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

authored by
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Submitted for publication in the *Wind Energy Science*
`https://doi.org/10.5194/wes-2019-96`

Reply to comments by Reviewer 1 (M. Paul van der Laan, DTU Wind Energy)

The authors investigate the impact of different terrain models with different resolutions on atmospheric flow simulations of the Perdigão site. The authors recommend a horizontal threshold resolution of 40 m. The authors define a *threshold resolution*, as the resolution beyond which the model quality deteriorated quickly, but below which no significant improvement in modelling results was observed. The article is well written, follows a good structure and includes a lot of useful technical information about the Perdigão site and related terrain models. However, I have two main comments about the article.

My main concern about using a *threshold resolution* is the fact that is not general, while it is the main objective of the article to find such a resolution, as the authors mention on Page 3, Line 68. The required resolution of the terrain map and flow model mesh are dependent on the quantities of interest. For example, one could be interested in the integrated drag force of the entire terrain, which might require a less strict level of refinement compared to a specific profile of a flow quantity near the ground and in the lee of a steep hill. This means that the concept of a threshold resolution, as mentioned in the introduction is not general, but it depends on the quantity of interest. In addition, the chosen flow model can also influence the required resolution. This is the reason why every article including a new flow model setup needs to be verified by a grid refinement study.

REPLY (First/Main concern or comment):

1. As shown by the title, our main concern is the terrain modelling, not the flow modelling or the flow results. First, we wanted to know to what extent the resolution of the digital terrain model (DTM) affects the terrain modelling and, second, how the terrain modelling affects the flow results.
2. The first (assessment of resolution on terrain modelling, section 4) was carried out by comparing terrain modelling results (terrain elevation and slope) on meshes of different resolutions (80, 40 and 20 m) based on two DTMs of different resolution (SRTM and Mil) against terrain modelling results on meshes based on a higher resolution (ALS) DTM.
    - This analysis was made on the basis of terrain properties (or attributes) of interest to atmospheric flow modelling; i.e. terrain elevation and terrain slope. Because these two are the most important and for the sake of simplicity, land coverage (roughness) was assumed constant.

3. The second (impact of terrain model resolution on flow model results, section 5) was carried out by analysing the flow results based on those meshes and DTMs.
   - This analysis was based on the vertical profiles of wind speed, wind direction and turbulent kinetic energy at three locations, and flow pattern at two transects. These variables and the locations were selected because of their importance in wind energy and sensitivity to the mesh resolution.
4. About *threshold resolution* and flow model results we note the following:
   (a) One must distinguish between the *threshold resolution* in the context of the terrain model and in the context of the flow model. The former is much simpler, independent of the flow model and was dealt with in full in section 4.
   (b) In the case of the flow model results, the *threshold resolution* depends on the flow model (e.g., discretization techniques) and for the same flow model will depend of the parameters of a given run (domain size, boundary conditions, mesh resolution along any direction, etc.).
   (c) The mesh is comprised by the horizontal mesh describing the terrain shape (the digital terrain model), often a terrain-following coordinate system, and the mesh along the vertical.
   (d) There are guidelines for setting the vertical mesh, namely the distance between the first grid node and the ground, but there are no guidelines for how fine the horizontal mesh should be.
   (e) The comment that *threshold resolution* is not general and depends on the flow model, suggests that our conclusions cannot be generalised. We are aware of that, nevertheless, we consider ours a useful work, because we are not expecting the resolution requirements (or the *threshold resolution*) to be much different, given the similarities among the available models.

My second main comment is that not all conclusions are motivated by the presented results because the grid errors of vertical profiles of wind speed, wind direction and turbulent kinetic energy are not directly shown and not clearly quantified.

REPLY (Second main comment):
We do not understand this comment. The vertical profiles of wind speed, wind direction and turbulent kinetic energy are shown (Figures 14, 15 and 16) for all meshes (80, 40 and 20 m) and DTMs (SRTM, Mil and ALS) and the errors quantified (see RMSE in Tables 6, 7 and 8).

