2 has been moved to the Supplement. Several titles have been updated and subtitles have been added.

*5) The ceilometer/lidar related part does not really fit in this paper. There are many issues with the comparability between radiosonde/lidar derived PBL heighths as discussed in many papers (and also obvious from Fig. 2) and you don’t want to include these issues and uncertainties in this paper. In fact, including these data makes some conclusions in the paper rather weak. Figure 6 and 7 (and stars in Fig. 11) which include the ceilometer data do not add anything new and can easily be removed.*

Despite the discussed limitations (especially for the ceilometer at Cabauw during night), we consider the ceilometer/lidar data useful as they provide information about the dynamic evolution, which is not well resolved by the IGRA data with only 2 measurements per day.

*6) The authors mention coastal and non-coastal stations as well as mountainous stations (that they have removed from the evaluation). It would be nice to include the IGRA stations in table (not just Radon stations as is currently done) and indicate what stations are in coastal and*

*mountainous regions. It also seems important that the authors explain how they define a coastal or mountainous station.*

We indicate now the chosen IGRA stations (which are closest to the InGOS stations) in the updated Figure 1.

We consider stations as 'coastal' (in a strict sense) if they are located at the coast (as e.g, Mace Head). However, when comparing with model simulations, also the horizontal model resolution has to be taken into account. Thus, model representation errors (related to the land / sea gradients) arise, if the model grid cell, in which the station is located, covers also a significant sea fraction. For stations, for which this is relevant (but which are not coastal stations in a strict sense) we choose now the term 'close to the coast'

*7) The reader is overwhelmed with data and figures (not to speak of the supplemental figures!). Reduce the number of figures and also the number of subfigures with certain figures. Some of this could be addressed by removing lidar/ceilometer related data as indicated in major comment 5. In Figs. 4 and 5, not all stations need to be shown. Just pick a few that clearly show some points you are making in the paper. It would also be nice to see in the figures which stations are in coastal/non-coastal terrain, as this seems important in the analysis (see previous comment on coastal and non-coastal stations).*

We have reduced the number of figures both in the main paper and in the Supplement. Furthermore, we have reduced significantly the number of scenarios. However, we would like to show the full set of InGOS stations, since this paper also aims to support the further analysis of the GHG flux inversions (CH<sub>4</sub>, N<sub>2</sub>O) performed within the InGOS project (manuscripts in preparation). We consider the link between these studies very important, since potential systematic errors in the simulation of the BLH dynamics (discussed in the present paper) could directly translate into systematic errors in the derived fluxes.

### ***Minor comments***

*1. P2, general: The abstract is very long (almost longer than the introduction) and reads like a summary.*

The abstract has been revised (and significantly shortened)

*2. P2, line 4: “dynamics” should be “height”*

We would prefer to keep the term “dynamics” (see also our reply to reviewer's major comment (2))

*3. P3, line 15: define BLH properly, is it above the surface (depth) or above sea level (height).*

The BLH is defined with reference to surface elevation, and not to sea level (Seidel et al., 2012). This has now been added in section 2.1.

4. P4, line 11: Section title could also be depth, depending on definition

We prefer to keep the title "Boundary layer height"

5. P4, line 19: The equation of bulk Richardson number should be introduced here and not on page 7.

Yes. The paragraph has been moved to Section 2.1 and updated

6. P4, lines 19-22: There should be some more explanation on choices made and how to use the bulk Richardson number. For example, how is  $\theta_y$  calculated from IGRA-soundings? The neglect of  $u^*$  is hardly explained, but this is stressed in the Seidel 2012-paper, a citation here would help.

This part of the text has been revised and the paper of Seidel et al. (2012) quoted again there

7. P5, line 13: The introduction of this figure is very strange, as it is not discussed here.

This Figure has been moved to the Supplement as Figure S1

8. Figure 2: Including the ceilometer data is not recommended as mentioned in the major comments. We see here clearly one of the issues in that ceilometer is underestimating blh from IGRA. A complicated issue that is not suitable for the current paper.

