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Interactive Comment

Interactive comment on "Concatenated non-stationary dispersive scenarios on complex terrain under summer conditions" *by* J. L. Palau et al.

J. L. Palau et al.

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We thank referee#1 for his/her review and thoughtful comments. We also appreciate his/her positive review of our paper, recommending its publication after addressing only minor changes and corrections in the original manuscript.

The referee made both general and specific comments, which we now address below.

1. General comment:

Referee#1 expresses no doubts in corroborating the scientific significance and interest of the issues and results addressed in the paper; furthermore, referee#1 stresses the consistency and reproducibility of the methodological approach.

Referee#1 says it would be very interesting to also show some results from modelling in the vertical. Unfortunately the measurements available (obtained with the COSPEC) correspond to vertically-integrated SO2 concentrations, and we haven't the possibility of validating or comparing the simulated vertical structure of the plume aloft. In any case, this issue is out of the scope of the manuscript because the main aim of the manuscript is to analyse how emissions from a tall chimney are (horizontally) distributed aloft in a multimodal way around the stack (with no clear mean plume advective direction during transitional periods, as documented between different, but concatenated, dispersive scenarios).

Nevertheless, as indicated in our answers to the specific comments below, we have now introduced and discussed some aspects of the vertical SO2 distribution of the simulated plume, connecting it to the different turbulent regimes activated in the MM5 PBL-parameterisation throughout the 3-day simulation (convective mixing and mechanical turbulence schemes).

2. Specific comments:

* Referee#1 suggests that we give our paper a different and more precise title, which includes some keywords. This was also suggested by referee#2. Thus, we have changed the title to: " Transitional dispersive scenarios driven by mesoscale flows on complex terrain under strong dry convective conditions ";

* Referee#1 suggests reordering the abstract and including an additional paragraph (line 8 on page 10844, " This paper analyses... ")

Done. The abstract of the revised version of the manuscript is as follows:

" By experimentation and modelling, this paper analyses the atmospheric dispersion of the SO2 emissions from a power plant on complex terrain under strong convective conditions, describing the main dispersion features as an ensemble of "stationary dispersive scenarios" and reformulating some "classical" dispersive concepts to deal with

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the systematically monitored summer dispersive scenarios in inland Spain. The results and discussions presented arise from a statistically representative study of the physical processes associated with the multimodal distribution of pollutants aloft and around a 343-meter-tall chimney under strong convective conditions in the Iberian Peninsula. This paper analyses the importance of the identification and physical implications of transitional periods for air quality applications. The indetermination of a transversal plume to the preferred transport direction during these transitional periods implies a small (or null) physical significance of the classical definition of horizontal standard deviation of the concentration distribution. "

* Referee#1 asks if we used "stack" as a synonym of "chimney". We did. In any case, for consistency in the manuscript, we will use "chimney" throughout the text, avoiding the use of "stack".

* Referee#1 asks about the vertical configuration of the nested grids.

The revised version of the manuscript includes this information when describing the configuration of the model in section 2. We include the following:

" Thirty-nine sigma levels were configured, with a 10 m spacing near the surface increasing gradually up to 1000 m near the model top at 15000 metres above ground level (m.a.g.l.), fifteen of them defined within the first 1500 m.a.g.l. "

* Referee#1 asks why we release particles randomly from a 0.1x0.1x0.01 km volume.

This is a very appropiate comment because, as a matter of fact, there is a misprint in the definition of the volume. The particles were released randomly in a volume of 10x10x100 meters.

10 x 10 meters corresponds (broadly) to the diameter of the chimney exit. We chose a vertical distribution of 100 meters to represent the distribution of the plume rise around the estimated effective high for the release (50 meters above and 50 meters below the estimated constant highs of 450 and 700 meters and the variable highs calculated

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following the Briggs plume-rise scheme).

* Referee#1 suggests combining figures 1 and 2 into a single (and bigger) figure.

Done. This modification has been made in the revised version of the manuscript.

