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Geoscientific Model Development Discussions

Interactive comment on "Evaluation of roadway Gaussian plume models with large-scale measurement campaigns" *by* R. Briant et al.

Anonymous Referee #2

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General comments

The paper is well-written so it is easy to read and it presents a useful development for an algorithm for an operational model of a line source. The description of the algorithm is the strongest part of the paper. The description of the comparison with diffusion tube data and with other models begs many questions. Hopefully the questions can be answered by some additions to the text based on existing materail, although some questions reveal weaknesses in the validation and model comparisons.

Specific comments

I agree with the comments of the other referee on: the abstract, Romberg integration, Model error and Comparisons to the HV formulation. Section 1.

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The time resolution of the background concentration and meteorological data is not given, is it hourly?

The measurement height, location with respect to the kerb and hence the classification (kerbside, roadside etc) of the diffusion tubes should be given.

There is no mention in the paper of treatment of non-road emissions and emissions from roads other than those modelled explicitly. These should not be neglected.

Section 2.2

The choice of 89° is arbitrary and would be expected to lead to over-estimates by the HV formulation. P3354 line 23 says this is the case but Figure 3 shows that HV under-estimates compared with Polyphemus when winds are close to parallel to the road.

Section 2.3

The nature of the "correction functions" and their effect should be described as they are the key difference between HV & Polyphemus.

Section 3.1

COPERT 3 would be expected to under-estimate NOX and NO2 emissions from certain vehicle classes: âĂć NOX emissions from diesel cars under urban driving conditions do not appear to have declined substantially up to and including Euro 5. There is limited evidence to suggest that this same pattern may occur for motorway driving conditions; and âĂć NOX emissions from HGV vehicles equipped with SCR reduction are much higher than expected when driving at low speeds.

There is no mention of whether diffusion tubes were co-located to assess accuracy and co-located with automatic monitors to calculate a local bias-adjustment factor. Properly verified and bias-adjusted diffusion tube results would be useful for comparing annual averages. 1 month measurements from diffusion tubes are not going to provide very good data (absolute values) for model validation, but are useful in showing spatial

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variation. Here we see the spatial variation (Figure 2) and we see that the model is not capturing the range of values, see comment under section 3.2.

I don't understand why a primary NO_2 value of 10% was considered for the base case as it was obviously an under-estimate.

Section 3.2 The "rural" and "urban" options of the HV and Polyphemus models are not explained. It's puzzling that the "urban" option does not apply in the suburbs of Paris. The RMSE is very large compared with the mean and what the scatter plots show is the model not capturing the monitored range in concentration, that is presumably due to variations in receptor siting and the range of traffic flow.

No mention has been made of whether canyon effects and other local effects have been considered. These are well known to be important when modelling near road concentrations.

Section 3.3

No reason is given for using a smaller domain for the comparison with ADMS-Urban, nor how the subset of 62 receptors compared with the whole set of 242 (proportion of kerbside and roadside sites).

The study mixes up differences in meteorological processing, chemistry and fundamental dispersion algorithms with the line source representation. The effect of chemistry scheme could have been excluded by using the NO_X/NO_2 post-processing for ADMS-Urban. There is a comment on NO_X concentrations but this needs to be backed up with tables and figures. It would be useful to run a baseline comparison between ADMS-Urban and Polyphemus modelling a line source perpendicular to the wind direction, and then a parametric study varying the wind direction so that the effect of the line source formulation alone could be identified.

As the meteorological data actually used was not spatially varying it would have been much better to use hourly data from a synoptic station. Similarly, it would have been

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much better to use hourly monitored rural and urban background concentrations rather than modelled value. These steps would have reduced uncertainty in the model inputs.

Section 3.5

This section describes major differences between HV & Polyphemus and ADMS-Urban. The former use broad categories of stability and only use wind speed in post-processing results. This is a major difference in the fundamental dispersion algorithms and should have been identified separately as shown above.

Although ADMS-Urban was run on a different PC, as computer run time is given as a key, determining factor at a couple of points the run times and computer specifications should have been reported, so the reader has the information.

Section 3.6

If you are going to compare modelled and values from a dedicated monitoring campaign, it is not sufficient to pick up a bias adjustment factor from the literature. A local bias adjustment factor or factors should have been determined as part of the campaign.

Section 4

I recommend deleting this late, unsubstantiated comment on the performance of Polaris3D, that has only been mentioned once previously in the paper. With a resolution of 5km it is not “fit for purpose” as a model kerbside and roadside concentrations so I don’t understand why that comparison was made. Your point surely is that you intend to embed the new line source algorithm in the Polaris3D model which has a horizontal resolution of 5km.

Tables 1-3.

The mean values should be labelled as “monthly mean values of NO2”.

Figures 4 & 5

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Should there be some data points?

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