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Geoscientific Model Development Discussions

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

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We thank both reviewers for their interest in our manuscript and their attentive review of our text. Please find below our answers to the second reviewers'questions and remarks.

### General comments

**R#2**: 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.

A: The deposition velocities are on the same order of magnitude with the roughness model and the one we propose. We then expect the dry deposition fluxes simulated with our model to be as realistic as the ones resulting from the roughness model. Unfortunately, there is no experimental study available to provide a quantitative evaluation of the flux distribution among the roof, wall, and street surfaces.

**R#2**: 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.

A: The sentence was reworded to emphasise that the combination of the concepts and their application to the transfer of mass of pollutants are new, but not the concepts themselves. The limitations of the roughness length approach for the urban environment are clearly identified, but we do not have any knowledge of another existing approach in regional air quality models.

"The combination of previously existing concepts allows us to propose here a novel approach to model dry deposition of atmospheric pollutants in an urban canopy. It is based on ..."

### Specific comments

**R#2**: Page 8704, line 17: Change "percents" to "percent".

A: Done.

**R#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).

A: Done.

**R#2**: Page 8704, line 23: I'm not sure what ". . .near sources of pollution" means, please clarify.

A: This sentence simply suggests that the dry deposition flux, assumed to be proportional to the atmospheric concentrations near the ground, is higher in the vicinity of sources where the dilution of atmospheric pollutants remains limited. This point was not really useful for the understanding of the model formulation and has been removed.

**R#2**: Page 8705, lines 19: A reference or two is needed here to define "dry deposition models" that use "classical approaches".

A: Done. The reviews of Wesely and Hicks (2000) and Petroff et al. (2008) are now cited.

**R#2**: Page 8706, equation 2: Is t time?

A: Yes, this clarification was added.

**R#2**: Page 8706, line 22: By "previous formulation" do you mean Ra?

A: The "previous formulation" refers to the formulation of $v_{\mathrm{d}}$. The sentence was clarified.

**R#2**: 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. (2010, 2011).

A: Done. The two references were added.

**R#2**: Page 8707, line 23: These flow regimes were previously defined by T. Oke and a reference should added here (e.g., Oke 1987).



A: Done.

**R#2**: Page 8711, line 14: "Schlichting et al. 2000" should be "Schlichting and Gersten 2000"?

A: Yes. The assistants and the translator were "partly" cited with the authors.

**R#2**: Page 8712, line 6: Suggest changing "lowest grid layer" to "lowest atmospheric layer".

A: We prefer to keep here our original wording. It seems important for us to recall that the numerical model is formulated within a discrete environment.

**R#2**: Page 8713, line 10: Change "an hypothesis" to "a hypothesis".

A: Done.

**R#2**: Page 8714, line 26: Change "followings" to "following".

A: Done.

**R#2**: Page 8715, line 1: What is meant by "sparse"? Small h/W? Small plan area?

A: It corresponds to small h/W. The sentence was modified.

"If the urban canopy has low building density,..."

**R#2**: Page 8715, line 14: Change "an harmonic" to "a harmonic".

A: Done.

**R#2**: Page 8716, line 1: I think this is commonly referred to as "plan area index", not "plane area index".

A: "Plane" was replaced by "plan" in different expressions. Indeed the expression "plan area index" is often used for $\lambda_{\mathrm{p}}$ (but not for instance in Grimmond et al., 2010, where four alternative names are proposed). We prefer to keep a more explicit wording but mention this common wording.

**R#2**: Page 8717, equation 16: I'm not familiar with the leftmost symbol preceding "z" (also in equation 19). Is this commonly used?

A: Yes, it is a rather commonly used symbol in mathematics to say "for any" (see for instance http://en.wikipedia.org/wiki/Universal_quantification).

**R#2**: 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?

A: Indeed. This is now explicitly mentioned.

**R#2**: Page 8717, line 15: Again, change "plane" to "plan".

A: Done.

**R#2**: 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.

A: We believe the equations are correct. Our text mentions Eq. (16), which corresponds to what is prescribed for isolated roughness flows in Appendix B, i.e., Eq. (B2) in Lemonsu et al. (2004). In our manuscript, the 2/pi term only appears in Eq. (19).

**R#2**: Page 8718, line 10: Is this coefficient newly introduced here or is it based on another published formulation?

A: This coefficient is only introduced to provide a more convenient writing of Eq. (38) and Eq. (39). It is now explicitly mentioned.

**R#2**:21. Page 8719, line 1: Section 5.5 does not seem to clarify why this parameter "must be chosen as small as reasonably possible".

A: Section 5.5 only mentions that the determination of $v_\mathrm{d}$ is not very sensitive to the chosen value of $\Phi$. A too high value for $\Phi$ would correspond to the assumption of a logarithmic profile for a large part of the urban canopy. This is now stated.

