

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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Response to referee#2:

The authors would like to thank referee#2 for her/his careful and constructive comments on our manuscript. We also thank the reviewer for his/her positive review of our paper as he/she has made only minor, specific comments without suggesting major changes in the original manuscript.

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

1. General comment:

The authors fully agree with the general comments by referee#2. As she/he remarks,

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this study is supported by an innovative summer campaign showing a new way to interpret measurements and modelled results by giving a description of the main dispersive features as an ensemble of concatenated stationary dispersive scenarios evolving during daily cycles. As referee#2 points out, we chose as a tracer of opportunity only the SO₂ pollutant, in order to avoid uncertainties associated with atmospheric chemical reactions during the dispersion modelling tasks.

2. Specific comments:

* Referee#2 suggests using a different title that would better fit the contents of the paper. This was also suggested by referee#1. Thus, following both referees' suggestions, we have given our paper a new title: " Transitional dispersive scenarios driven by mesoscale flows on complex terrain under strong dry convective conditions "

* Referee#2 commented that the qualitative description of the results is too detailed and should be shortened both in text and figures (figs. 4 to 8), making the residual figures more readable.

Following the referee's suggestion and we have shortened section 3.1. And we have eliminate figures 4, 7 and 8. In the revised manuscript, the text is illustrated with figures of the daily cycle only for day 2; for a qualitative description of the whole three-day period the reader is referred to the supplementary material. Moreover, we have tried to make figures 5 and 6 more readable in the revised manuscript.

* Referee#2 says that table 1 and figure 9 give redundant information.

It is true that both represent the same measurements, but table 1 stresses (in bold-faced numbers) that the greatest differences occur during transitional periods (periods of time with strong dry 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.

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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.

* Referee#2 states that the authors should better explain and clarify the results shown in figure 9.

Following referee#2's suggestions, the results shown in figure 9 (and table 1) have been explained and clarified in the revised manuscript.

* Referee#2 says Page 10847, 1st row: clarify reference to proper tables.

We have done this in the revised manuscript.

* Referee#2 comments that a cost-benefit analysis in terms of the meteorological database and pre-processors needed and the reliability of the simulated plume dispersion in the atmosphere might help to give proper credit to the study.

Two specific comments regarding this issue have been introduced in the revised manuscript.

At the end of section 2 (Methodology), we have included the following discussion:

" The total amount of particles released was high (2×10^6), as we need high precision in the calculations of the horizontal dispersion of the simulated plume. This fact and the high resolution of the meteorological simulation used a large amount of computer power (the equivalent of 6 days of CPU time on a 3GHz local lab-top computer). However, optimizing the modelling setup for AQ-forecasting purposes (e.g., 10% of the particles might be enough) would allow two-day forecasts to be made in 12 hours on a current laptop computer. "

At the end of section 5 (Conclusions), we have included the following text:

" The modelling approach proposed here is physically consistent with the observed processes and avoid the misfits of models based on classical dispersion parameters under transitional periods. The proposed system is more expensive than classical

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models in terms of computing power, but current advances in computer science allow it to be used at an affordable cost. "

* Referee#2 says that the reference to a PhD thesis (by one co-author) seems redundant.

We don't agree with this comment. We think it must be included because this referenced PhD Thesis is edited and published (ISBN: 84-688-4440-3), and gives details and relevant information regarding the case study presented in this manuscript (as, for example, the statistical significance of these transitional periods, detailed information regarding the experimental and modelling procedures, validation techniques, etc.)

* Referee#2 suggests increasing the color differentiation used in the figures.

We appreciate this comment and we have tried to make the figures more readable in the revised manuscript to be published in the ACP.

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

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