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Geoscientific Model Development Discussions

Interactive comment on "A spatial evaluation of high-resolution wind fields from empirical and dynamical modeling in hilly and mountainous terrain" *by* Christoph Schlager et al.

Anonymous Referee #1

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Abstract

This study comparatively evaluates three different wind products: a dataset of very high resolution (100 m) observations, an analysis product that blends modelled and observed fields (resolution about 1 km), and a pure regional climate model simulation (resolution 3 km). The comparison is carried out in two regions, labelled FBR and JDT, being the latter characterised by a more complex orography. The assessment is mostly based on a rather new metric, the Wind Fractional Skill Score (WFSS) that avoids the "double penalty problem". They separate the analysis into "calm" and "windy" events, and conclude that the 3 km spatial resolution of the regional climate model is not suf-

ficient to reproduce some of the characteristics of the wind field as recorded by the observations, especially in calm events. As expected, the agreement is better in the region characterised by less complex orography. Both, the analysis product as well as the climate model, tend to overestimate wind speed for strong wind events.

General comment

I think the authors make a good work at taking the rare opportunity provided by an unique grid of very highly resolved observations (both in time and space) to carry out a valuable comparison between products obtained through simulations and observations in two areas with markedly different orography. Further, it employs a new skill metric that cleverly combines the performance respect to wind speed and direction, and allows the evaluation of model performance at various degrees of neighbourhood. The text is, in my opinion, well written and easy to read, although it acknowledge that I am not native speaker. I have not found mayor flaws in the methodology and the way conclusions are drawn from the results. Still, I have found minor caveats or issues that perhaps deserve corrections or improved explanations before being considered for publication.

Major comments

As I said above, I do not find issues that deserve the category "major". Still, there are few issues that are more or less general and that is hard to allocate in a given line, as I do below with other minor comments.

1. The term "dynamical modelling" is repeated through the manuscript, and even in the title. I think this expression it is not very common in the Regional Climate

Modelling literature. This term seems to combine two more common expressions: "regional climate modelling" and "dynamical downscaling". Both are used in the literature more or less interchangeably, but I think "dynamical modelling" is not generally used. The reason for this is that, technically, a Global Circulation Model is also dynamical modelling, but I'm sure the authors do not mean this type of model. Therefore, I would advise to stick to one of the two aforementioned alternatives.

2. The authors refer to two former publications (Schlanger et al. 2017, 2018) where the WPG seems to be further described. I acknowledge that I didn't read these publications, but it is not clear to me what this article improves or how it complements the formers. I think putting emphasis somewhere in the introduction on what new issues/questions this new article tries to address, compared to the formers, would help to frame this work and to better justify why it is necessary.

3. The INCA dataset assimilates observations. Then this dataset is compared/validated with respect to the WPG, which are also observations. Are they the same? Are WPG observations assimilated to produce INCA? I assume not, as otherwise there would be an important circularity issue.

4. I'm not sure what is meant by a "wind event". I understand that the criteria in Table 2 is applied on an hourly basis, right? Are then the events hourly-based, i.e. a given hour might be included as a calm event, while the next one might be included as strong? Or do the authors select for instance the whole day when at least a single hour within the day meet the criteria? Another way of posing this question is, are there as many events as hours within each period?

5. Another detail I could not understand is how the WFSS is calculated for different spatial scales. Is the data interpolated onto successive grids with coarser resolution?

Minor comments (in order of appearance)

1. The abstract is in my opinion longer than necessary. For instance, between lines 5 and 10 a great amount of details are given about the datasets. This level of detail is overwhelming at this early point of the paper, and distracts the reader from the main conclusions of the manuscript.

2. Pag 2, Line 9: course-resolution → coarse-resolution.

3. Pag 2, Line 15: "data fusion". I think a more precise term is "data assimilation" or "assimilation of observations".

4. Pag 2, Line 19: "dynamical regional climate models" → "regional climate models".

5. Pag 3, Lines 3-8 These two paragraphs read as a summary of the methodology. I do not think this is necessary in the introduction.

6. Pag 3, Line 10: I was not aware of the concept "two penalty problem". Therefore I was puzzled to read this without either a reference or a couple of lines that briefly summarise what is the deal with this. It is explained later, so I would advise to bring those explanations already here.

7. Pag 4, Line 6: "eleven" → 11 (for consistency reasons with the way this is reported for FBR)

8. Pag 4: Lines 20-26: Is it really necessary this amount of detail about how the data about temperature and humidity is produced for this system, given that these fields are not used in the manuscript?

9. Pag 5, Lines 15-16: "Therefore the output shows errors in regions with low station density" The model resolution does not imply that there are larger errors in areas with low station density. Why would it be the case? The validation is more difficult, but it could be that the model does a good job. We just don't know.

10. Pag 5, Line 22: The number of vertical levels in the RCM (not only the driving dataset) is an important parameter worth to mention.

11. Pag 7, Line 23: the units (m s^{-1}) should not be italic. This applies to several locations through the manuscript. Please review them.

12. Pag 8, Line 15 says that wind speeds are systematically underestimated. This is curious, as normally models tend to overestimate wind speed. Indeed, in the conclusions (Page 13, Line 19) this is noted when it is stated that wind speed are overestimated in both types of events. Isn't this contradictory? Please clarify the details.

13. Page 10, Line 21: "fundamentally able". Do the authors mean "unable"?

14. A bottleneck of WFSS is that it does not allow to disentangle if low skill is driven by problems with wind speed or direction. However in Pag 10, from lines 29, this is somehow solved, and low skill is attributed to errors to these two variables separately. But it is not obvious how these conclusions can be drawn from the shown figures. Is this based on an analysis that is not shown in the manuscript?

15. Page 12, Line 21: where → were.

16. The conclusions are overly long. They review every single detail of the results and after reading them is not obvious what are the take-home messages. I advise to summarise the conclusions to leave the most important and general conclusions, those that can be exported to other studies/regions.

17. This may seem as a tiny detail, but the fact that the panels in Fig. 1 do not follow the expected order (a, then b, finally c) puzzled me for a couple of minutes until I realised that FBR (labelled b, and firstly described in the text) is actually the last panel of the figure. Perhaps a trivial re-ordering of the panels following a more intuitive order might facilitate the reading.

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