

Interactive
Comment

***Interactive comment on* “Modelling atmospheric dry deposition in urban areas using an urban canopy approach” by N. Cherin et al.**

Anonymous Referee #2

Received and published: 20 January 2015

Manuscript Review for GMDD-7-8703-1014, “Modelling atmospheric dry deposition in urban areas using an urban canopy approach”

General Comments

This manuscript introduces a dry deposition model for urban areas that accounts for spatial variability in deposition. Deposition is calculated for streets, roofs, and walls for two different flow regimes, recirculation and ventilation, within the urban canyon. The model is presented as an alternative to a roughness length model which cannot account for this spatial variability. The model is coupled to the Polyphenmus air quality modelling platform to predict dry deposition over the Paris region. Additional experiments are done to investigate the model’s sensitivity to key parameters. The paper is well-written, particularly the model description, well-organized, and properly recog-

C3004

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



nizes previous work. The lack of specific observations to compare the model to is a weakness. For instance, I have no idea if the deposition rates and fluxes shown in Figures 11 and 12 are reasonable. However, the difficulties in obtaining representative observations are fully discussed in section 5.1 and so this limitation is perhaps unavoidable.

On page 8707, it is claimed that the dry deposition method presented is “novel”. In reading the following sections it is not clear to me what the novel aspects are. The modeling concept, the local mixing length method, the different flow regimes, and the turbulence schemes are all based on existing work. So it is simply the combination of these that is novel? Does all previous dry deposition modeling work in urban areas use the roughness length approach? Please explicitly state the novel aspects of the approach. Otherwise, I have only a list of fairly minor comments/suggestions below that should be addressed.

Specific Comments

1. Page 8704, line 17: Change “percents” to “percent”.
2. Page 8704, line 19: I suggest listing the original reference for the statement “. . .80% in Europe,. . .”, which is Elvidge et al. (2004), not Oleson et al. (2008).
3. Page 8704, line 23: I’m not sure what “. . .near sources of pollution” means, please clarify.
4. Page 8705, lines 19: A reference or two is needed here to define “dry deposition models” that use “classical approaches”.
5. Page 8706, equation 2: Is “t” time?
6. Page 8706, line 22: By “previous formulation” do you mean Ra?
7. Page 8707: In the discussion about urban parameterizations, I recommend to also reference a more recent analysis/categorization of urban models by Grimmond et al.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

(2010, 2011).

8. Page 8707, line 23: These flow regimes were previously defined by T. Oke and a reference should be added here (e.g., Oke 1987).
9. Page 8711, line 14: “Schlichting et al. 2000” should be “Schlichting and Gersten 2000”?
10. Page 8712, line 6: Suggest changing “lowest grid layer” to “lowest atmospheric layer”.
11. Page 8713, line 10: Change “an hypothesis” to “a hypothesis”.
12. Page 8714, line 26: Change “followings” to “following”.
13. Page 8715, line 1: What is meant by “sparse”? Small h/W? Small plan area?
14. Page 8715, line 14: Change “an harmonic” to “a harmonic”.
15. Page 8715, line 1: I think this is commonly referred to as “plan area index”, not “plane area index”.
16. Page 8717, equation 16: I’m not familiar with the leftmost symbol preceding “z” (also in equation 19). Is this commonly used?
17. Page 8717, equation 16: Has $u(h)$ been defined yet? I assume it is the wind speed at the top of the urban canopy at roof height?
18. Page 8717, line 15: Again, change “plane” to “plan”.
19. Page 8718, line 5: For isolated roughness flow, I think it should be the same as equation 16, except that there is no $2/\pi$ term (see Lemonsu et al. 2004). Please clarify.
20. Page 8718, line 10: Is this coefficient newly introduced here or is it based on another published formulation?
21. Page 8719, line 1: Section 5.5 does not seem to clarify why this parameter “must

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



be chosen as small as reasonably possible”.

22. Page 8719, line 9: What do you mean by “historical”? Perhaps a reference would be better here.

23. Page 8727, line 1: Is “SD” standard deviation?

24. Page 8727, line 12: I’m a bit confused by the introduction of WRF here as providing the “meteorology” for the simulations. On the next page, it is stated that meteorological data are provided with a horizontal resolution of 0.04deg X 0.027deg as implemented in the Polyphenmus air quality modelling platform. Does Polyphenmus run within WRF? Please clarify and provide more details in this section.

25. Page 8728, line 10: When you say that the “results are consistent with the range of measurements reported in the literature”, do you mean the ones you discussed in section 5.1?

26. Page 8728: The equation for relative difference should be multiplied by 100 to be consistent with what is shown in Fig. 9 and discussed in the following paragraph (%).

27. Page 8728, line 20: It is stated that the “annual-average difference is about 45% with a SD of 15%”, but that this is not shown? I thought that Fig. 9 is showing the annual-average difference. Are you providing an average value over the region? If so, how can it be that high when the highest values shown in Fig. 9 appear to be ~36%. Please clarify.

28. Page 8728, line 24: Fig 10 is introduced but then not discussed. It should be. Also, does this represent an area-averaged value over the Paris region? More detail is needed in the caption (also in Fig 11 caption).

29. Page 8734, line 17: A reference for an example of a multi-layer model could be provided here, e.g., the Martilli et al. multi-layer model implemented in WRF.

30. Page 8734: What are the plans for future use of this model? Are there any plans

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



to adopt this model in an operational sense, e.g., as a permanent component of the Polyphenmus air quality modelling platform?

31. Page 8737: Is Jonsson et al. (2008) referenced in the text?

32. Figures 1, 2, 3 captions: It would be useful to reference what types of flows these refer to, i.e., skimming, wake interference, isolated roughness, respectively.

33. Figures 10, 13, 14, 15: The lamdas here should have a “p” subscript.

REFERENCES

Elvidge, C.D., and Coauthors, 2004: Urbanization. Land Change Science: Observing, Monitoring, and Understanding Trajectories of Change on the Earth’s Surface, G. Gutman et al., Eds., Kluwer, 315-328.

Grimmond, C.S.B., et al., 2011: Initial results from phase 2 of the international urban energy balance model comparison, Int. J. Clim., 31, 244-272, DOI:10.1002/joc.2227.

Grimmond, C.S.B., et al., 2010: The International Urban Energy Balance Models Comparison Project: first results from phase I, J. Appl. Meteorol. Clim., 49, 1268-1292, DOI:10.1175/2010JAMC2354.1.

Oke TR (1987). Boundary Layer Climates. 2nd ed, Routledge

Interactive comment on Geosci. Model Dev. Discuss., 7, 8703, 2014.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

