The digital terrain model in the computational modelling of the flow over the Perdigão site: the appropriate grid size

authored by
José M. L. M. Palma, Carlos A. M. Silva, Vitor M. C. Gomes, Alexandre Silva Lopes, Teresa Simões, Paula Costa, and Vasco T. P. Batista
Submitted for publication in the *Wind Energy Science*
`https://doi.org/10.5194/wes-2019-96`

Reply to comments by Reviewer 2 (Anonymous)

The paper addresses the important problem of the numerical model setup for simulations of the wind over two parallel ridges at the Perdigão site, the area of interest during the intensive observational campaign of the New European Wind Atlas project.

The introduction is too long, given that the problem is quite a straightforward one: the numerical model resolution is sufficient when further refinement no longer affects the results. This is often ignored by necessity due to computational cost, and compromises are generally accepted if the results are still applicable after validation. So the goal of this interesting and relevant paper could be elegantly achieved by presenting a clear and comprehensive overview of the resolution requirements, so that future simulations of this highly studied area can easily be set up and evaluated with respect to the terrain and grid resolution.

> REPLY:
> We do not understand the comment. The introduction is two pages long. This is not a grid refinement study as usually in a computational fluid dynamics study. We are concerned mainly with the resolution of the digital terrain model. These are matters that go beyond computational fluid dynamics and have been dealt with in other disciplines, namely geomorphology. We are of the opinion that mainly in complex terrain, the accuracy of terrain model should precede the CFD three-dimensional grid and this is the message that we try to convey in section 1. Furthermore, the final message is that in Perdigão (and any complex site) the publicly available databases (e.g. SRTM, ASTER) are not accurate enough.

I would recommend that the paper is published after revision, addressing the specific comments below.

Specific comments:

L23: It is unclear what is meant with the resolution of the measuring equipment. Perhaps density would be a better word.

> REPLY:
> The proximity between measuring stations is very low. It reads now *on par with the resolution provided by such a large number of measuring equipment within a small region.*

L24: Please provide the geographic coordinates in WGS84 at this stage, and the equivalent in UTM. It is acceptable to use UTM later in the text.

REPLY:
Yes.

L33: The SRTM terrain resolution is 1" (24 m) but in the conclusions it is considered inadequate, in spite that the 40 m resolution is recommended as a minimum. Please comment.

REPLY:
For meshes with identical horizontal resolution (Figures 6 and 7), both SRTM and Mil yield higher RMSE of terrain elevation than meshes based on ALS. Terrain elevation (z_{Max}, Table 3) and slope (S_{Max}, Table 4) are higher for meshes based on ALS. It is not just a question of horizontal resolution, but the RMSE of terrain elevation of both SRTM and Mil. The abstract says it all.

L58: The threshold resolution is not a concept, but a requirement in numerical modelling. It is often ignored by necessity due to computation cost.

REPLY:
No comment.

L80: 22 square km would be easier to understand than 22 million square meters.

REPLY:
No comment.

L79,84: What is Blom TopEye? TPDS? Please supply a reference or description.

REPLY:
Blom[1] is the largest company in Europe, specialized on topographic laser ranging and scanning (Mallet and Bretar, 2009). TPDS and TASQ are acronyms of software by Blom. The information in the first four paragraphs of section 2.1 is technical information related to the lidar scanning.

L81: This is an impressive number of points in the cloud, but does it have any relevance? Please remove.

REPLY:
This information is generally provided in all laser scanning surveys and is essential for determining the horizontal resolution.

[1] `http://www.skgeodesy.sk/files/slovensky/ugkk/medzinarodna-spolupraca/bilateralna-spolupraca/norSirotek-2-Bratislava.pdf`

L83-89: This paragraph is very hard to comprehend. Please reformulate.

REPLY:
The text was rewritten.

L94, Figure 1: please display the two images in the same projection

REPLY:
Our apologies, we were unable to display the images in the same projection, due to different software used to draw either image.

L96: Table ?? ?