* Referee#1 comments that section 3.1 should be simplified and reordered; maybe by using a synoptic weather chart in figure 3.

This comment is quite similar to one made by referee#2. We have followed the referees' suggestion and simplified section 3.1. As suggested, we have also added a new figure showing the synoptic weather charts for the 3-day period considered.

* Referee#1 suggests modifying the colors in figures 4 to 8 to make them more readable. Referee#2 makes the same suggestion.

Considering both referees' comments on figures 4-to-8: on the one hand, we have eliminated figures 4, 7 and 8, and we have illustrated the text with figures of the daily cycle only for day 2, referring the readers to the supplementary material to get an image of the qualitative description for the whole three-day period. On the other hand, we have tried to make figures 5 and 6 more readable in the revised manuscript.

* Referee#1 suggests that the delay and biases in ground concentrations (modelled and measured) deserve more analysis and discussion.

We consider referee#2's comment a very important and interesting issue, although it is slightly out of the scope of this paper which focuses on the study of horizontal integral advection under strong convective conditions in complex terrain.

Recently, the authors have published in the "Environmental Monitoring and Assessment Journal" another paper directly related to the referred issue and entitled "Seasonal differences in SO2 ground-level impacts from a power plant plume on complex terrain". The reference is: DOI: 0.1007/s10661-008-0221-x This paper is published online and can be easily download from the following webpage

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http://www.springerlink.com/content/rk748625120x8210/

We think it may be of interest to referee#1, as it (published in February 2008) is fully complementary to this new manuscript.

In any case, following referee#1's comment we have included this new reference in the revised version of the manuscript; and we have rewritten section 4 (Discussion) including the following text (and a new figure showing the vertical wind directional shear and its implications on the plume footprint on the ground during the plume-reorganisation phase):

"Thermal mesoscale tropospheric flows of this kind are simulated by MM5 as a physical consequence of the simulated exchanges of energy and water between the vegetation (or soil) and the atmosphere, which are directly dependent on the land-use defined on each grid-point and the initialisation of the model. The authors have found this to be the main reason for the significant delay of two-to-three hours with ground-level concentration records, when simulating the transitional periods of the dispersive conditions. This delay has been attributed to the configuration of the model and, more specifically, to the limitations in either the implemented parameterisation of the PBL in the meteorological model or the land-use database used in the FLEXPART model, or both. On the one hand, Blackadar's nonlocal closure scheme is based on the assumption that turbulent mixing is isotropic (i.e., symmetric) in the PBL; however, from observational evidence and large-eddy simulation modelling studies (Schumann 1989), it is well-known that mixing processes in a convective boundary layer are essentially asymmetric (i.e., turbulence is anisotropic). On the other hand, soil moisture data and surface cover-type classification systems are responsible for heterogeneous surface fluxes of sensible and latent heat. Furthermore, our simulations show significant spatial biases with ground-level concentration records. Despite the model resolution, under stable dispersion conditions (plume advected as a ribbon-type plume), limitations on the model performance have been evidenced due to the local nature of plume impacts far away from the chimney ($>$50 km). Under such dispersion conditions slight deviations between the real

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and the simulated plume advective direction have a strong impact when comparing the simulated and measured local ground-level concentrations. Previous results (Palau et al. 2006, 2008) showed how on this complex terrain area, under stable dispersion conditions (nighttime), the simulated mechanical turbulence leeward of the mountains reproduces highly concentrated SO2 fumigations on the ground more than 60 kilometres away from the power plant but with significant spatial biases associated with the local nature of the fumigations. Besides, under convective activity, an isotropic ground-level concentration field is simulated within a circular area with a radius of 20 km around the power plant; i.e., under summer conditions plume fumigations near the chimney are essentially equiprobable all around the chimney. A detailed analysis of these issues, disaggregating the accumulated plume footprints into the contributions of the different turbulent regimes activated in the MM5 PBL-parameterisation throughout the 3-day simulation (convective mixing and mechanical turbulence schemes), can be found in Palau et al. (2008). "

The corresponding references are:

Schumann, U.: Large-eddy simulation of turbulent diffusion with chemical reactions in the convective boundary layer, Atmospheric Environment, 23, 1-15, 1989.

