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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The importance of meteorological scales to forecast air pollution scenarios on a complex-terrain coastal site of the Iberian Peninsula" *by* J. L. Palau et al.

Anonymous Referee #2

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1. General comments

There are two ways one can approach the paper. It can be considered as a case study, performed with high proficiency and supported by a lot of experimental data, which demonstrate some specific mesoscale effects in a given region and their impact on the local to regional scale air pollution transport. The objectives of the work are much boarder, however, and the basic concepts and the conclusions made in the paper go further beyond a mere case study. In this sense the title is appropriate and clearly reflects the contents of the paper. From the other hand, the particular region for which

the numerical experiments were carried out is not of such importance for the general conclusion and the main message of the paper, so perhaps a shorter title like “The importance of meteorological scales to forecast air pollution scenarios in complex terrain” will better underline the authors’ ideas and the “phylosophycal” aspect of the paper.

The main questions the paper address - the interaction of different scale dynamics phenomena and their influence on the mesoscale flow patterns and thus on the pollution transport, are by all means within the scope of Atmospheric Chemistry and Physics (ACP). The problem of different scale interactions and how to account for them in the models of atmospheric dynamics and air pollution is a crucial one not only in local to regional pollution transport scales, but also perhaps in regional weather forecast, regional climate studies, etc. This determines the scientific significance of the problems addressed by the paper. I have no doubts in suggesting the paper to be accepted for publishing in ACP.

2. Specific comments

The idea, that smaller scale phenomena may have great influence on larger scale flow systems is certainly not a new one. Quite a nice and informative overview of related works is given by the authors in the introduction to the paper, providing an appropriate number and quality of references. The novelty in the present paper is that it gives a very clear and convincing demonstration of the importance of different scale interaction, using a several day case study in a complex region as an example.

There is no reason the paper to be summarized in a referee comment, so the brief referee’s answers to some standard questions will just be listed below:

- Are the scientific methods and assumptions valid and clearly outlined? - The study is based on synergetic use of numerical models and experimental (meteorological and air pollution) data from both routine observations and a special field campaign. The models (RAMS mesoscale meteorological model and HYPACT particle model) are internationally recognized models with proved simulation abilities. The model validation

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is carefully performed, by using several agreement criteria and shows the model relevance to the particular problem.

- Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists? - The models applied and the agreement criteria are well known; the model configuration and the source of background meteorological information are clearly described, so the numerical experiments can be reproduced.

- Are the results sufficient to support the interpretations and conclusions? In my opinion the authors' interpretation of the results is correct and the main conclusions in the paper logically follow from the results and their interpretation.

- Does the abstract provide a concise and complete summary? - In my opinion it does.

- Is the overall presentation well structured and clear? - In my opinion it is. The amount and quality of supplementary material (tables and figures) is appropriate and very well illustrates the text. In my opinion there is no need to modify, clarify, reduce, combine, or eliminate any parts of the paper (text, formulae, figures, tables).

The main and most interesting results in the paper appear from the comparison of simulations S22 and S12. They are both performed in the same domain G2 with the same resolution of 13.5 km. The difference is that in S12 the feedback from finer resolution simulations in the sub-domains G3 and G4 is accounted for by two-way nesting. The comparison shows that both the dynamic and air pollution fields obtained by S22 and S12 are qualitatively different and that simulation S12 better agrees with the experimental data (the S22 simulation for example fails to simulate drainage flows). This is a clear result, which arises no doubts or objections and convincingly demonstrates the impact of smaller scale phenomena on larger scale processes.

Some questions arise however, which I will try to summarize in a compact way and will be very happy if the authors answer them, or provide some comments.

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First of all the comparisons between simulations S22 and S12 are shown for points within the innermost domain G4. It will be interesting to know if the feedback provided by the two-way nesting causes as significant difference between S22 and S12 simulation results in G2 points, which are out of domains G3 and G4, i.e. are the effects of smaller scale phenomena manifested outside the domains where high resolution nesting is performed and to what extend? It seems to me that this is reasonable and important question. If the answer is affirmative, this means that simulations in larger domains can be improved by selecting a limited number of sub-domains, known for their complex multi-scale dynamics and performing high resolution simulations within these domains, which trough the feedback provided by the two-way nesting, will influence the flow patterns in the larger domain. If such a procedure is possible and fruitful it can save a lot of troubles with parameterization of sub-scale processes in the numerical models.

The present study concerns a selected coastal area and synoptic background chosen for its high recurrence and because on such a background the mesoscale phenomena in the region are typical and well displayed. It is almost trivial to expect, that similar effects (complex interaction of different scale phenomena) are not unique for the selected region, not even for costal areas and may take place also in other regions with complex terrain.

It is not so apparent what the situation could be under other synoptic conditions, which do not favor such an explicit manifestation of typical mesoscale flow systems, like for example slope winds or sea breezes. Could in this case small scale phenomena, masked by well organized large scale flows, also have significant influence on the larger scale processes and will a cascade of two-way nested domains provide relevant feedback that will account for this influence in the model simulations?

Finally, not able to resist the temptation, I would like to ask a “phylosophycal” and a little bit provocative question: The expression “(in)adequate scale” is used several times in the paper, without being defined. By intuition, experience and data analysis one can

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think out a working explanation for given particular case, but still there are no general and instructive criteria for what adequate numerical treatment of meteorological phenomena really is. Even the optimal choice of grid resolution is not yet a solved problem (provide one shares the point of view that the higher resolution technically available is not necessarily the proper one).

3. Technical corrections

I didn't have any difficulties in understanding the English language of the paper, but I don't think my own poor knowledge of English gives me the right to make any comments and suggestions about the language.

As far as symbols, abbreviations, and units are concerned, in my opinion they are correctly defined and used. I didn't noticed any technical mistakes, but one - in the text describing Table 2 it is obvious that "Simulation S22, S14 was performed", should be replaced by "Simulation S22 was performed".

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 4701, 2005.