REPLY:
Reference to a missing table was removed.

L135: It is important to remember that the concept of two-dimensionality quickly breaks in the case of atmospheric flows, i.e. as soon as not all the forces acting on an air parcel are aligned with the 2D plane.

REPLY:
Yes, but that does not invalidate our statement, which is from the strictly geometrical point of view, given the high length to width ratio.

L146: It is inappropriate to use anything more precise than integer meters when describing terrain altitude with regard to atmospheric modelling. The same for geographic coordinates, for example in Table 1, especially since the highest data resolution is 2 m.

REPLY:
The 2 m resolution is the horizontal resolution, because this was the finest mesh generated from the lidar data. However, it is acknowledged that the vertical and planimetric accuracy of the airborne laser scanning are lower than 0.1 m and 0.4 cm (Mallet and Bretar, 2009). Geographic coordinates are now in metre (Table 1) and terrain elevation in decimetre (Tables 1 and 2).

L228-233, Section 5: It is unlikely that the atmospheric conditions at 22 UTC are neutral and stationary, as the simulation setup assumes. It is also unclear whether the authors are aware that the conditions might not be neutral and stationary, yet they proceed with the analysis because the referred publications have established the opposite? Please clarify by including the measurements of relevant parameters.

REPLY:
Yes, we are aware. That was the reason why we selected the 30 minutes averaged between 22:09–22:39 UTM on 4 May 2017. Because from the analysis of the 45 days of the IOP (Intensive operation period), this was a period during which stationarity conditions could be assumed. The reference (Carvalho, 2019), supporting our statement, is publicly available and the data was plotted in a way that shows the deviation from stationarity. The text was rewritten and information on stratification was also provided.

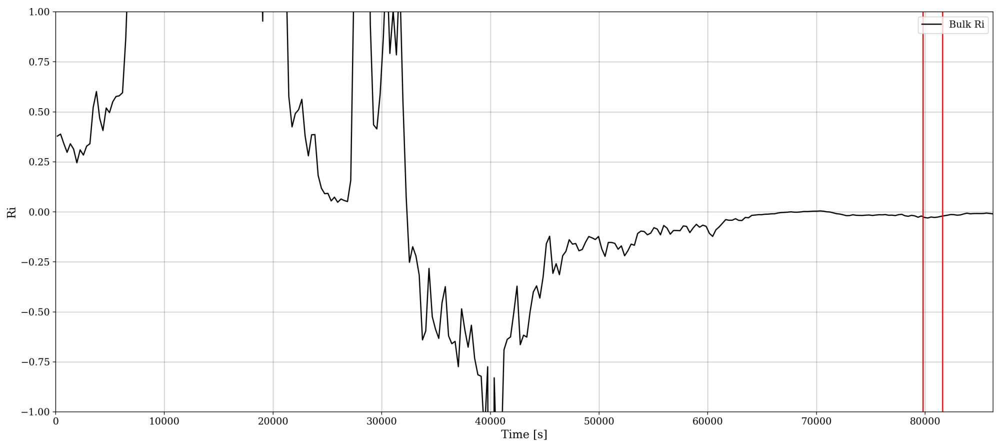

Figure 1: Bulk Richardson number

L246-250: Is it justifiable to use uniform roughness for this simulation? Have you performed any sensitivity analyses if the sufficient resolution threshold changes when non-uniform roughness is used?

REPLY:
It is justifiable only to reduce the number of variables influencing the problem. Our main objective was to assess the resolution required for the digital terrain model. There was no sensitivity analysis on the effect of non-uniform roughness; this will be the subject of future publications, as stated in the paper. The land coverage at Perdigão is a heterogeneous distribution of eucalyptus and pine trees and will require an approach different from simple characteristic roughness.

L250: Does the impact of the first node height show any convergence, like it is the case for the horizontal resolution? You state that this is worthy of further studies, whereas this is the very study where this should be evaluated.