"where $\Phi \in [0, 1]$ is a dimensionless parameter, which must be chosen as small as



reasonably possible since a too high value for $\Phi$ will correspond to the assumption of a logarithmic profile for a large part of the urban canopy. The sensitivity of $v_\mathrm{d}$ to the chosen value of $\Phi$ is discussed in Section 5.5."

**R#2**: Page 8719, line 9: What do you mean by "historical"? Perhaps a reference would be better here.

A: It corresponds to Eq. (1). The reference Gregory (1945) is now recalled.

**R#2**: Page 8727, line 1: Is "SD" standard deviation?

A: Yes. The full name is now provided.

**R#2**: 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 $0.04 \times 0.027$ as implemented in the Polyphemus air quality modelling platform. Does Polyphemus run within WRF? Please clarify and provide more details in this section.

A: The meteorological inputs are obtained from simulations conducted with the WRF model. However the simulated meteorological fields are not directly used to compute the dry deposition velocities. There is a preprocessing step to interpolate the fields from the WRF discretization grid to the Polyphemus discretization grid. The dry deposition velocities are computed for both models within the Polyphemus discretization grid. The paragraph was slightly reorganized to clarify this point.

"The dry deposition model presented above was implemented within the Polyphemus air quality modelling platform (Mallet et al., 2007). The roughness length model based on Zhang et al. (2001) was already available in the Polyphemus platform. The meteorological fields are interpolated from the WRF discretization grid to the Polyphemus one. After this preprocessing, meteorological data are provided with a horizontal resolution of $0.04 \times 0.027$ every hour."

**R#2**: 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?

A: Yes. Taking into account the reviewer 1 recommendation, this section has been moved to an appendix. An explicit reference to the new appendix was added.

**R#2**: 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 (%).

A: Done.

**R#2**: 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 $\sim$36%. Please clarify.

A: Indeed Figure 9 shows the relative difference of the annual averages and does not correspond to the given numbers. The given numbers correspond to the average of hourly relative differences for a chosen grid cell with a high ratio of urban coverage. This explanation was missing. This point is now clarified and the Figure 9 and the equation were modified to be more consistent with the numbers provided.

" The differences are computed for each hour, then they are averaged over the year 2011. The mean over all the fully urban grid cells (100% of urban coverage) of the annual-average of the hourly relative differences is about 45 % with a mean standard deviation (SD) of 18 % (not shown). This mean difference reaches 82 % for $\lambda_p = 0.6$ with a SD of 26 % (not shown)."

**R#2**: 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).

A: A reference to the discussion about the sensitivity of the deposition velocity to the



building density in Section 5.4 was added. The caption of the figure mentions that the plot corresponds to one urban grid cell. This clarification is now also provided in the text. The caption of Figures 11 and 12 have been modified accordingly.

**R#2**: 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.

A: The proposed reference was added.

**R#2**: Page 8734: What are the plans for future use of this model? Are there any plans to adopt this model in an operational sense, e.g., as a permanent component of the Polyphemus air quality modelling platform?

A: The model will be integrated as an option in the Polyphemus air quality modelling platform. We plan to address some of the points mentioned in our conclusion in the near future to make an operational version of the model available to the users'community.

**R#2**: Page 8737: Is Jonsson et al. (2008) referenced in the text?

A: This reference was added in the introduction section. It corresponds to an interesting CFD study that shows the heterogeneity of the deposition in urban environments. Another reference, Maro et al. (2014), was also added.

**R#2**: 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.

A: Done.

**R#2**: Figures 10, 13, 14, 15: The lambdas here should have a "p" subscript.

A: Done.

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

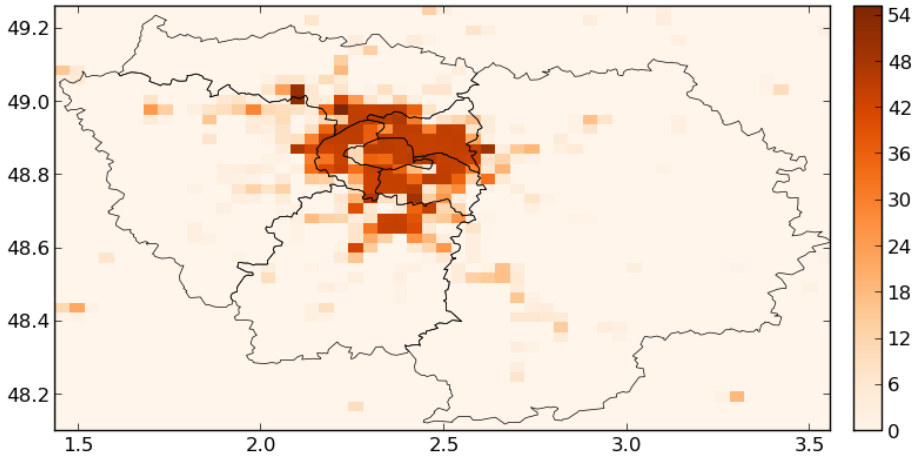


**Fig. 1.** Annual-average over 2011 of the hourly relative difference (in %) between the urban canopy and the roughness-length models in the Paris region.