



Interactive
Comment

Interactive comment on “Sensitivity of the WRF model to the lower boundary in an extreme precipitation event – Madeira Island case study” by J. C. Teixeira et al.

Anonymous Referee #2

Received and published: 4 January 2014

Review of “Sensitivity of the WRF model to the Lower Boundary In An Extrme Precipitation Event - Madeira Island Case Study” by Texeira et al., submitted to NHESS (nhess-2013-307).

The authors report on a set of experiments run with WRF-ARW to test the sensitivity of their numerical set-up to different topographic and land-use databases. After presenting individual difference fields of 10m horizontal wind and accumulated precipitation, they perform a quantitative verification of each experiment against observations for a number of operational surface stations operated by official Portuguese institutions.

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In my opinion, the interest of these findings for the broad NHESS audience is limited for various reasons. On the first place, the authors do not provide any insight on the interpretation of their results so that the reader is left alone at trying to link the reported experiments with his own interests. On the other hand, and tightly connected with this, it is not clear to me what is the motivation of the authors to publish these findings. The case is of course of maximum interest, but reporting on the results of 4 numerical experiments and not providing any physical interpretation seems weak for a journal article and more towards a technical report for the consortium dealing with the numerical prediction set-up the authors use.

The manuscript would gain much interest if some physical interpretation of the results was provided. For instance, what is the impact of the different topographies and land-use databases on the PBL structure and characteristics? Can you identify significant changes in the formation of convective updrafts? As a general rule of thumb, the better the databases, the better the numerical simulations, but often you can get unexpected results and it might be worth reporting to the community. For me, this is not the case of this manuscript in its current form.

MINOR COMMENTS

This is a (non-exhaustive) list of minor aspects to improve:

- a) Throughout the text, you mix together word and number expression “two meter”, “ten meter” and 2-m and 10 m. Please unify.
- b) You say “The simulated period was 24 h, starting on 20 February 2010.” What time?
- c) You say “The model skill is high when the following criteria are verified” and it is a clumsy way of introducing subjectivity (“high”) after the good job done using objective indices. You may want to provide the values of your indices for a perfect forecast, instead.
- d) I’m not familiar with Luna et al. (2011) but it is incorrect to turn off the convective

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parameterization scheme in a domain with 25 km grid size. The need for it in the 5 km grid is more debatable, but not for the first one. You should repeat the simulations, although you might find inconsistencies on the boundaries between domains.

e) You state that “this enhancement is small”. Could you even show it is significant?

f) Page 5622, line 25. “. . .of a high. . .”

g) Page 5623, line 4. “ Therefore, the use of . . .”

h) Fig. 4 panel a doesn't have coastline plotted. I believe the cross section plotted in Fig. 3 is only used in panel b of Fig. 4. Could you just mention in the caption of Fig. 4 the coordinates of the cross section?

i) Fig. 5 doesn't have a unit vector to scale the horizontal wind vectors. The caption has multiple flaws: “ten meter” instead of “10-m”; the vector field doesn't seem to indicate “direction” only but the full horizontal wind vector, if I'm not misguided.

j) Fig. 8 and 10 “fields”

k) Fig. 9. Why do you use a continuous scale for a discrete classification of land uses?

l) Please revise the entire manuscript and clarify you are dealing with the horizontal wind and not the full 3D wind field.

m) Figs 11 and 12. One of the advantages of the Taylor diagram is that allows to plot together different variables. Why do you plot two (almost empty) Taylor diagrams instead of a single one. The captions of these figures state that “green curved lines represent the STDE” whereas in a regular Taylor diagram (as also depicted in Fig. 11) the circles around REF (OBS in your case) show the RMS index. Please correct or clarify.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 5603, 2013.

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