Point-to-point response to comments by Stijn Janssen

[Comments are in regular font and answers are in bold font.]

We thank both the reviewers for their positive and constructive comments, and finding the article interesting and worthy of publication. We have carefully considered their suggestions and necessary changes have been made to the manuscript. Our detailed responses to their comments are provided below.

The authors have presented an interesting manuscript in which they describe the im-pact of an inhomogeneous emission scheme for the widely used street canyon model OSPM. After a brief theoretical description of the OSPM model and the way the inho-mogeneous emissions are implemented, the updated model is tested with .eld data for two Scandinavian cases. Validation statistics are convincing and show that the im-proved model is able to better describe pollution dispersion from asymmetric emission sources in a canyon.

I can recommend publication ones my comments below are properly addressed.

Specific comments

-P4, line 5-9: It is unclear why CO is mentioned here. CO is not discussed at all further on in the text. So clarify or remove the paragraph at all.

Upon a closer thought the section about CO should be removed. When validating air pollution models it can be a good idea to look at several species, but in this case it will only point to the validity of the emission model. Therefore it will be removed in the final version of the manuscript.

-P4, line 30: a multiplication factor of 4.2 is mentioned. This seems to be a very big factor to correct for a bias. Or this is a mistake, of the sever underestimation should be discussed in more detail.

The exact technical reason for the factor 4.2 is not known, but the important thing is that we know that we can correct for this bias. Firstly we have the manual checks of the number of vehicles in each lane, and then we have compared the counts reported by the Marksman counter with automatic camera recordings in a campaign during the fall of 2009. The automatic camera recordings are described in the manuscript, but we will add that we also compared the camera recordings and the Marksman counter and found excellent agreement using the factor 4.2.

-P5, line 13: Would be interesting to mention here already the differences in emission for uphill/downhill driving to have an idea about the size of the effect.

Since this article is not about emission modelling, the emissions are not presented. However, Gidhagen et. al. quotes other similar studies where the uphill emissions are 3-4 times larger than the downhill emissions. These studies will also be quoted here in the revised manuscript.

-P6, line 16: the mean value of 2.27 seems to be higher than the values in Fig. 2. Please check or clarify.

The number given in the text is the median value whereas the numbers in the figure are the mean weekly and diurnal variation in the mean value. This will be clarified in the final version of the article.

-P8, line 22: the statement "... from inside the recirculation zone..." seems to be incorrect when the recirculation zone covers the whole canyon? Please clarify.

When the recirculation zone covers the entire canyon both receptors are exposed to the emissions inside the recirculation zone, however, the windward receptor is only exposed to the recirculating contribution whereas the leeward receptor is exposed to both the direct and the recirculating contribution. This will be clarified in the text.

-P9, line 3: "h0, is the initial dispersion" seems to be a strange definition. It is at least a height. The height due to the initial dispersion?

The plume is assumed to disperse initially to the height h0 regardless of the spatial position in the canyon. The height of the emission plume will subsequently increase linearly above h0 downwind of the location of the source. H0 can thus be said to be the height of the plume in the wake of the cars. This will be clarified in the article.

-P9, line 27: it seems more logical to me to express the criteria "f_ext is greater than zero" as a function of "Theta_street". Is this possible?

Yes. This means the definition rather than being "close to parallel" will be "Theta_street<45"

-P10, Eq 2: u_b is not defined.

U_street used to be called u_b in earlier versions of the model. This is a mistake in the manuscript and will be corrected in the final version.

-P10, Eq 9: it is unclear how the remaining parameters in the equation are defined. I believe they are completely fixed by the street geometry and the wind vector. Please mention this for the sake of clarity.

The remaining parameters will be defined in a table in the revised version

-P10, end: after those two sections 3.1.1 and 3.1.2 it is still not fully clear from the equations why the leeward receptor in a canyon receives more than the windward site. A brief discussion summarizing the principle ideas of the OSPM formula would be very instructive. Not all readers are familiar with the Berkowicz et al paper.

This will also be added in the revised version.

-P11, Eq 10 -12: I'm confused here. The model user defines the W_i values based on street (lanes) geometry. But if the W_i bands do not match with the dynamic L_rec, x_esc,... values, the limits of the sum in Eq 10 -12 are not determined by the dynamics of the canyon flow (e.g. e_esc, x_end). This seems to be a fundamental issue in the new scheme. Or I still don't fully understand the newly proposed scheme.

This is a good and valuable point. The limits created by the dynamics of the canyon flow (x_esc, L_rec etc.) are in the implementation treated like any other segment. In order to handle this, an "artificial segment limit" is inserted at these points. Subsequently the impact of the different "artificial" and user-defined segments can be calculated with the corresponding equation. This will be clarified in the revised version without going into too many calculation details.