See our reply to the reviewer's major comments (5)

9. P6, line 5: unclear:  $\pm 10$  to  $\pm 15\%$  ? or does  $\pm$  means approximately?

We have corrected this to '10-15%'

10. P6, line 9: 15m inlet should with a space. The paper has many of these types of typos. Please check.

The GMD convention seems to be not to use a space before 'm' (meter)

[http://www.geoscientific-model-development.net/for\\_authors/manuscript\\_preparation.html](http://www.geoscientific-model-development.net/for_authors/manuscript_preparation.html)

11. P6, section 3.1: the addition of a figure where vertical resolution of TM5 model and radiosonde are compared would be helpful. This would also make clear at what exact depths the TM5 model gives output. Then, as an example one could examine a typical boundary layer depth in this figure. Keep in mind that many readers of Geos. Mod. Dev. are probably not familiar with a concept like boundary layer height. See also major comment on appropriateness of journal.

We agree that such a figure would be useful. However, it would increase the number of figures that the reviewer asked to decrease. We refer the reader to the paper of Seidel et al. (2012), where the method is nicely illustrated in their Figure 1.

*12. P6, line 30: there are 60 vertical levels below 0.1 hPa and 25 layers below 0.2 hPa. How dense is the layering between 0.1 and 0.2 hPa? Or is it ECMWF and TM5 layering?*

The 25 vertical layers of the TM5 model version used in this study are defined as a subset of the 60 vertical layers of the ECMWF ERA-Interim reanalysis. The text has been updated.

*13. P7, line 5 . The idea of an “updated slopes scheme (treatment?)” is very unclear and should be clarified.*

We have updated the short description of the “revised slopes scheme” (and use this term now throughout the text). For further details the reader is referred to van der Veen (2013).

*14. P7, line 19: Delete “vertical”. “aerosol” should be plural.*

Has been corrected as suggested (moved to section 2.1)

*15. P7, line 20: All the observational devices..... are based on the search...” Not an accurate statement. For example, sometimes strongest gradients occur right at the surface.*

This should exclude indeed the gradients right at the surface.

*16. P7, line 21: “can be either” should be “can be based either on”.*

Corrected as suggested

*17. P7, line 42: m/s is m s<sup>-1</sup>.*

Corrected as suggested

*18. P7, line 44: Unclear/ambiguous sentence.*

First, we computed the Richardson number  $R_{ib}$  at each of the model levels by using the equation (1). To determine the boundary layer height, the vertical profile of  $R_{ib}$  is interpolated linearly between consecutive levels. The BLH is defined as the height, where  $R_{ib}$  reaches the critical value  $R_{ic}$ . The text has been updated.

*19. P8, line 1: Why is a value of  $R_{ic}$  of 0.3 used in TM5 and not the more common value of 0.25? Should be an easy fix for the model developers.*

$R_{ic}$  of 0.3 is the default value for the BLH determination in TM5, but there is no publication about this specific aspect. However, as discussed in Seidel et al. (2012), the choice of  $R_{ic}$  close to 0.25 does not introduce large uncertainty. Moreover, the differences between BLHs determined by using  $R_{ic}$  of 0.25 and  $R_{ic}$  of 0.3 are very small (see e.g., Figures S2-S11 in the Supplement; acronyms TM5 and TM5\_InGOS).

20. P8, line 8 and 14: *What is the difference between ‘222Rn flux map’ and the ‘InGOS 222Rn flux map’ one? Be sure that the ‘abbreviations’ are used properly throughout the text.*

It is the same flux map. This has been clarified throughout the text by using “InGOS 222Rn flux map”

21. P8, line 18: *mBqm-2s-1. Some spaces are lacking in the unit.*

In accordance with our response above, we keep it as it

22. P8, line 30-32: *How can the extraction (or calculation) of variables (model boundary layer heights) be a simulation set-up. See also one of the major comments.*

The extraction of the BLH according to the INGOS definition required some specific modification of the TM5 source code.

