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Interactive comment on "On the calculation of the topographic wetness index: evaluation of different methods based on field observations" *by* R. Sørensen et al.

R. Sørensen et al.

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We thank Referee #1 for valuable comments. Here we would like to respond to the general comments put forward by referee#1. The more specific comments we will respond to in the revised version of the paper.

"I remain doubtful if the methodology that is presented by the authors can be considered as a reliable evaluation of the different TWI methods. The underlying assumption is that the measured variables are all strongly influenced by the topography of the soil surface and that it is just a matter of finding the best parameter set to predict the variables at the ungauged sites. How can we be sure of this? Other properties (e.g patterns of varying soil conductivity) could be at least as important as the topography."

As we state in the introduction we assumed in this study that there actually is a relationship between TWI and the different variables and that a suitable computation algorithm would provide this correlation. This assumption is supported by Zinko et al (2005) and other studies (Zinko et al. submitted paper) that found significant correlations between TWI and species richness of vascular plants. (It is also known that there is a positive relationship between species richness and pH in boreal regions [Grubb 1987, Pärtel 2002]). Our assumption can of course be questioned and we are aware that other properties might be of importance, but the focus in this paper is the calculation of the TWI. In other words, how strong correlations can we get with the TWI? Many of the adjustments we have made to the calculation of the index have all been individually evaluated before in different studies (Hjerdt et al., 2004; Güntner et al., 2004; Holmgren, 1994; Quinn et al., 1995; Tarboton, 1997). Here, on the other hand, we take an overall look at the well known topographical index including all the individual components of it. We studied the calculation of the index and its application on DEMs. No guidelines to the application of the TWI (and its derivatives) on DEMs have yet been established in the scientific community. For that reason we investigated the importance of the calculation methods of the TWI to emphasize the importance of calculation method and application of the index algorithms on DEMs. Eventually this work should lead to general guidelines on which method to use.

“The authors should put more emphasis on their main working hypotheses because if, for a particular variable, this correlation is not well established, the results become very difficult to interpret. Varying parameter sets could simply compensate for other local phenomena that were not explicitly taken into account. Hence, I think that the actual reasons why a given method performs better than another remain unclear (except for the difference between species richness and ph on the one hand and hydrological variables on the other hand that is well explained on p. 1822). For example, what is the reason for finding different “optimum” methods for soil moisture and groundwater? Furthermore, I do not understand why the authors did not start their study by calculating the correlation coefficients between all the measured variables. In fact, I guess that some of the conclusions that you draw in the end could have been obtained easily

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without having to calculate the respective correlations with the TWI. If there is only a weak correlation between e.g. the plant species richness and the groundwater levels, it is not surprising at all that the best performing parameter sets to calculate the TWI differ. The physical reasons for these discrepancies need to be further outlined."

Since we varied every single model parameter individually we were able to detect connections between the parameter setting and the measured parameters in a detailed way. So if the correlation between the index and a measured parameter varied with calculation method we had data detailed enough to pin point how each single model parameter influenced on the variation in correlation strength. In that way we could couple the model parameter setting to the landscape feature of interest. The next step is the interpretation of these relationships. As we discussed in our response to referee#2, we did not want to speculate and draw any major conclusions in these interpretations since we had no more evidence than the observed relationships. So it is correct that we are not able to present the actual reason why some parameter settings performed better than others. But we showed that the topographical index is more than just the number it presents. It is also a set of assumptions about how the soil water moves in the landscape and how that can be represented in a DEM. Why different settings are better for estimating different landscape parameters is a question of interpretation and speculation. What we did was to discover which parameter settings that had effect on the representation of the different measured variables. In our revised edition of the paper we will give moderate suggestions to the processes behind the observed differences. It is no surprise that the best performing parameter sets to calculate the TWI differ for different measured parameters. However, to our knowledge it has not been quantified before and no one has analysed the different parameter settings individually before. We will add a table of correlation coefficients between all the measured variables, as an addition to the existing results. In general these correlations varied from 0.4 to 0.9 and were mainly around 0.7. A weak correlation obviously is a prerequisite for the possibility that different methods provide best results, but even with low correlations one single method could have been best in all cases.

“I would suggest toning down the conclusions. In the abstract you mention that the “results provide guidelines for choosing the best method”. The main message that I get from this paper is that the best parameters to calculate the TWI are site and variable specific. Thus, it is obviously very “uncertain” to derive any general guideline from your results. The obtained results only hold for the presented case study. Hence, regarding the difficulties to find a “global optimum”, I don’t see how you can derive any general results. Two catchments are certainly not enough to do so and the comparisons with other studies also showed some notable differences. I admit that some patterns were highlighted but is this really enough to guide the choice of appropriate parameters in ungauged catchments? (which I guess is the ultimate goal of such a study).”

As mentioned in our conclusions, the guidelines provided by our study are: (1) that different methods should be explored at an initial stage, prior to performing estimates based on the index, (2) suggestions to the ranges within which the model parameter values could be expected to make the index perform better, (3) suggestions regarding further refinement of the calculation method of the index. We find these guidelines moderate, and do not claim generality. In the revised version of the paper we will clarify that the study is site specific and that the guidelines presented are derived from Fennoscandian boreal forest.

“Obviously the DEM is at the core of the study. Surprisingly only a few details are given on the DEM that was used. We know that the spatial resolution is 20 meters. But what can you tell us about the vertical accuracy? What would be the effect of using more/less accurate DEMs with lower/higher resolutions? I guess that the properties of the DEM would have as much of an impact than the parameters that are used to calculate the TWI. Could you please give some comments on this?”