I have listed related and additional main and minor comments below, which need to [be] addressed in order to consider a publication in Wind Energy Science.

Main comments

1. Page 3, Line 54: What is meant *by terrain attributes and topographic meaning in Deng et al. (2007) indicates that the mesh resolution can change not only terrain attributes in*

specific points but also the topographic meaning of attributes at each point. Do you mean roughness length? Please clarify.

REPLY:
Terrain attributes are elevation, slope, plan and profile curvature, and topographic wetness index, as defined in lines 45 and 46. For instance, Deng et al. (2007) listed and evaluated six terrain attributes (slope, plan curvature, profile curvature, north-south slope orientation, east-west slope orientation, and topographic wetness index) as a function of DTM resolution.

The terrain attributes of interest to atmospheric flow modelling are terrain elevation, terrain slope and terrain or land coverage, i.e. roughness.

2. What do you mean by *switched on 1 sec registration after assistance by the Portuguese National Mapping Agency*? Do you mean a sampling frequency of 1 Hz?

   REPLY:
   Yes.

3. Section 5.1.1: I do not understand that you k profile is varying with height. If you use a log profile for the streamwise velocity at the inlet, then I assume that you are modelling an atmospheric surface layer, which is represents a constant k value, as discussed by Richards and Hoxey (1993). In addition, if you model a boundary layer height by simply capping it above a certain height, how do you make sure that such an inflow profile is in balance with an empty (flat terrain) flow domain? If the inflow profile is not in balance with your RANS model, then the results at area of interest are dependent on the distance of inflow boundary to the area of interest, which is highly unfavorable.

   REPLY:
   The distance between the inlet boundary and the area of interest was identical in all simulations.

4. Page 6, Line 157: You mention *The slope, ... varies between 21.08^o and 45.09^o, always above the threshold for flow separation (Wood, 1995).* However, flow separation also depends on the roughness length and atmospheric conditions as turbulence intensity and atmospheric stability. So an attached flow could exist for a hill with a 21^o slope if the conditions allow it. Wood (1995) only looked at neutral atmospheric conditions. Therefore, I think you rephrase your statement that flow separation is likely to occur for the site your are investigating.

   REPLY:
   Changed, as suggested. The new version reads:
   The slope ($S = |atan(h_{SW,NE}/2)|/\ell_{SW,NE}$), also on a 20 m *grid varies between* 21.08° *and* 45.09°*, always above the threshold for flow separation under neutral conditions (Wood, 1995).*

5. Page 16-17, Lines 272-274: You mention that results from 20 and 40 m meshes yield similar results and appear to be accurate enough for computational modelling of atmospheric flow

over Perdigão based on Figure 12, using the reversed flow regions. Please clarify *appear to be accurate enough* by quantifying the differences in order to motivate your statement. You could also remove this statement and quantify the differences in Section 5.4.

REPLY:
The text was rewritten and experimental values by Menke et al. (2019) included.

6. Figures 14-16: I would not plot the simulation results of the different meshes together with the measurements in the same figure in order to separate the grid refinement study (model verification) from the model validation. I would also remove the statement on Page 19, Lines 286-287: *For some reason, in the valley the best agreement with the experimental data occurred in the case of the coarser meshes.* (A common mistake in literature is to choose a coarser grid because it compares better with measurements and I would recommended that you do not suggest the reader to do so.) If you would like to include a model validation, you could make separate plots of the chosen grid size (or finest grid size) for each terrain input model and measurements. In addition, have you tried to normalize the measurements and CFD results of wind speed and TKE with their local friction velocity u_{*0}?

   REPLY:
   Computational and experimental results in the same figure are necessary to show how similar or different they are. Plots of the chosen grid size for each terrain input would increase the number of figures. By plotting the results at the valley we are not suggesting that the reader should choose the coarser meshes; cases like these, where coarser meshes provide better agreement with experimental data, are not so uncommon. The text was rewritten.