The text has been updated and “Simulation set up” has been deleted

23. P8, line 42: *What does 2D interpolation exactly mean? Various 2D approaches exist. Be specific and more accurate here.*

It is linearly interpolated between the grid cell and its closest neighbor (both along longitude and latitude). The text has been updated

24. P8, section 4.1: *Is it really valuable to have so many different definitions? Besides, in this section, I would expect some discussion about the representation of the grid points chosen with respect to reality of the stations as this seems important for your discussion later on (coastal and non-coastal).*

As already mentioned in our reply to the reviewer's major comments (item 7), we now consider only two definitions: “TM5\_INGOS” and TM5\_INGOS\_IGRA” that use the same expression of Bulk Richardson number, as performed for the IGRA data. TM5\_INGOS stands for the boundary layer heights (BLH) extracted at InGOS stations, while TM5\_INGOS\_IGRA is the BLH of the closest IGRA stations. The other model experimental settings are now defined in the Supplement and the relevant results are also shown in the Supplement

25. P9, line 7: *ECMWF can be added as a bullet point.*

This has been deleted in the main paper and put in the Supplement.

26. P9, section 4.2: *it is very unclear what type of simulations have been done. Consider a table.*

As already described above, now we use only two definitions of boundary layer height in the paper. The figures have been revised accordingly and are more readable

27. P9, line 31: *for clarity, at least one bl-profile with the different calculations of bl-height could be shown. Here, also vertical resolution of both IGRA and models can be shown. Besides, you can point out the differences generally found for a nocturnal and daytime (a 00 and 12 UTC) bl-figure, for example.*

As mentioned above, we refer the reader here to Seidel et al. (2012) (and their Figure 1, which clearly illustrates the method). Finally, we do not think that illustrating the vertical bl-profiles for both nocturnal and daytime will help in our discussions on IGRA BLHs and modelled BLHs

28. P9, line 34: *Which mountain stations, and how did you define a mountain station? You could add labels in Table 1. The same holds for coastal and non-coastal stations, it is not defined what they are, this could be labeled in Table 1 as well.*

For mountain stations, we excluded InGOS measurement sites such as e.g., Jungfraujoch and Schauinsland in the analysis. About the coastal sites, see our reply to the reviewer's major comments (item 6)

29. P10, line 4: *“coastal sites”. Why don’t you show a map with the representation of these two stations in the several data points extraction?*

We now show the locations of the IGRA stations associated to InGOS in Figure 1.

Regarding “coastal sites” see our reply to the reviewer's major comments (item 6)

30. P10, line 11: *How are non-coastal sites defined?*

We have not defined objectively “coastal or non-coastal stations” as explained above or in our responses to the reviewer's major comments in item 6

31. P10, line 15: *“probably”. What makes you think probably and not certainly?*

This sentence has been deleted when revised the text

32. P10, line 25: *“relatively” compared to what? And are you surprised by these results? It is well known that Sbls are very shallow, and often these are missed by the model anyway.*

“Relatively” here was about a comparison between nighttime and daytime BLHs. “Relatively” has been deleted

33. P10, line 27: *costal should be coastal.*

Corrected

34. P10, line 31: *As mentioned in previous comments, figure 2, and, in general, ceilometer related data, should be removed in this paper (the correlation is poor and subject to many discussions that are not appropriate to discuss in this type of paper). Furthermore, this figure*



*that shows observations vs. observations does not fit in a section which is called simulations vs. observations?*

As discussed in our reply to the reviewer's major comments (see item 5), we prefer to keep the analysis based on the ceilometer/lidar data, but moving the Figure 2 (submitted version of the paper) in the Supplement (Figure S1 in the revised version). The text has been revised accordingly

*35. P10, line 37: DeBilt should be De Bilt.*

Corrected

*36. P10, Figures 6 and 7 are redundant, see one of the major comments.*

We prefer to keep these two Figures, but they are now in a single Figure (Figure 5 in the revised version). For more detail, see our reply to the reviewer's major comments (item 5)

*37. p10, lines 29 to 45 should be removed. See previous comments on ceilometer data.*

In accordance with our reply to the reviewer's major comments (see item 5) about the use of ceilometer/lidar in this paper, we have kept the content of this part of the text

*38. P11, line 4-5: About the timing of Rn-concentrations (05 and 14 UTC). Does this hold for both summer and winter?*

