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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Evaluating topographic wetness indices across central New York agricultural landscapes" *by* B. P. Buchanan et al.

Anonymous Referee #3

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The study cover an evaluation of TWI generated from two different datasets. The evaluation includes parameters as the resolution, source, flow accumulation algorithm, inclusion of soils in the TWI and filtering. The study area is in New York, USA. The study takes a relative approach on deciding on a number of input parameters to calculate TWI. This is then synoptically evaluated against measured soil moisture.

Parts of the background, i.e. the effects of each of the input in the TWI formulation is presented and accounted for in the methods section. This is unevenly distributed with rudimentary background/discussion on cell size and slope calculations as oppose to the flow accumulation algorithm. The benefits and downsides of the other inputs would strengthen the discussion in the end.

The resulting derivatives of elevation models are to a large extent a product of the input, in this case the datasets called USGS and LiDAR. The data quality of those datasets differs with RMSEs of 2.44m and 0.15m respectively. An evaluation of these parameters or at least an overview of what is stated in the literature on this topic would improve the paper.

When it comes to the smoothing scheme carried out in the study, the problem of TWI's with "high local variations" is something that concerns me. It is not explained how this method logically would improve the result. Smearing out errors in a DEM with a RMSE of 2.44m generate a different impact on elevation values than in a DEM with a RMSE of 0,15m. This issue is more of a data quality issue than how to obtain a good TWI value. I argue that if such a procedure as filtering is needed then the data is not at all optimal for the task. Another issue related to this is if DEMs were filtered before all derivatives were calculated?

The source data discussion is also related to the data quality and resolution. Since LiDAR data often offer a higher point density and a better point distribution, the generation of DEM with almost any chosen interpolation algorithm is better than data obtained with a coarser sampling method. A deeper discussion of the data quality, generation of DEMs and resolution on how these parameters affect the TWI in the study should be added. The connection between the quality of data and the generation of TWI is also missing in the flow accumulation discussion. I get the impression that the authors want to connect this with the sampling method. The way the data was obtained is only important in the data quality, point density, point distribution and DEM generation. This applies to both the LiDAR and the USGS discussion. In the section 3.5.1, P14060L12-16, the authors obviously think there is a risk that conversion may occur in their dataset as Park's results are referred to. However, it is also stated that this was for larger cell sizes than 20m. In this study the resolutions studied are 3 and 10m so why is there a need for this in the present study?

The comparison of TWI and VWC becomes vague in its shortness. The relation of

TWI and deviation from mean VWC is not accounted for. A study or a comprehensive presentation of references should be inserted.

Conclusions meet and answer the questions raised in the introduction. Given the way of presenting and discussing the results it is good to have it all covered in an overview.

Table numbering must be edited.

P14057L6-7: “3m TWIs” is a confusing description. Another way of putting it would be “the TWIs generated from the 3m resolution DEM”

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 14041, 2013.