7. Page 19, Lines 288-290: You mention: *As a whole, results depend more on the resolution than on the DTM and at least a resolution of 40 m is required. Differences between the computational results on 20 and 40 m resolution meshes are minor and within the uncertainty of computational modelling.* This statement is not sufficiently shown in Figure 14-16. I would suggest to (also) plot the differences between the inflow profiles in percentages as function of height, with respect to the reference simulations. This should provide a more clear presentation of the differences between the simulations compared to the RMSE values of Tables 6-8. You can then conclude that the grid error in (for example) wind speed at the reference locations, at a certain height (e.g. around a typical onshore wind turbine hub height) is x% and then it is easier to quantify the impact of grid resolution and terrain model on a wind resource assessment.

   REPLY:
   First, we do not see how the suggested representation can provide a clearer presentation of the differences between the simulations. Second, this type of representation (in absolute values) gives us an idea of the uncertainty for each variable in its own absolute values.

8. Page 20, Line 304: I could not find the second statement of the conclusion elsewhere in the article: *Only meshes based on the ALS have the ability to reproduce the smaller scales between 10 and 100 m.* Please remove the statement from the conclusion or motivate it in the article.

REPLY:
This is related to Figure 11 and section 4.4.

9. Page 21, Lines 320-327: The conclusions made here are not motivated by the results presented in the article.

   (a) I do not agree with the concept of a threshold resolution because it depends on both the applied flow solver and quantity of interest. You can either remove it or reduce the statement by writing that required resolution only applies to your investigated quantities of interest and the chosen flow solver. This also applies to the abstract, title and motivation.

      REPLY:
      Please, refer to our first comment on terrain attributes and objective of the study. Note also that the title is already a long one and the conditions under which our study was carried out are described in full in the abstract, including the flow solver.

   (b) Statements 2 and 3 should be motivated with plots showing the grid error in terms of percentages, as mention previously.

      REPLY:
      This matter is the subject of section 4, thoroughly illustrated by Figures 6 to 11, and Tables 3 and 4. The statement (2) that SRTM should be restricted to far away regions is supported by Table 3, where it can be seen that the absolute error of SRTM is equal to 10 m. Statement 3 is a final recommendation and message of the article, where we say, please use this dataset (ALS) and meshes of at least 40 m horizontal resolution.

Minor comments

1. Section 1.1: It is more common to use a past tense instead of a present tense when referring to literature. (For example on Page 3, Line 59: develops → developed.)

   REPLY:
   You are right. The whole section 1.1 was revised.

2. Page 3, Line 64: Please rephrase *..the flow with the sharp edge..*, because it is the terrain model geometry that has a sharp edge, not the flow.

   REPLY:
   It now reads *the cliff with the sharp edge*

3. Page 15, Line 253: I would abbreviate Reynolds-averaged Navier-Stokes as RANS, which is more common. In addition, you forgot to define RANS.

   REPLY:
   The acronym was removed, because it was not used.

4. Page 15, Line 253: I would write the two equation $k - \varepsilon$ model with k instead of κ, as κ is commonly used as the Von Karman constant and you also use k as the turbulent kinetic energy.

   REPLY:
   This was a misspelling. It should be as in line 247.

5. Not all reported values need to reported fully. For example, you could report 22071075 m^2 as 22.1×10^6 m^2 (Page 4, Line 79) and 993198375 as 10^9 approximately (Page 4, Line 79), this also applies elsewhere in the article.

   REPLY:
   These are detailed technical specifications, usually found in aerial topographical surveys, useful to readers in this area.

6. Page 6, Line 33: I would rephrase the parallelism between the two ridges.

   REPLY:
   We do not see anything wrong with this sentence.

7. Page 7, Line 145: Do you know the distances between the ridges in cm? If not, I would remove the two zeros.

   REPLY:
   Yes, you are right.

References

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