The chosen reference times should be reasonable for both summer and winter seasons

*39. P11, line 14: The list of coastal stations gets longer throughout the paper. Add it as labels in the paper. Is Cabauw really coastal station? I don't think locals would agree.*

See our reply to the reviewer's major comment (6)

*40. P11, line 24: This could be the start of an additional section.*

Yes. We add a new sub-section: "Relationship between 222Rn activity concentrations and BLHs"

*41. P11, line 30: What do you mean by "Apparently"?*

The term "apparently" has been deleted

*42. P11, line 31: What's a "model world"?*

The sentence has been changed as follows: "The sharp changes in BLHs and 222Rn activity concentrations are due to the relatively coarse temporal resolution of ECMWF meteorological data (3-hourly for surface data (e.g., BLHs) and 6-hourly for 3D fields (temperature, wind, humidity, and convection); see Section 3.1)"



43. P11, line 44: *“This finding...” What finding exactly?*

This “finding” refers to the fact during daytime, the TM5 BLHs are close to IGRA measurements at most stations, while larger differences are observed between  $^{222}\text{Rn}$  activity concentration simulated and observed. The text has been clarified

44. P9-13, Section 5. *what about a station selection for the figures? You seem to overwhelm the reader with graphs and bars, whereas only few things are to be highlighted. For example, the coastal and non-coastal zones are interesting, some are necessary to show due to later analysis with the Rn- and BLH combination. But certainly not all the stations are necessary. Then space would be saved, figures could be enlarged, these would be better readable and the article in general would be better appreciated. All the other redundant stations can then be stored in the supplemental material (which is very large as well).*

See our reply to the reviewer's major comments (item 7). We have considered in the revised version of the paper only few relevant experimental settings. This makes the figures more readable

45. P10-11, Section 5: *Can be better divided in more sections.*

Yes. We add four more sub-sections as follows:

- Relationship between  $^{222}\text{Rn}$  activity concentrations and BLHs
- Sensitivity of simulated  $^{222}\text{Rn}$  activity concentrations to convection scheme
- Comparison of simulated and observed  $^{222}\text{Rn}$  activity concentrations: Impact of sampling time
- Vertical gradients of  $^{222}\text{Rn}$  activity concentrations in the boundary layer at Cabauw

46. P12, line 11: *Add section 5.4 for this paragraph.*

Yes. See our responses above

47. P12, line 38: *“weather conditions”, I suppose you mean “stability regimes”?*

Yes. Corrected and suggested

48. P12, line 42-43: *About CB1 and CB4, see remarks about table.*

The text has been revised. The Table 1 is quoted there, where CB1 and CB4 are defined

49. P13, line 14 (section 6): *this section feels more like a summary than a conclusion.*

This has been revised

50. P13, line 16: *“dynamics” should rather be “height”. The dynamics are not evaluated.*

See our reply to the reviewer's major comments (item 2). Therefore, we prefer to keep the term “dynamics”

*51. P13, line 19: 10-20%, this is the first time I see a statistical value between TM5 and observations. That's very late.*

Figure 11 (submitted version; Figure 8 in the revised version) that summarizes the statistics on BLHs and  $^{222}\text{Rn}$  activity concentrations is now commented earlier (see Section 4.2)

*52. P13, line 23: IGRA observations (not “data”).*

Changed as suggested

*53. P13, line 26: moderate correlation or reasonable? In any case, it is not good and opens up the floor for many discussions that are not relevant to the topic of the paper. See comments before about removing ceilometer related analysis.*

We have kept the ceilometer/lidar observations in the analysis, but this part has slightly been revised

*54. P13, lines 26-35: remove ceilometer related analysis/results*

See our reply to the reviewer's major comments (see item 5)

*55. P14, line 23-24: It is indeed difficult to draw conclusions. Try to be more quantitative, perhaps add more statistics, and this would make it easier to draw conclusions.*

As clearly shown in Figure 11 (submitted version; Figure 9 in the revised version), the performance of the model simulations compared to the  $^{222}\text{Rn}$  activity concentration observations is very similar in term of e.g., root mean square and correlation coefficient for both convection schemes. The statistics are shown in Figure 11 (submitted version; Figure 8 in the revised version)

