

## ***Interactive comment on “Mesoscale circulations over complex terrain in the Valencia coastal region, Spain, Part 1: simulation of diurnal circulation regimes” by G. Pérez-Landa et al.***

**G. Pérez-Landa et al.**

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We would like to thank the three referees and Dr. Kovalev for their reviews and helpful comments. The reviewers agree that the paper contributes to gain knowledge about the behavior of mesoscale circulations in complex terrain and about the ability of a mesoscale model to reproduce the main processes. We have modified our manuscript taking into account their suggestions. In the following we address their questions.

### **Anonymous Referee 1:**

*a) the introduction should be considerably shortened. Namely reference to literature on CO<sub>2</sub> should be omitted as they belong eventually to the second part of the work*

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(mainly lines 23-end page 2811, and 1-23 page 2812).

We have shortened the introduction, omitting the general background about CO<sub>2</sub>.

*b) Sections 2.2 and 2.3, in my opinion should be omitted. In this paper you investigate a 2 day event and eventually during these days you are performing the study on the CO<sub>2</sub> in the second part the work. The conditions during the whole experiment period (summer 2001) are not relevant. The omission of these sections (and of their figures) would contribute to the decrease of the size of the paper.*

Sections 2.2 and 2.3 have been omitted.

*c). Decrease the size of section 7 (discussion) and merge it with the conclusions.*

The Discussion and Conclusion sections have been reformulated into a “Concluding remarks” section, thus decreasing the length of the paper.

*d) Following the major point in general, the appendix (with the relevant figures) should be omitted.*

The Appendix has been dropped.

*Minor points:*

*1. page 2820, lines 15-18: It is not clear to me what you are using as SST for your simulation. How do you combine weekly climatological with satellite data?. SST is very important factor for the definition of the sea-breeze. Please specify.*

Weekly satellite data are used only where no NOAA-processed data are available. In our case study, this occurs only in specific regions of the outer domain (far from the area of interest). The text of the revised manuscript has been clarified in this point.

2. *page 2821, lines 1-6: there is a striking difference of the simulated RH compared to the observations at the VI station during night time. Can you comment on this?*

The use of the variable  $q$  instead of relative humidity, as suggested by Referee 3, has changed the bias patterns of the moisture. As the revised manuscript describes, there are some deviations in specific humidity probably related to the limitations in our soil moisture initialization. These limitations are more evident at the VI site, although not only during nighttime conditions, as Figure 3 of the revised manuscript now shows.

3. *Fig. 7b has a different vertical and horizontal scale than 7a and 7c. Please provide the same scale.*

The scale of Fig. 7b has been corrected.

## **Anonymous Referee 2:**

### *Section 1:*

*(1) The introduction is too long. The CO<sub>2</sub> discussion is not relevant for the paper (maybe for part 2).*

This has been corrected.

*(2) last para (page 2813), 'modeling simulations': only 'simulations'*

This has been changed in the revised manuscript.

### *Section 2:*

*(3) drop section 2.2, this is not relevant for the case study*

*(4) drop or shorten section 2.3*

Both subsections have been dropped.

*Section 3:*

*(5) page 2817, para 2: aircraft legs (not routes), there is only one flight leg, but at different heights*

This term has been eliminated.

*(6) page 2817, para 3 (and at other places): you cannot simulate meteorology (or biology), since this is a scientific discipline (better: meteorological fields).*

We have changed this expression in the revised manuscript.

*Section 4:*

*(7) page 2817, para 3: Soil moisture and soil type is very important for the simulations. It is not clear, where the soil type comes from (only clay loam?). The authors should be aware that there are other methods for the generation of soil moisture fields in current NWP models. The spin-up method used here is not clear. Which input data are used? If you don't use precipitation and radiation, what is the use of your exercise? You could just prescribe a soil moisture profile.*

For simplicity we used only one soil texture: “clay loam”. We tried to avoid the unrealistic patterns of soil conditions found in previous tests, which were related to the horizontal resolution of the soil texture information available (FAO database). As we referenced in the manuscript, we know that there are other methods to initialize soil moisture, most of them using conventional meteorological observations to force the land surface scheme (e.g. Smith et al. 1994). However, the complexity of this task considering the orographic and climatic features of our region, was far outside the

scope of this study. Instead, we forced the land surface data with the meteorological fields solved by RAMS, smoothly nudged to the analysis of a global model. As we showed in the revised manuscript this method improved the use of soil conditions from the global model, and we think that can provide more information than a homogeneous soil profile. At any rate, assuming the limitations of our scheme, the skills of the model when compared with observations (see manuscript) were good enough to capture the main processes in the region.

