

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

### **Anonymous Referee #1**

Received and published: 27 November 2012

### **1 General comments**

This paper describes a Gaussian plume model developed for line sources and its evaluation in a real-case study with a full set of near-road measurement data of NO<sub>2</sub> at regional scale. The model takes into account the chemistry with a simplified scheme. It also contains numerical corrections for cases when the wind direction is parallel to the road section. The modelling results are compared to measurements as well as to results obtained with another model, ADMS, and discrepancies are discussed. The computational efficiency is also discussed, which is a question of major interest as far as operational modelling tools are concerned.

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)

The modelling approach is interesting and clearly explained, and the model-to-data comparisons give good results. The reasons for discrepancies between models (ADMS-urban and Polyphemus) should be more deeply investigated, as I detail below, and the influence of using heterogeneous input data instead of domain-wide averaged values could have been further discussed. Otherwise, the paper is clearly written and well structured. It presents interesting model-to-data comparisons and technical discussions, and is worthy of being published, provided minor revisions are carried out as detailed below.

## 2 Specific comments

**Abstract** The authors should be more specific when giving “error” values in their abstract: which indicator does this refer to? Is this the mean fractional error? Please give the indicator name in the abstract for more clarity.

**Romberg integration** Why is the “Romberg intergration” mentioned in the abstract, since it is not further detailed in the paper? I think it is not worth mentioning in the abstract. It should be detailed a bit further in Section 2.3, at least to explain what this method does. Is it specific to the Polyphemus model? Is it an existing numerical method, and if so, where are the references? Why using this method in particular? In what way is it better, more efficient, or different from what is done in other models?

**Emission rates** Is there a reference for the COPERT 3 methodology? If so, it should be included in the bibliography.

**Meteorological data** 3D nested meteorological data are used for this study. However, domain-wide average values are used for meteorological data. I think the authors should mention how this could impact their results. What would be the result if they

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

used the meteorology given in each source cell instead of a domain-wide average? What does “domain-wide” mean when three nested domains are used? I suppose it is the smaller one, but it is not indicated how big this domain is, compared to the road network shown in Figure 1. The meteorological data used should be summarized here, even if it was in another reference, at least with the domain sizes and the height of the first vertical level.

**Background concentrations** Same question for background concentrations. 3D data are generated, but values at two stations are used. Thus, why using modeling data and not observations? The 3D input data seem under-used here. In any case, I think the authors should mention the background values used in their simulations in the result tables, to see what part of the concentrations is actually due to the road model. Maybe including results with Polair3D alone would be a good idea, since it is mentioned in the conclusion.

**Model error** Again, the authors should be more specific when discussion the model “error” in the text, and should explicitly refer to the indicators given in the tables. For instance, p.3352, to what indicators does the sentence “the model error was similar but the model underestimation was slightly larger” refer? To what indicators and what table do the values (−33% and so on) correspond? This is not clear.

**Comparison to ADMS-urban** This part of the paper is not very satisfying, since no real explanation is provided as to why the discrepancies in the results are so large. What differences in the chemical schemes could explain the results? The authors should be more specific about that. There is no mention concerning the distance between the roads and sensors, but I suppose it is within a few meters from the roads, to measure a significant difference with the background concentrations. If this is the case, then the dispersion parameters such as initial source height and dilution, and

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

also the wind used in both models, are very sensitive since the plume is very small when it reaches the sensor. The authors should definitely discuss these issues. The ADMS technical documentation is detailed enough to help infer some of these points. For instance, it probably uses an initial dilution to model the turbulence due to traffic, which means that the plume vertical dilution is higher and concentrations are lower. Besides, how is the road width modeled in ADMS simulations? Is the source located at the center of the road? Could the ADMS results be reproduced with Polyphemus on a given road by changing the source height, position or initial dilution? The fact that the ADMS values are close to background values is very strange. It might be that the plume is higher and does not touch the ground at the receptor point, or that the source is further upwind and the plume is “older” when touching the receptor, and chemically close to the background. Moreover, ADMS uses a meteorological preprocessor to recompute vertical wind profiles. Did the authors verify what wind values are actually used by ADMS in the simulation?

**Comparisons to the HV formulation** Both formulations give similar results on the case study presented here, since measurements are integrated on one month. It would have been interesting to have time series measurements near roads to provide a better model validation. Do the authors plan to carry out such comparison ? Since the wind is not always parallel to the road, and a particular receptor is often influenced by several adjacent roads (which could compensate some errors), the results presented in this paper tend to indicate that the correction for parallel wind might not be essential. Increasing the computational time by a factor ten should be justified by a better accuracy in the results, which cannot be concluded with this study alone. However, as pointed out by the authors, a compromise might be found by decreasing the number of source points used in the discretized case.

**Sensitivity study** A column labeled “Reference case” or similar should be added in

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

Table 3 for more clarity, to simplify the comparison between the results given in Table 2 for Polyphemus and the two sensitivity cases. The use of Monin-Obukhov length to compute stability class is investigated, but not the sensitivity to the meteorological data in itself. In particular, I don't think the use of Monin-Obukhov length to compute the discrete stability class should be labeled as "more detailed meteorological information" (in the conclusion). Using a meteorological preprocessor to recompute vertical wind profiles, or a different value than a domain-wide average, would be a more significant change. Besides, the sensitivity to the dispersion scheme in itself is not assessed. When using the Monin-Obukhov length for stability, it might have been more consistent to use similarity theory for standard deviations as well, instead of Briggs formulae.

**Conclusions** The conclusion is a bit dense and provides information that is not shown elsewhere in the paper, such as model evaluation criteria, or results with Polair3D alone. I think these two points should be mentioned in Section 3.2, and briefly summarized in the conclusion.

### 3 Technical corrections

- p.3345, "stationarity and homogeneity" should be outside the brackets,
- p.3346, I would replace "indeed" by "included" or "taken into account",
- p.3347, Section 2.2, the first sentence is too complicated and should be reformulated,
- p.3356, Section 3.5, only five stability classes are given instead of six ("E" missing)