*56. Table 1. Extend this table with labels as coastal and non-coastal stations. what is ANSTO? What is CB1? CB4? Probably different levels at the tower? The average readers do not have previous knowledge of the dataset and an attempt needs to be made to make it more readable for them. Maybe you want to change CB1 and CB4 to level 1 and level 2. Are the ‘o’ for latitude and longitude degree (o) signs? What does Altitude/Height exactly mean? Do you mean Terrain elevation (Mean Sea Level) and Height (Above Ground Level), respectively?*

ANSTO stands for Australian Nuclear Science and Technology Organisation and was already defined in the submitted version of text (Section 2.2). The Table 1 (including its caption) has been revised (including definition of ANSTO) to clarify the points mentioned in the comments. However, as already mentioned, we do not qualify the stations as “coastal or non-coastal sites”

57. *Figure 1. What are the abbreviations? The authors should refer to Table 1. Some letters are very hard to read, consider another color for either the names, or for the black dots. The black triangles and orange circles are barely visible. Where are the coastal stations exactly? What are the vertical and horizontal lines? Longitude and latitude? It should be indicated on the axis.*

Figure 1 (including the caption) has been revised. Table 1 is now referred

58. *Figure 2. remove. See major/minor comments*

Figure 2 (submitted version) is now put in the Supplement (Figure S1 in the revised version)

59. *Figure 3. Vertical and horizontal axis in the upper diagram?*

They are latitude and longitude.

60. *Figure 4. These figures are very small. Can the whisker plots be centralized around the months to which they are concerning to? And maybe the spacing then between the whisker plots could be enlarged. The scaling on y-axis is not the same. It doesn't have to, but it should be stated in the caption. Although the scales are not very far apart, so probably same axis length would work. This is true for almost all figures.*

Figures 4-5 (submitted version; Figures 3-4 in the submitted version) have been revised. The scaling on y-axis is now the same for nocturnal BLHs and different, but the same for daytime BLHs. In general most of the figures of the paper have been revised, as mentioned above

61. *In the text it is referred to coastal and non-coastal stations. It would be helpful to highlight that in the figure. Is it really necessary to show all stations? Maybe you could make coastal stations fig4a and continental stations in fig. 4b?*

Figure 1 in the revised version helps to have an idea about coastal and non-coastal stations, as already stressed on our reply to the reviewer's major comments (see item 6)

62. *Figure 6/7. Redundant/remove*

See our reply to the reviewer's major comments (5)

63. *Figure 11: remove CEIL/LIDAR data from figure.*

See our reply to the reviewer's major comments (item 5).

64. *Figure 12. What is ratio? TM5 divided by IGRA?*

Yes. This has been clarified. It is now Figure 10 in the revised version of the paper

65. *Figure 13. Abbreviations are not explained well in caption (e.g what is CBI and CB4?).*

*“Mean diurnal variations...” should probably be “Monthly mean diurnal variations...”*

Corrected by “Monthly mean diurnal variations” as suggested.

The acronyms CB1 and CB2 already defined in Table 1. These acronyms are now defined in the caption of Figure 13 (submitted version; Figure 11 in the revised version)

*66. Figure 13: Data points are outside the y-axis range. This should be corrected.*

Yes, but we did so because we wanted to focus on the variations of the vertical gradients of  $^{222}\text{Rn}$  activity concentrations around noon. We now increase a bit the y-axis, but some data points during night and early in the morning are still outside

*67. Figure 14. How can the R of the lower figure be almost the same as the R for the upper figure?*

This has been verified and it is correct. This is fortuitous. This is certainly due to the large values for daytime

### ***References:***

*Pillai, D., Gerbig, C., Marshall, J., Ahmadov, R., Kretschmer, R., Koch, T., & Karstens, U. (2010). High resolution modeling of CO<sub>2</sub> over Europe: implications for representation errors of satellite retrievals. Atmospheric Chemistry and Physics, 10(1), 83-94.*