REPLY:
The impact of the first node height would bring to the discussion other aspects of

computational modelling, beyond the scope of the present paper, focused strictly on the digital terrain model and how accurate the description of the terrain surface must be. A large proportion of Perdigão area is covered by trees, a canopy model will be required to resolve these regions and the first node height is not the only parameter to take into account when using canopy models Lopes da Costa et al. (2006) Silva Lopes et al. (2013).

L278, Figures 12,13: It is not clear what in these figures exactly illustrates the impact of mesh resolution. Are we supposed to see the differences in the insets? Please provide more information in the captions and some discussion about the figures in the text.

REPLY:
Figures 12 and 13 were redrawn and the lines in the insets were corrected.

L282: The statement how too high z_0 and u_* yield a high loss of momentum is probably badly formulated.

REPLY:
The text was rewritten.

L291, Figures 14, 15, 16: There are several revealing features in the figures, but only the absolute wind speed, and to some extent the TKE, receive any discussion. The diverging wind speed in the higher altitudes in Figure 15a is particularly interesting. The legend is hard to read.

REPLY:
Figures 14, 15 and 16 were redrawn and the text rewritten.

L294: Please clarify what is meant by "... where a pattern similar to the slope (Table 4) can be observed."

REPLY:
The text was rewritten.

L294: Is the RMSE calculated over all the points in the vertical where the measurements are available? The relevance of this statistical measure in a situation where the basic characteristics of the two datasets disagree quite fundamentally (e.g. the TKE at the tower 20/tse04, Figure 14) is probably questionable.

REPLY:
No, the RMSE is calculated over the whole profile; it is the RMSE measured against the results on the 20 m resolution mesh (ALS). It is questionable, but once we use this indicator for one variable it should be used for all others.

L304: This conclusion 1. is very categorical, while not entirely based on presented facts. At the resolution of 40 m, which is suggested as sufficient for Perdigão, the RMSE for the SRTM-based simulation is not systematically larger than of the other two datasets; in fact, it can be seen that in terms of RMSE for the 40 m resolution the SRTM-based simulations often have the lowest RMSE (Tables 6, 7, 8).

REPLY:
This conclusion is based on the SRTM ability to replicate the ALS data. It is not based on the comparison of flow results using computational meshes from different DTM sources. This conclusion is based on section 4, namely Figures 6 to 11, and Tables 3 and 4, and is based on the ability of the SRTM data to replicate the terrain elevation and slope as measured by the airborne laser scanning (2015).

Please note that the conclusions are organized into three groups: related to the DTM sources (numbered from 1 to 5), flow model (numbered from 1 to 3) and the final recommendations (numbered from 1 to 3).

References

J. P. D. B. Carvalho. Stationarity periods during the Perdigão campaign. Master's thesis, Faculty of Engineering of the University of Porto, 2019. URL `https://repositorio-aberto.up.pt/bitstream/10216/122036/2/348346.pdf`.

J. C. Lopes da Costa, F.A. Castro, J.M.L.M. Palma, and P. Stuart. Computer simulation of atmospheric flows over real forests for wind energy resource evaluation. *Journal of Wind Engineering and Industrial Aerodynamics*, 94:603–620, 2006. doi: 10.1016/j.jweia.2006.02.002. URL `https://www.sciencedirect.com/science/article/pii/S0167610506000328`.

C. Mallet and F. Bretar. Full-waveform topographic lidar: State-of-the-art. *ISPRS Journal of Photogrammetry and Remote Sensing*, 64(1):1–16, January 2009. ISSN 0924-2716. doi: 10.1016/j.isprsjprs.2008.09.007. URL `http://www.sciencedirect.com/science/article/pii/S0924271608000993`.

A. Silva Lopes, J.M.L.M. Palma, and J. Viana Lopes. Improving a two-equation turbulence model for canopy flows using large-eddy simulation. *Boundary-Layer Meteorology*, 149(2):231–257, November 2013. ISSN 0006-8314, 1573-1472. doi: 10.1007/s10546-013-9850-x. URL `http://link.springer.com/article/10.1007/s10546-013-9850-x`.