Answer to reviewer n.3 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 comments and suggestions, specific answers follow after the unabridged reviewer comments (reported in italics).

Chemistry transport models have to deal with simplified descriptions. The neglect of small-scale emission fluctuations (e.g. caused by highways, localized industry) is one of these simplifications. This paper investigates ways to account for this sub-grid scale variability. The specific solution proposed is to do an ensemble simulation in which different concentration fields are created. Each concentration field has a different emission, and as such the ensemble of fields represents the concentration fluctuations that are expected from sub-grid-scale emission variability.

The paper is well written, and the theory is well introduced. However, to be appreciated by a wider audience, some additions are recommended, which are listed below.

1) The effects of the stochastic forcing and the Tmix term remain unexplored. Obviously, leaving out of modifying these terms will change the results and it would be interesting to investigate the effect of these terms on the comparison with the LES results. The effect of the stochastic term influences the vertical transport (how?), which currently seems a bit too fast in the RANS-SFM model (figure 2).

The role of the random and mixing terms is briefly discussed in the paper (page 15220 line 21) and more extensively in the reported references. These terms are fundamental for correctly interpreting the Fokker-Planck and the equivalent SPDE as a modeling of the exact PDF transport equation. The role of these terms in a PDF transport equation is extensively discussed in the literature (see e.g. the references in the paper). Leaving them out would decrease the physical consistency of the approach. We remark here that the stochastic term should not significantly influence the vertical transport but only the turbulent production of scalar fluctuations which are then dissipated by the mixing term at a rate controlled by the mixing time scale (see the comments at page 15220 line 21). The source of discrepancies in the vertical profile of the mean concentration (figure 2) is not the stochastic term. This is verified and briefly discussed in the results (page 15230, line 15-23) where it is underlined that the level of agreement for the mean concentration (see fig. 2) between the RANS-SFM and the LES is the same obtained using a standard RANS model (previous results of Galmarini et al. 2008). This means that the stochastic and mixing terms do not noticeably alter the mean concentration field which is basically the same of what obtained using a standard RANS model. To improve the clarity of this point we will modify line 15 of page 1530 as follow:

“In general the results follow the one obtained by G2008 using second order RANS simulations; the mean ……".

As mentioned at page 15230, lines 24-30, and already discussed in Galmarini et al. (2008), the differences in the mean concentration between the LES and the RANS (or the RANS-SFM) derive from the different grid resolution, source surface coverage and from the details of the two meteorological models (LES and RANS) which generate slightly different wind fields.

2) The cells E and F are not influenced by the emissions in cell C. Please explain to the reader why. The situation appears symmetric, but the wind backs towards the surface I assume.

The reviewer is right; a brief comment will be added at page 15231 after line 1: "This is due to the rotation of the mean wind vector towards the left of the assumed higher elevation westerly wind while progressively moving closer to the surface"

3) The emission distribution in the LES simulation is chosen rather arbitrary (figure 1). Why not a random distribution or a distribution that would represent line or point sources, surrounded by non-emitting areas? This would make the simulations more representative for a real-world example. Moreover, the edge effects now strongly influence the transport of tracers to the cells A and B. This is partly caused by a shrinking source area in the LES

As explained in the paper the source configuration and all the experiments were selected for the sake of comparability with the previous work of Galmarini et al. (2008) who addressed in a different way the same problem. The simulated source represents in our opinion the most fundamental SGS source configuration and it is therefore a fundamental prototype of the problem. As also noted by the reviewer, this source configuration is a demanding test of the RANS and RANS-SFM model capability since, due to its concentrated nature, it tests the limitation of the model regarding the sub-grid scale spatial localization. We agree with the reviewer that other pseudo-realistic source configurations are possible and can be investigated through the use of LES simulations. However, in this paper we presented a completely new modeling approach and we decided to test it with what we considered to be the more fundamental SGS source configuration. Further tests are possible but they would enlarge an already "verbose" discussion and distract the reader from the main points of this paper that are, i) demonstrating the presence of a problem, ii) proposing a new modeling method to tackle the problem, iii) having the possibility of verifying the quality of the result by comparing them with those from a different approach and iv) conduct fundamental but preliminary test to demonstrate the capability of the approach and discuss its potential.

4) The discussion of the applicability could be extended. In the conclusions, it is mentioned that about 50 realizations are required for one tracer. If more chemical reactive tracers would be included with either correlated or un-

correlated emission structures, what would be the computational burden? What about emission variability in adjacent grid cells? Would that require 50x50 tracers?

The RANS-SFM is a stochastic method and therefore the inclusion of more chemical reactive tracers increases the computational burden linearly (e.g. Dopazo et al. 1997, Valiño 1998, Pope 2000, Fox 2003, Sabel'nikov and Soulard 2005). More in detail this is strictly true as long as we are interest in extracting from the simulations only single tracer moments or to marginal PDFs. However, it must be remarked that the "influence" of the joint PDF on marginal statistics is correctly accounted for, since the joint PDF transport equation is solved using a stochastic method. The increase in the computational burden is more pronounced if joint statistics need to be extracted from the simulations with an accuracy (statistical error) comparable to the one for single tracer moments, and become maximum in the unlikely case that the joint PDF of all the species simultaneously need to be known with comparable accuracy. These aspects are discussed in the reported references on PDF methods (e.g. Dopazo et al. 1997, Valiño 1998, Pope 2000, Fox 2003, Sabel'nikov and Soulard 2005). It is not expected that emission variability in adjacent grid cells will increase significantly the computational requirement. However, this is a newly proposed method and, as mentioned above, discussing and "testing" all these aspects in details is not the purpose of the present paper. Extensive test will be conducted in the future and the suggestions of the reviewer will be certainly considered in planning new extensive numerical and ideally, laboratory and field experiments. Similar, more concise, comments will be added in the conclusion.

5) Instead of focusing on the third order moments, it would be instructive to show plots with typical concentration fluctuations in both the LES (averaged) and the RANS-SFM.

The capability to predict high order statistic is a peculiar characteristic of the proposed approach which clearly shows that the level of information available with this method is completely different from what available using a standard second, or even higher order, RANS approach. Testing the ability of the method in predicting high order statistics is therefore fundamental and shows to the reader the potential of the proposed approach.

Some small textual points:

15216, line 6: and the results compared. Change to: and the results are compared.

Ok

15217, line 19: emissions heterogeneity. Change to: emission heterogeneity.

Ok

15219, line 13: scalars. Change to: scalar

Ok

15220, line 19: Change 'scalar' in 'scalars'.
Ok

15226, line 3: Namely the atmospheric flow..sentence unclear (Namely?)
Will be changed to "Specifically"

15228, line 3: reaction source terms. Why not sink terms?
It was meant in the broader sense, i.e. including negative source term. For clarity it will be modified in "source/sink".

15230, line 29: at this point, the reference to Galmarini et al. (2008) starts to bother me. Maybe introduce G2008 or remove some of the recurring references to this paper.
We will use G2008.

15233, line 6: negative value, change to negative values.
Ok

15234, line 24: change 'issue' in 'issues'
Ok

15236, line 21: change 'word' in 'world'
Ok

15236, line 26: scalars instead of scalar.
Ok

15237, line 3: 'reactions' instead of 'reaction'
Ok

15237, line 8: 'concentration values'
Ok

15237, line 9: What are gridded populations?
Population assigned to each grid element