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Hydrology and Earth System Sciences Discussions

Interactive comment on "The use of LIDAR as a data source for digital elevation models – a study of the relationship between the accuracy of digital elevation models and topographical attributes in northern peatlands" *by* A. Hasan et al.

Anonymous Referee #1

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General Comment

In this paper the authors describe a set of data analyses aiming to test the accuracy of Digital Elevation Models (DEMs) created from the same LiDAR data and interpolation technique, but using different interpolation settings (i.e. search radius) and DEM resolutions (i.e. grid-size). A series of check points (i.e. evaluation data) have been selected in order to estimate the accuracy of the elevation values provided by the different DEMs. Special attention has been paid to i) the variability of the DEM accuracy in

relation to the 'ground' slope and ii) the variability of drainage area estimates in relation to DEM resolution.

While the subject of the paper could be well-within the scope of HESSD; in my opinion, the results provided in this manuscript fail to reach one of key criteria for publication, namely these lack the impact, significance and novelty expected of a HESSD article and may therefore fail to be of wide appeal to the readership.

Specific Comments

My main criticism is that, in my opinion, the research questions the authors have formulated have been previously addressed. This criticism goes, in some way, in the same direction the comment posted by Salvatore Grimaldi in his recent Interactive Comment on 11th July 2011. In his comment, he literally found that 'reading the manuscript it is frustrating the authors did not feel enough important the literature about their specific analyses and to specify the literature behind the methods used in the proposed analysis'. As he has reported, there are numerous studies done by his research group focusing on some of the methods the authors have used that deserved a full review and inclusion in the manuscript (I will not refer to this in my review since these studies have been cited already in his comment, providing the link where all reference could be seen). Therefore, an exhaustive literature review would help the authors to realize that some of the hypothesis they based their work have already been addressed and tested. I will not provide a full list of publications they could review and include, but I will identify two publications that cross-correlate with their specific objectives:

(I) In some way, main research questions of this manuscript are addressed and discussed in the review provided by X. Liu in Progress in Physical Geography (2008, 32(1), 31-49). In this paper the author listed and reviewed some critical issues behind the generation of DEMs from Airborne LiDAR data. Specifically, section IV provides a nice and complete discussion about model, interpolation and resolution for DEM generation. The discussion and the numerous references listed in this section may be useful to the authors.

(II) S. Erdogan presented a comparison of interpolation methods for producing DEMs in Earth Surface Processes and Landforms (2009, 34, 366-376). In this work the authors may found a full analysis of the accuracy of DEMs based on the Inverse Distance Weighting (IWD) interpolate technique (same used in their manuscript). This work also identifies steep surfaces as regions of less accuracy when DEMs are generated. Some of the findings in this paper may be also useful to the authors.

A part of this main criticism, I found that basic information is missing. I'm referring here: (a) general details of the data used to elaborate the DEMs and, (b) some methodological aspects that have not been fully described. The later issue is also reported by S. Grimaldi; he suggested explaining in more detail the applied methods (see last paragraph in his comment). In terms of (a), I would like to point out that the manuscript does not describe anything about the quality of the original dataset used to generate the DEMs. I'm referring here to the accuracy of the LiDAR survey. As far I'm concern, vertical and horizontal LiDAR data accuracy could be in the order of $\sim$10 cm -each. These values are important to contextualize the accuracy values reported in the paper by comparing DEMs and evaluation data. Some of the accuracy values found in the paper are < $\sim$10 cm, clearly smaller than the potential accuracy of the LiDAR data set, form which DEMs are generated. More information and discussion would be necessary to make clear the magnitude and significance of the accuracy errors found in the paper. Additionally, some of the methods applied are not fully described or have some degree of uncertainty that may confuse the reader. Therefore, in terms of (b), I would suggest adding more details about, for instance, the way catchments have been delimited. Some of the computational uncertainties involved in these calculations may be in the same range of the accuracy found by comparing catchment areas from different DEMs; then, more discussion about the method applied to calculate the contributing areas would be necessary. Some graphical information would be necessary to show the reader the products that have been elaborated (already addressed in the Interac-

tive Comment). In the same way, the method used to estimate the slope would need to be described or shortly reported. Some of these methods have been applied in other studies (and have been referred) but, in my opinion, a brief summary would be useful to the reader. Finally, I think the description of the methodological approach used to generate the check points (i.e. evaluation data) needs also further details. I would suggest reviewing the way Erdogan (2009, see reference above) described different validations approaches: leave one technique, split sample method and independent set of sample approach. The criterion I have seen in the manuscript is based on the distance between the evaluation points and cell centres of the DEMs, but in any case details about the spatial distribution of these are provided.

Conclusions and Final Recommendations

I conclude that the paper reflects work that is, from my point of view, still at a preliminary stage of development, and would benefit from further research prior to publication. The authors have justified the significance of the analysis in relation to the importance of elaborating high accurate DEMs to improve the estimates of the Topographic Wetness Index (TWI) in peatlands environments where the change in elevation between neighbouring points is relatively small, while the difference in wetness is rather high. In my opinion then, it would be appropriate to evaluate the TWI using slope and catchment area estimates from different DEMs and provide, then, a fully description of the degree of variability of these indexes according to DEM resolution and accuracy. The accuracies reported in the manuscript may or not interfere in the final values of the TWIs. At this stage the reader does not know the degree in which the TWIs may vary and how this may effect further estimations or calculations of, for instance, gas emissions ... These analyses would provide to the paper more significance and novelty, it would be a step forward to the DEMs accuracy approach presented in the original work; altogether deserving the publication of the manuscript. In summary then, my opinion is that the paper requires significant further development before it is ready for publication in a major journal such as HESSD. With this is in mind, my recommendation is to reject the paper in its current form but encourage the authors to continue their research and submit a revised manuscript following the open discussions and the comments provided by the referees and the editor(s).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 5497, 2011.