-P11, line 26: Why does the integration length approaches zero for parallel wind? Can you make this visual in a figure?

It should say that this is only for a canyon with segments with zero emission at the sides. This will be illustrated with a figure.

-P14, line 15-17: It seems to me a poor argument that the bad performance is due to a previous calibration. Is this something that can be solved for this study?

In the present study the most up-to-date emission input for Jagtvej is used. This emission input is markedly different from the emissions used in previous validations of this model against this street. Previously some model parameters (e.g. fRoof) that had high degree of uncertainty were used to tune the model against the measurements. For this study, no further tuning of the model has been performed, since this would distract the focus from the inhomogeneous emissions. The poor model performance with the new emissions is an ongoing area of research and it was considered to be outside the scope of the present article to solve this issue as well.

-P14, line 22: Nothing is mentioned about the mismatch at 100. and 250-360. . In general, this case Jagtvej is rather poorly discussed compared to the first one. Some more discussion about model performance would be welcome.

The mismatch at 100 is probably a result of a random error since this spike in the curve is not seen when comparing the wind direction for the full diurnal cycle. The mismatch at 250-300 has been reported in previous calculations and is caused by an opening in the street canyon at this wind direction. The cause of this deviation has not been examined before and was assessed to be outside the scope of the present study. These discussions will be added in the revised manuscript.

-P14, line 24: Please give a short introduction and motivation (1-2 lines) why the theoretical calculations are added to the analysis.

The theoretical calculations are added to illustrate the impact of inhomogeneous emissions alone without the confounding effects of the emission model, the aerodynamics of the street etc. This will be added to section 2.3

-Fig 5: An additional schematic figure comparable to Figure 5 but for another recirculation zone would be very useful to understand the general principles of OSPM. Not all readers will be familiar with the Berkowicz et al paper. Please also illustrate (if possi-ble) the lengths x_start, x_end, x_esc, W. The definition of L_b in the caption is very unclear (it is the distance to the next corner, I suppose?).

This is a useful comment. We will improve Figure 5 and the description in Table 4 in the revised version.

-Fig 13: Complicated graph and difficult to analyse and understand. The authors could consider to only show 2 sets (e.g. 50/50 and 70/30) since the general trend for the other fractions is similar and does not add new information. Probably this will simplify the graph and result in a better interpretation.

The graph will be simplified in the revised version

-Fig 13, caption: the description of the setup of the theoretical exercise should be given in the text, not in the caption.

This will be corrected in the revised version.

-Fig 15 and Fig 15, caption: same comments as for Fig 13. Further, it is not fully clear what the difference is between solid and dashed line. I suppose leeward and windward, but this should be mentioned explicitly in the caption.

This will be corrected in the revised version-

-Table 4, caption: All definitions should be given in the text and not in the caption! Further, I notice that some of the symbols are mixed: eg. Theta and Theta_street although they refer to the same physical quantity, I suppose. Please make sure that only one consistent set of symbols is used throughout the text.

The definition of the lengths will be added to the table. There is a missing symbol for the street level wind direction (right now called theta). Moreover, there is a missing definition of theta_l. These will be corrected in the revised version.

-Table 4, x_start definition. As far as I understand x_start = 0 if h_r < h_0. If so, please include in the denition.

This will be corrected in the revised version.

Technical corrections

-P3, line4: Clarify “this” in “... of this type of model...”. It is unclear to what model you

refer.

This will be clarified in the revised version.

-P3, line 22: replace “Figure 1” by “Table 1”

In the word file and in the pdf on the webpage it says “table 1”

-P3, line 28-29: update reference to Sect. 0

In the word file and in the pdf on the webpage is says “section 3.2”

-P5, line 7: “classified” or “categorized” seems to be a more appropriate term than

“harmonized”.

“scaled” is probably a better term. This will be used in the revised version

-P5, line 14-16: Sentence is difficult to understand. Please reformulate.

This sentence will be rephrased in the final version

-P5, line 21: update reference to Sect. 0

In the word file and in the pdf on the webpage is says "section 3.2"

-P9, line 1: should "receptor" not be replaced by "source" in this sentence?

No. What is meant is the distance from the receptor into the area source where a plume originating would reach the receptor height. This will be clarified in the revised version.

-P9, line 25: "parallel" should be replaced by "perpendicular", I suppose?

No. The integration length is extended for parallel wind directions to account for the contribution to the lee side receptor from the segment outside the recirculation zone.

-Fig 1 & 2: add the location of the data set to the caption.

Both figures are data from Hornsgatan, Stockholm. This will be added to the captions in the revised version.