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Hydrology and Earth System Sciences
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Discussions



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

**Anonymous Referee #1**

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This study explores the impact and influence of various approaches for defining and determining TWIs. The study is well written and well conceived. Further, there is need for such a review analysis in the scientific literature. For example, it is a commonly faced problem in the modeling community to determine (1) the best DEM resolution for the task at hand and (2) which TWI techniques to use in practice. With that, this study helps fill the void by working through several combinations of techniques and resolutions and exploring the potential impact.

One part that is a bit of a concern in this study is the fairness of comparison between soil moisture distributions observed in the top 12 cm of soil and the topographic indices. In near stream, near saturated conditions, this comparison may be better supported than one would expect in up slope positions of the landscapes. I wonder what the



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results would look like if soil moisture values were treated with a binary filter (wet v. dry) and then compared to the TWI values. The authors could expand the text a bit to better develop the theoretical connections between TWI and soil moisture distributions. This would help balance out the discussion and presentation a bit. It is particularly relevant when considering the flow path delineation influence.

Perhaps this can be taken up through a discussion of temporal variability in water flows relative to the static nature of the indices in and of themselves? That is an aspect of the study that could be better developed. Why have all conditions (wet v. dry) been lumped together for analysis? Would you expect the same results under wet conditions versus dry conditions? I could imagine there been a hysteresis aspect to the landscape wetting up or drying down that could potentially come into play. This could be more thoroughly considered and the consequences put forward (either through analysis or discussion).

Further, it is not transparent from the presentation of the soil moisture observation experimental design if the distribution of observations made in the field come from a representative set of wet and dry locations. Did you divide the regions into various classes and then randomly select sampling locations within each class (a stratified random design)? A bit more detail there would help the reader judge the fairness of the field sampling in comparison to the calculated TWIs. Also, the impact of averaging results across various times of sampling should be considered. Would you expect the same results early in the season (say the spring time) relative to late in the season (say the autumn)? This is fairly central since you are assessing the accuracy of the TWIs relative to moisture observations as opposed to assessing the difference in TWIs relative to each other.

I get the feeling that the authors might have missed the chance to make a statement about the relative contribution of each factor (e.g., LIDAR v. USGS; 3m v. 10m) in the overall error between soil moisture and TWI. It would be really interesting to see a breakdown of the amount of error due to these different factors in explanatory power for predicating soil moisture. Not exactly sure how this would look, but it would be in-



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formative to know that (for hypothetical example) 80% of the error or misrepresentation in a given representation was due to using USGS data while 20% was due to using 10 m resolutions. What is the (quantified) added value we get by going to LIDAR data at higher resolutions? You start into this a bit around the top of P14064, but do not fully develop it. I cannot think of an easy way to estimate this, but it could be taken as food for thought. Maybe adopting some simple comparative examples from your 432 variants and tracing out the explanatory power of the various factors in estimating the soil moisture values. There is some space in this study to expand along some case study examples.

I really appreciate the effort made in section 3.7 of the text. I think this is clearly the heart of the study and its central contribution to topographic analysis literature. That being said, it is rather cumbersome to wade through (even with the help of Table 5). Would it be possible to add in a flow chart or a step-by-step walkthrough to guide the decisions of someone faced with picking an "optimal" TWI approach? Something that highlights "if this, then that" aspects and gives a method to guide to the best possible approach given constraints (like LIDAR v. USGS data). I am thinking along the lines of the walkthrough chart for evapotranspiration calculations provided in Maidment's Handbook of Hydrology. This would help bring forward the main message especially since the results and discussions have been consolidated (which I am not a huge fan of in this current study).

Minor/Editorial Comments

P14042L5: Remove "in the US"

P14042L10: Change "; each field was visited 5-8" to "with each field visited 5 to 8"

P14044L28: Calculate "it" or "them"?

P14047L9: What is the impact of the resolution between 10m DEM and a 3m DEM when you use a fixed number of neighbor cells (four) rather than a fixed distance?



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P14056L26: Not clear what is meant with calibrated here.

P14057L6-8: How much does the 4 cell integration matter here relative to the change in resolution? See general comments. Could you quantify the impact?

P14063L11: Here and everywhere, check Table numbers.

Conclusions: I do not really like the use of a structured list as a conclusion. I think more impact could have been made using a flow chart or decision tree. See general comment.

Table 3 seems wrong in my printout. There was no information in there. . .

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