#### *Section 5:*

*(8) page 2821, para 1: use the same symbols in text and figures (HR in Fig.3, RH in text)*

Now the study is focused on specific humidity ( $q$ ) instead of RH.

*(9) page 2824, para 1: I don't believe that what you see above 1200m is the return flow. It is too strong. Check synoptic fields.*

We checked the synoptic fields from the GFS global model at this height, and the wind comes from the South or South-East, without any Westerly component. So, it seems to be the effect of the return flow.

#### *Section 6:*

*(10) This section is much too long, and it is partly repeated in section 7. Shorten it!*

We have shortened this section.

#### *Section 8:*

*(11) page 2833, para 3: your soil moisture initialization is not 'proper'!*

We have eliminated this expression.

*Appendix A can be dropped. It is well known that sea-breezes are a climatological feature in that area.*

The Appendix has been dropped.

*Tables and figures*

- *Fig.2: drop*
- *Fig.3: gray curves can are hardly be seen*
- *Fig.A1: mark lows and highs*
- *Fig.A2: drop*

We have followed all the suggestions about the figures.

### **Anonymous Referee 3:**

*Comments*

*Page 2818 first five lines another example of repetition Line 10, 15.*

We have tried to remove the repetition in the revised manuscript.

*How do the fluxes of the tower compare to the airplane? What is in fact the relevance of mentioning that fluxes are measured, while there is no no effort made to show the fluxes or alternatively use them for model validation. This would improve the paper dramatically and make the model results more robust!*

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The first question is, in fact, the main subject of the paper written by Gioli et al. (2004). We tried to clarify this in the revised manuscript. As the paper shows and as we mention in the manuscript, the rice fields under the aircraft horizontal legs were flooded, which generated a very low Bowen ratio. This feature was not included in the model and, consequently, the surface fluxes over the rice fields are not comparable.

In the new version of the manuscript we tried to follow the Referee suggestion including the comparison between Net radiation, surface latent and sensible fluxes of the model in SA eddy-covariance tower. We added a new figure (Fig. 5), although these results cannot be considered representative of the whole basin.

*Page 2820 The soil moisture initialization is complicated, but why not show how the fields compare to analysis or a simple water balance model that uses observed P. The fact that someone else has also done this is not necessarily a correct justification; one does not want to repeat errors. See also page 2826 line 23-24.*

New Figure 2 in the revised manuscript shows a comparison, at different levels, between the soil moisture analysis from the AVN global model and those obtained from the 2-months spin-up. Additionally, we included more information in the text regarding the limitations of our approach.

*Page 2820 L 25. I do not understand why RH is shown and not q. Since RH is also strongly dependent on T, the RH picture is far more difficult to interpret than q. I would also suggest to make conserved variable plots of  $\log T$  versus  $q$  as in the work of Allan Betts that can really show the diurnal trends well (f.i. on page 2826).*

The referee's suggestion really improved our analysis of moisture, which showed a different pattern when  $q$  was used instead of RH. The dependence of RH on T was giving a different perspective on moisture behavior in our study. We also added the theta- $q$  plots suggested, which added some information about the heating-drying processes. These suggestions really improved the study.

Page 2830 It would be good to see how the stable boundary layer development proceeds during the night in this environment.

We have added some discussion about stable conditions during the nighttime (see section 8). However, the objective of the experimental campaign was centered on the daytime and the lack of experimental data aloft during nighttime conditions prevents a further analysis of the stable regime in this environment.

### I. Kovalev:

*General opinion about the paper is very good: it gives comprehensive comparisons of wind measurements and simulations in sea breeze and complex topography conditions. However, at first I agree with the criticism of all reviewers concerning the soil moisture initialisation: it's presentation is not very clear. In particular, it is not clear, why precipitation measurements were not used during "spin-up" calculations (as far as I understood that) and with respect to what FDDA was used? In the case, when real advantage was gained with the use of such initialisation scheme, it is preferable to describe it in more detail, because that initialisation approach seems to be novel.*

As discussed above, we have added more information on the moisture initialization in the revised text. The FDDA consisted in a smooth analysis nudging of RAMS towards the AVN Global meteorological fields.

*Secondly, in my opinion comparisons of predictions with vertical temperature profiles are also highly desirable, since they can give impression about the adequacy of the simulated PBL depth and turbulence, which can significantly influence CO2 distributions.*

We have added to our study a comparison between the RAMS and the measured vertical potential temperature profiles (Fig. 8).

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