Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2019-106-RC2, 2020
© Author(s) 2020. This work is distributed under
the Creative Commons Attribution 4.0 License.

WESD

Interactive comment

Interactive comment on "Evaluation of the Lattice Boltzmann Method for wind modelling in complex terrain" *by* Alain Schubiger et al.

Anonymous Referee #2

Received and published: 13 March 2020

General comments:

This paper deals with the evaluation of an LBM method for modelling the neutral atmospheric boundary layer over complex terrain. I like the idea to promote LBM for wind energy applications, and found the paper interesting and of overall good quality. Presented results are very encouraging. Although I have some remarks regarding the methodology (see specific comments).

Specific comments:

The main drawback of this paper lies in the differences between the models that are compared. I understand this is an evaluation of the LBM method, and thus it comes with its own limitations (no terrain fitted meshes in this case). However, there are

Printer-friendly version

Discussion paper

large differences in the meshes that are used (fully cartesian versus vertically stretched meshes), but also mesh sizes (quite different cell numbers, probably due to the stretching applied in the NS solver? Why not using some mesh “coarsening” with Palabos?), the turbulence models (LES vs DES), boundary conditions (staircase vs terrain fitted). Under these conditions, it is difficult to compare the models and draw conclusions (thinking about the conclusion regarding the use of roughness boundary conditions in NS solvers). Although it can be understood that solvers are intrinsically different and methodologies adapted to each solver have been used, I think it could have been interesting to reduce the differences when possible, comparing the models using the same meshes (no vertical stretching), similar turbulence models (LES vs LES), and same roughness models (slip and no-slip for the NS solvers).

From my point of view, a first, preliminary study regarding velocity and turbulence intensity profiles on a simple flat terrain could have brought insight to the model comparison, rather than directly addressing the complex terrain case. Even on this complex terrain case, a comparison of the velocity and turbulence intensity profiles (as shown in Bechmann et al.) are missing, and could provide more insight in the comparisons.

I also wonder about the potential of LBM to handle terrain roughness. The authors used wall-slip conditions for the ocean and no-slip conditions on land with the LBM solver. Isn’t it possible to account for the terrain roughness more precisely, using partial-slip boundary conditions? Is the use of a logarithmic profile at the inlet sufficient to model an ABL? Some insight would be welcome.

One last point that is missing is the choice of the collision model. A discussion is proposed regarding the different possibilities (SRT, MRT, entropic, etc.), but the choice is made to use the standard BGK model, which is not supposed to be the most stable. Moreover, the choice of the relaxation parameter “tau” is not discussed (i.e. is equation 6 fully respected?). A small discussion on the non-dimensioning procedure could also be interesting.

CC BY

Finally, the “code and data availability” section is not present. Can the Palabos simulation setup be shared with the community? It would probably help researchers to get more familiar with LBM and its application to wind energy.

- Page 1 Line 13: LBM is said to have a particular ability to automate the geometry. The argument is often retained to promote LBM methods. However, I do not see the advantage of LBM in comparison to cartesian-grid Navier-Stokes solver with immersed boundaries. Can the authors comment on this point?

- Page 5 Line 12: the authors should be more specific regarding the value of the Smagorinsky constant they have used. Also, is it the same Smagorinsky model used in the NS solver?

- Page 6 Line 22: more details should be given regarding the inflow turbulence generation. Is the same methodology used in the NS solver?

- Page 7 Line 8: some details regarding the mesh used for NS simulations are given. From my understanding, the mesh is wall-adapted. The authors should make it clear.

- Page 7 Line 20: average results of the DES simulation should also be shown.

- Page 13 Line 9: I think this conclusion should be argued, and, from my point of view, is not receivable. There are too many differences in the models to draw such a conclusion (different meshes, turbulence models?, different wall boundary conditions, etc.)

- Page 13 Line 13: LBM is said to be 5 times faster than DES. However, the total CPU time is only 30% lower. Perhaps a comment on the mesh size reduction that could be obtained(using mesh refinement techniques) would help clarify the potential of LBM methods to reduce CPU time. Anyway, would it be possible to have similar meshes between LBM and NS even using mesh refinment, and, have LBM solvers the same mesh size requierements than NS solvers?

Technical corrections:

CC BY

Figures text size should be made uniform in the different plots. In the current version, fontsize is obviously too small to be readable (Figs 1, 4, 5, 6, 7, 8).

Page 1 Line 15 : doubled dots

Page 2 Line 22 : a extremely fine –> an extremely fine

Page 4 Eq. 5 : "with" in italic and attached to "f_i"

Page 6 Line 40 : "to an total" –> "to a total"

Page 9 Line 12: a reference to the figure should be added

Page 11 Line 5: space between "et al." and parenthesis.

Page 11 Line 6: "summarise" –> "summarize"

Page 13 Line 2: "is far" –> "it is far" or "LES is far", or replace "; however" with something else to improve readability

Page 13 Line 8: "of cliff" –> "of the cliff"

Interactive comment on Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2019-106, 2020.