Answer to reviewer n.1 of the paper “Stochastic fields method for sub-grid scale emission heterogeneity in mesoscale atmospheric dispersion models” by Cassiani M, Vinuesa J. F., Galamarini S. and Denby B.

We thank the reviewer for the useful comments. Specific answer follows after the unabridged comments by the reviewer (reported in italics):

1) *The first paragraph of Introduction is a little biased. It gives the impression that generally emission inventories are much more detailed than the atmospheric model resolution and that the emission inventory techniques are more advanced than atmospheric modelling methods. I think these statements need to be toned down because I do not think that it true. Atmospheric mesoscale models can now resolve hundreds of metres, but on the other hand emission uncertainty with associated coarse resolution is still a big problem in air quality models.*

It is not our intention to suggest that emission inventories techniques are more advanced than atmospheric modeling methods. What we want to point out in the introduction is that if emissions are available at resolution higher than the maximum grid resolution achievable by the simulation, this additional information is completely lost due to the lack of methods for handling sub-grid processes in dispersion models. To avoid any possible misunderstanding we have partially rewritten the introductory sentences. We agree with the reviewer that mesoscale models can currently resolve hundreds of meters, but this only when limited domain size and short integration time are considered. In most simulations air quality models work with resolution ranging from few to hundreds of square kilometers. Regarding the uncertainty in the emission inventories it is surely a serious problem that requires constant attention and it is obviously major source of uncertainty for transport models, but we believe that this is a different issue with respect to the one investigated here. More specifically the method proposed here (and the one proposed in Galmarini et al. 2008) attempt to make the best possible use of the available information on emission heterogeneity, while presently this information would be completely disregarded independently from its quality. In the unlikely event that tomorrow an uncertainty-free emission inventory would be presented, models would still not be able to describe the sub-grid emission heterogeneity. We consider this an important passage of the paper, necessary to create awareness with respect to an existing problem that the increase of grid resolution will not solve if not in a long distant future.

2) The approach followed by the authors uses the IEM technique. Cassiani has also previously used the IECM (Interaction by Exchange with the Conditional Mean) technique, which is supposed to be superior, and I wonder why this technique was not followed in the present work. Some reasons need to be given, and whether the results would have been different.

The reviewer is right, Cassiani and coauthors used in several works the IECM mixing model since it allows further physical consistency with respect to the IEM model (e.g. Cassiani et al. 2005a). However, the application of the IECM model in the context of Lagrangian stochastic dispersion models is quite "natural" while its use in the present case would require modification of the mesoscale meteorological model (to generate the conditional averages necessary to the use of the IECM model). The IEM mixing model technique has been chosen for its simplicity, since it uses unconditional average, and can therefore be used with only small modification to existing CTM codes and without the need of any modifications to the meteorological models. It is difficult to predict which difference in the results could be generated by the use of the IECM model, also considering that the meteorological model would be modified. In general the more consistent mixing model gives better results but the difference can be quite limited. An explanation of the choice has been added to Section 3.1 after equation 2.

3) Is there a way of validating the model results with real-world data? In the paper, an idealised emission distribution is used, and its representativeness to the field situation is uncertain.

The simulation of real-world measurements is the final goal of any modeling work. However, we are proposing here a new modeling method. The simulations of real-world data, entailing the influence of many uncontrolled factors, would have hindered the correct understanding of the results and it is beyond the scope of the present work. We believe that starting the process of model validation with simulations of simple and controlled situation is the only way to correctly evaluate the approach and we consider the source configuration chosen to be the most fundamental among the possible sub-grid source configurations. It should also be considered that the only existing work on this topic is that of Galmarini et al. (2008) that tackled the problem from a completely different perspective. Therefore, we thought that comparing the two methodologies was a minimum requirement and an obligatory step in the process of validation of the current methodology.

The simulation of more complex, but still synthetic, sources configurations is currently under study and will be a good test of the ability of the model to simulate real-world data. The final validation of the model with real-world situations will need measurements of second and higher order concentration moments.

4) As far as I can tell, the paper does not mention as to what kind of atmospheric flow was simulated. Was it convective? Would there be bigger differences as a result of emission heterogeneity in other boundary layers?

The present simulations are for convective condition and it is now explicitly stated in the paper. The simulation of neutral condition is expected to provide similar results and agreement with LES simulations. The applicability of the present method for simulating very stable conditions would need a thorough investigation and it is not straightforward.

5) *I find the whole of Section 4 too verbose, and it will be good to cut down on unnecessary text, to keep the reader interested.*

We believe that the whole content of section 4 is necessary to correctly understand the paper and reducing the explanation could compromise the clarity of the presentation.

Technical corrections

Fig. 1: The grids labelled E and F are not used anywhere in the paper, so these labels should be deleted.

We would like to keep the labels for consistency with the previous paper of Galmarini et al. (2008). Moreover they facilitate a discussion that has been added regarding the asymmetry of the configuration due to the wind rotation.

Figures, especially 2-4: There is a lot of wasted space in these figures. I suggest using log scale along the x-axis in these plots (may be in all figure from 2 onwards).

The use of the linear scale is consistent with the previous paper by Galmarini et al. 2008. The use of the log scale would amplify the visual weight of very low concentration values that are of relatively low importance. We prefer the use of this linear scale since it allows the reader to better focus on the grid cells and elevations with the higher concentration values, which are more relevant.

Section 4.5, 3rd para: ': : :these issue' should be ': : :these issues'.
Ok

Section 4.4 and elsewhere: I think Skewness should be skewness.
Ok