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Geoscientific Model Development Discussions

Interactive comment on "Sensitivity of the WRF model to PBL parametrizations and nesting techniques: evaluation of surface wind over complex terrain" *by* J. J. Gómez-Navarro et al.

Anonymous Referee #1

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The present work assesses the ability of a mesoscale model (WRF) on reproducing the wind over Switzerland during 24 major wind storms. The paper is focused on two different questions. The first is identifying the optimum configuration of the model, by testing four different PBL schemes (YSU, YSU*, MYJ and ACM2) and three different running modes (free, analysis nudging and spectral nudging). The second part is focused on the important question of the added value, this is, the improvement of the results when using a higher horizontal resolution. Both are relevant questions within the scope of GMD, and especially the second one is nowadays the subject of a lot of interest and debate in the regional modelling community. For the evaluation , the papers uses a comprehensive observational dataset containing more than a hundred

station records over a very interesting domain, featuring an extremely complex and mountainous terrain (Switzerland). Thus, in my opinion, it represents a substantial advance in modelling science. The overall methodology and statistics used are robust enough to support the results: using boxplots to represent the sample uncertainty is a good decision. Furthermore, using an open source model as WRF does guarantee the reproducibility of the results. The writing is also very good, with precise explanations and almost no typos. I think that this is a pretty good manuscript and I only have minor comments. Consequently, I support publication after minor review.

Minor comments:

Figures: The line colours in the figures containing lines (figures 4, 6, 7, 8 and 10) are for me very difficult to distinguish. Some of them are also difficult to print or to see in a projector. I recommend using a different colour scheme, for example the Paired scheme in the qualitative schemes in colorbrewer2.org is a good choice.

Page 5438. Lines 11-12: I would say "the lack of representation of the unresolved topography", rather than the unresolved topography itself.

Page 5440. Line 24: Some of the regions studied in the cited papers could be categorized as "complex topography", it is even mentioned in the title of one of them (Jimenez et al. 2010). But of course nothing like Switzerland. Maybe replace by "not focus on areas with an extremely complex topography as Switzerland" or equivalent.

Page 5442. Lines 18-19: I think that it would be more correct to use a power law instead of linear interpolation. Anyway, the difference is likely small for small heigh differences, as most of them likely are.

Page 5445. Lines 5-6: As I see it, more than a different PBL scheme, it is the YSU scheme itself, with a parameterization of the unresolved terrain implemented in its surface layer scheme. Also, it is not mentioned in the paper so far but, in the scheme that Jimenez and Dudhia (2012) implemented, there is also a correction that effectively

removes the roughness when the laplacian of the (resolved) terrain is below -30m. This feature is intended to remove a negative bias found by Jimenez and Dudhia (2012) in the wind speed over summits, but it can be a source of bias in some resolutions and terrains, as shown by García-Díez et al. (2015). This may be have a relevant influence on the results of the present paper.

Page 5446. Lines 13-14. Another advantage of the re-forecast running mode is the low computational cost. As WRF scalability falls fast for large number of processors, splitting the simulations in time can save great amounts of run-time if a large number of cores is available. Even the extra spin-up required does not compensate this effect.

Page 5449. Lines 5-8: Looking at all the results, I won't say that YSU's non-local approach is clearly the best. YSU* is clearly superior, but it all seems to depend on the proper representation of the surface roughness and the orographic drag. Maybe ACM2 and MYJ approaches could reach YSU* skill with a similar parameterization of the unresolved topography effects.

Page 5450. Lines 5-7. This is interesting. There is also a similar effect of in García-Díez et al (2015), a lower correlation when using Jimenez and Dudhia (2012) correction, that is corrected when removing the laplacian-dependent part of the correction. Though it is very small. Maybe something is going on in the momentum budget?

Page 5450. Line 28: Mentioning the label of Lothar in the paper here (S13) makes easier find it in the figure.

Page 5451. Line 7: If I get it right, this means that the WRF winds are too ageostrophic, which could be related to too much roughness. But looking at the wind speeds it seems the opposite, there is a positive bias. I wonder what is going on in the model.

Page 5454. Lines 9-10: This is not surprising given the cases under study. The strength of the flow crossing the domain during wind storms gives small room for internal variability to develop. I think that if the simulations covered a longer period,

especially summers, with a weaker large-scale circulation, the difference between free and nudged simulations would be more important.

Page 5454. Line 28: RMSE is in Fig. 7B, no 7C.

Section 4.3: The role of horizontal resolution. I find the analysis to be good. However, I am going to suggest the citation of a couple more references.

Mass et al. (2002) is relevant for explaining the lower correlation found in the 2 km simulations respect to the 6 km. They explain how the RMSE can be lower in higher resolution simulations, despite increased realism. In this case, the RMSE is lower in the 2 km run because of the great reduction of the bias, but the lower correlation found is directly related to the smaller intrinsic predictability of the finest scales, in the mesoscale-gamma and microscale (Lorentz, 1969). García-Díez et al (2015) is very relevant here. In contrast with the present work, these authors do not find added value (or very small) in 9 km resolution simulations over Spain, when comparing with the driving model (GFS). The very different topography, with the extreme mountainous environment of Switzerland, can be an explanation of the different results. I think that this should be discussed in the text.

Page 5458. Line 12. "p" missing in topography.

Page 5459. Line 12-14: As noted before, likely the strong large-scale circulation related to the wind storms is making the effect of nudging smaller than it would be for a generic climate run.

Page 5459. Lines 22-23: Commas after "scheme", and "expected"?

García-Díez, M., J. Fernández, D. San-Martín, S. Herrera, and J.m. Gutiérrez. 2015. "Assessing and Improving the Local Added Value of WRF for Wind Downscaling." Journal of Applied Meteorology and Climatology, January. doi:10.1175/JAMC-D-14-0150.1.

Lorenz, Edward N. 1969. "The Predictability of a Flow Which Possesses Many Scales of Motion." Tellus 21 (3): 289–307. doi:10.1111/j.2153-3490.1969.tb00444.x.

Mass, Clifford F, David Ovens, Ken Westrick, and Brian A Colle. 2002. “Does Increasing Horizontal Resolution Produce More Skillful Forecasts.” Bull. Amer. Meteor. Soc 83 (3): 407–30. doi:10.1175/1520-0477(2002)083<0407:DIHRPM>2.3.CO;2.

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