Palau, J. L., Pérez-Landa, G., Melia, J., Segarra, D. and Millan, M. M.: A study of dispersion in complex terrain under winter conditions using high-resolution mesoscale and Lagrangian particle models, Atmospheric Chemistry and Physics, 6, 1105-1134, 2006.

Palau, J.L.; Meliá, J.; Segarra, D.; Pérez-Landa, G.; Santa-Cruz, F.; Millán, M.M.: Seasonal differences in SO2 ground-level impacts from a power plant plume on complex terrain, Environmental Monitoring and Assessment, DOI: 10.1007/s10661-008-0221-x, 2008.

* Referee#1 asks for references (or an explicit explanation) of the procedure followed to calculate horizontal dispersion for the plume aloft. We have introduced on p. 10845,

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line 20 the following text:

" Our method of obtaining typical horizontal deviations of the plume distribution aloft from the available experimental records has already been described in the literature (Millán et al., 1976; Millán, 1978); moreover, the details of the modified Pseudo-Lagrangian method used in this study can be found in Palau et al (2006). Nevertheless, we should point out that the experimental measurements of the SO2 distribution aloft, followed by the above-referenced procedure, allow us to obtain mean values of the transversal plume dispersion at different distances from the emission point and during a determinate temporal period. "

The corresponding references are:

Millán, M. M., Gallant, A.J. and Turner, H.E.: The application of correlation spectroscopy to the study of dispersion from tall stacks. Atmos. Environ., 10, 499-511, 1976.

Millán, M. M.: Remote sensing of SO2, a data processing methodology. Proceeding from 4th Joint Conference on Sensing of Environmental Pollutants. American Chemical Society, 1978.

Palau, J. L., Pérez-Landa, G., Melia, J., Segarra, D. and Millan, M. M.: A study of dispersion in complex terrain under winter conditions using high-resolution mesoscale and Lagrangian particle models, Atmospheric Chemistry and Physics, 6, 1105-1134, 2006.

* Referee#1 asks for the differences between the data shown in tables 1 and 2 and figure 9, suggesting that this figure may be redundant. Referee#1 also says that the heading of table 2 requires some editing.

Referee#2 makes a similar comment regarding table 2 and figure 9.

It is true that table 1 and figure 9 represent the same measurements, but table 1 stresses (in boldface numbers) that main differences occur during transitional peri-

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ods (periods of time with strong convective conditions). The purpose of figure 9 is to readily show the magnitude of the differences between the different emission schemes and experimental measurements. From our point of view, the quantitative discussion is clarified by figure 9 while table 1 quantifies exactly the discussion in the text.

Thus, we think that both the table and the figure are worth publishing, but if we had to choose, we would maintain table 1 and remove the figure.

Regarding table 2, this is relevant because it shows the statistical skills of the model.

Following referee#1's comment, we have re-written the heading of table 2 as follows:

" Table 2: Statistical skills for the horizontal dispersion values simulated with the three different emission schemes (450m, 700m and Briggs). The nomenclature for the different columns is: m: fitting slope; b: ordinate [in kilometres]; SE: Standard Error (for both the fitting slope and the ordinate [in kilometres]); p-value (for both the fitting slope and the ordinate); RMSE: Root Mean Squared Error [in kilometres]; RMSEu: Unsystematic Root Mean Squared Error; RMSEs: Systematic Root Mean Squared Error; RMSEa: Additive Root Mean Squared Error: RMSEp: Proportional Root Mean Squared Error; RMSEi: Interdependence Root Mean Squared Error; d: Index of Agreement. "

* Referee#1 points out that one reference is repeated.

Done. This misprint has been corrected in the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 10841, 2008.

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