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Hydrology and Earth System Sciences Discussions

Interactive comment on "Aerodynamic roughness length estimation from very high-resolution imaging LIDAR observations over the Heihe basin in China" *by* J. Colin et al.

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Dear Referee, Please find below the answers to each of your comments and questions.

[Referee] The subject of this study is interesting. Simulation of the aerodynamic roughness length over heterogeneous landscape and discussion of its interaction with wind flow will im- prove our understanding of the hydrological process over land surface. This manuscript gives an example to how aerodynamic simulations can benefit from high resolution 3D surface structure model acquired by remote sensing. The result is promising but still with space for further improvement. I have two major questions: 1.

Treating trees and crops as solid blocks is a too rough approximation. This may be the main reason for the large difference between model simulation of wind speed and the measurement of AWS. Is it possible to give some simple consideration of the porosity of vegetation, and discuss about the result?

[J.COLIN] Dear Referee, The difference between simulated wind speed and AWS values is due to the design of the CFD itself. To compute the wind fields with this CFD approach, we do not have boundary forcing conditions (e.g. coarser estimates of the wind field over a large area). Therefore, the wind speed from the AWS (single point) is used in this study as an initial value to iteratively compute the wind fields. Starting from this initial value, the actual heterogeneity of the surface roughness is iteratively introduced in the system, and propagated from cell to cell. The consequence is that the final wind speed at the location of the AWS might be numerically different from the measured value (which wasn't the target value to converge to, but the initial forcing data). We agree that considering a vegetation canopy as a solid block is a rather rough assumption. As we mentioned in the discussion, the simple representation of the roughness elements cannot account for the porosity of the foliage structure. To parallel aspects can improve this situation: i. the use of the fullwave form of the LIDAR signal should allow for a better representation of the canopy vertical structure. Such kind of approaches were already publish. Unfortunately, we only had the first and last impulse of the LIDAR signal at the time of this study ; ii. to adequately account for the vegetation porosity and related parameters derived from LIDAR measurements, we would need to implement our own CFD model. Such a work is not the core of our activities, and will require an adequate collaboration.

[R] 2. The roughness length derived from CFD is related to wind direction, but roughness lengths derived from other methods may not be related to wind direction. Which is more consistent to the meteorological or other studies?

[JC] This question is closely linked to the spatial resolution of the calculation as regards surface heterogeneity and footprint scale of local roughness elements within the scene.

In this study, the area considered is of high heterogeneity, both in terms of land cover, crop types and height. Therefore different wind directions lead to very different results, as illustrated in the figure 3. We are now trying to perform the same calculations over an arid tundra, and even if the results are not available at present, we intuitively expect a much less impact of the wind direction on the results.

[R] Generally speaking, the manuscript presents new method to estimate roughness length as well as new data. But it is kind of roughly written. I would suggest the author put more effort to refine the manuscript.

[JC] I would like to apologize for the poor english of the paper. We carefully worked on the text again, and hope this new version will give satisfaction.

[R] Some other comments and suggestions are as follows. 3. In section 3.5, it is said "The AWS wind speed and direction measurements at 2 and 10m are used to initialize the profile". Then, my question is that why the simulated wind speeds at AWS position and height are so much different from the initial value. Is there any data assimilation method can be helpful to bring the simulation close to initial value? I fail to find answer in the sentence in page 3407 "As quoted in Sect. 2, this is due to the solving of the transport equation".

[JC] The answer to the question 1 gives explanations to this point.

[R] 4. It is not clear how the atmospheric sounding data are used in CFD model, and what is its relation with AWS data. What is "PBL height"?

[JC] PBL is the Planetary Boundary Layer. The acronym wasn't mentioned clearly in the beginning of the section 3.5, I changed this and I used the full name as often as necessary. The height of the top of the boundary layer is mainly useful in the CFD model in situations of non-neutral atmospheric stability. In such cases, the vertical boundary may be considered as an impervious slip wall. In our case studies, as we always have neutral stability situations, this parameters is not significant, but must still

HESSD

7, C2140–C2144, 2010

Interactive Comment

be provided.

[R] 5. In Table 2, the date is 30 June and 14 July. But in Table 2, the date is 30 July and 14 June. Which is correct?

[JC] This is now corrected (table 2 was the right one).

[R] Another question is why these specific date and time are chosen for study. I found it not understandable to choose 4 times in 30 June and 1 time in 14 July.

[JC] This was imposed by our neutral stability criteria.

[R] 6. A major point in this paper is to use high resolution DSM in roughness length estimation. I wonder that if there is no high resolution DSM, e.g. using classification map from TM image to assign height value, how much will affect the result?

[JC] None of these approaches should be used in such cases. The main motivation of this study is the availability of airborne LIDAR measurementsÂăwith a metric ground resolution. Only such very high resolution datasets can allow to identify not only the obstacle height, but also gaps between obstacles. That said, in most of the studies in environmental sciences, large areas are considered. Therefore, one of the aspects we investigate is how the coupling of very high resolution LIDAR and visible to near infrared remotely sensed observations can be combined to improve roughness length algorithms based on vegetation indices (eg. the relation between the fullwave form, leaf area index and canopy roughness).

[R] 7. In Fig.3, what is the meaning of each sub-figure?

[JC] The same computation for various wind directions. The label of the figure was extended and improved.

[R] Another question is has the edge effect been considered and removed? My expression from the figure is that the experiment area is very small.

[JC] The size, in particular the width, of the experiment is depending on the swath of the

HESSD

7, C2140–C2144, 2010

Interactive Comment

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LIDAR system. The only solution to extend the area would be to make parallel flight paths. As regards the border effects, this can be removed following a nested scale approach, as mentioned in the conclusion. This requires a coarser but larger DEM (eg. ASTER) to compute the wind fields on a larger extent, then use the results as initial data for local simulations. This is still under progress.

[R] 8. In Fig.4, what does the number at X and Y axis mean?

[JC] These are pixel coordinates. These numbers do not provide any information and are confusing. Therefore we removed them from the figure 4.

[R] How large is the total area.

[JC] The total area is 7.2 square kilometers (mentioned in the introduction and in section 3.2). We have duplicated this information to figure 4 and 5 labels, as well as in adequate parts of the text.

[R] Can the author give a true color image for the exact experiment area?

[JC] We have made a new figure 1 to provide a better view of the area.

We wish to thank you for your interest in our paper and your detailed comments. Should you have further questions, please feel free to contact us.

With our best regards,

J.Colin, R.Faivre and M.Menenti

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 3397, 2010.