The DEM was based on measurements with a Zeiss PlaniComp analytic stereo instrument with the Microstation program (vertical accuracy of ś 0.7 m). The accuracy of the DEM would probably have an effect in situations where the scale of the topographical features is lower than the resolution scale of the DEM. Thus it can be expected that a higher DEM resolution will give more accurate estimations of landscape features as long as the spatial scale of the processes behind landscape features are not larger than the DEM resolution. We are currently preparing a paper on the effect of the DEM resolution on the TWI. Initial results show that the TWI becomes more negatively skewed and more evenly distributed at higher resolutions, the latter is mainly explained by the changes in upslope area distribution (Sörensen, Seibert & Stendahl, unpublished).

"Please avoid using the term "optimal". You found the best performing parameter set out of a limited number of plausible parameter sets. Unless I did not get an important point, there was no optimization algorithm involved." Specific comments: p. 1810 line9: "If the methods provided differing results then we sought to determine if it was possible to define an optimal TWI computation method that works well in different geographic areas". I suggest rewriting this sentence so that it becomes clearer what you really want to achieve. In my opinion, an "optimal method" can always be foundˇE but will the global optimum and the specific optimums be alike?"

We will change the wording and avoid using the word optimal. However we would like to point out that our intention never was to find a global optimal TWI calculation method, but one single best calculation method in our study.

"Overall I think that this paper is quite worthwile. I suggest that the authors put more emphasis on the reasons why some TWI methods and parameters perform better for some of the measured variables but not for others."

As mentioned before, we have been careful to not speculate and draw unsupported conclusions. In our revised edition of the paper we will give moderate suggestions to the possible processes causing the observed differences.

"p. 1815: are the tubes only 9 mm wide?" Inner diameter, yes.

"p. 1816: only the soil moisture measurements of July 2002 were considered (which was a particularly dry month). Is it not likely that under these dry circumstances the spatial variability of soil moisture values is much more related to the spatial variability in rainfall amounts than to the differences in TWI (i.e. vertical flows are much more important than the lateral redistributions?). I believe that the soil moisture patterns are much more topography driven in October with shallow groundwater levels. Moreover, on the HP site you only considered the groundwater measurements of October. The relationship between soil moisture, groundwater depths and the degree of wetness needs to be discussed. In fact, before trying to relate them to the TWI, you should give more information on the correlation between these hydrological variables. This would make it easier to interpret the results that are shown later."

We used the July data because in general correlations between TWI and soil moisture were lower for the October data set. As mentioned in the paper soil moisture measurements with TDR-instrument can be a problem if soil conditions are too wet. One should also note that these areas do not get 'very' dry even during July. For groundwater level, the October measurements correlated best with TWI, which probably is due to the fact that groundwater levels were generally higher and we could therefore measure groundwater levels in more tubes and hence get more data points. Topographic indices have been shown to represent the variation in soil water conditions better at intermediate soil moisture conditions as opposed to dry or very wet conditions (Western et al. 1999). We will describe the correlation between the hydrological variables more in the revised paper.

"p. 1816 line9: predictor=independent variable."

This will be changed.

"p. 1816 I am not very convinced by your method to calculate a "degree of wetness".

Why do you give the same weight to the estimated value of groundwater depth than to the measured one??? Moreover, Figure 1 suggests that a linear function between soil moisture and groundwater depth is probably not acceptable." The 'degree of wetness' is an approach to combine soil moisture and groundwater data; whenever both

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types of measurements were available they should get the same weight. While the relationship between soil moisture and groundwater is not linear we suppose that a linear approximation is acceptable within the ranges of our measurements.

“p. 1822 line 13: The authors make the strong assumption “that h should decrease when going from mountainous to hilly areas” The results shown on Fig. 4 do not really underpin this assumption. The calculation of the parameter performing best for the plant species and soil pH gave an h value of 2-8. In my opinion, these results do not differ enough from those that were obtained by Güntner et al. to draw such a general conclusion.”

Güntner et al used saturated areas to compare different TWI calculation methods and we suggest that their results should be compared to the hydrological variables in our study (best results for h between 0.5 and 2).

“p. 1821 line 9: this seems rather obvious. You should skip this unless you want to quantify the loss in correlation. The same statement is repeated on line 23.”

We agree, will be excluded.

“p.1822 In my opinion it would be important to discuss the results that were obtained for groundwater and soil moisture. Could this result have been anticipated by calculating the correlation coefficients between the measured variables? What are the reasons for this difference especially since Figure 1 showed that there is a relationship between the two hydrological variables.”

We will include a table with correlation coefficients between the measured variables One issue, which might have affected the results, is that the subsets of points were different for groundwater and soil moisture since for both variables there were missing values (too dry places for ground water and too wet places for soil moisture); it is only for the variable ‘degree of wetness’ that all points could be included. It could be argued that some methods work better for wetter areas and some for drier.

“p. 1833 Figures 3 and 4 overloaded (very difficult to read with the patterns you used).”

We have tried to deal with the problem by using colours instead of patterns. The results are exposed on the website:

http://web.telia.com/Ÿu18513692/Sorensen_etal_HESSD_fig3_4_color.pdf

“Technical corrections p. 1811 line 19 and line 22: repetition p.1811 line18 and line 24: down slope or downslope (?).”

This will be changed.

Once again we thank referee #1 for interesting comments and suggestions, which we will take into account in the revised version of this paper.

Additional references:

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Western, A.W.,Grayson, R.B., Blöschl, G., Willgoose, G.R., McMahon, T.A.: Observed spatial organization of soil moisture and its relation to terrain indices. Water Resources Research, 35, 797-810, 1999.

On behalf of all authors I would like to express my gratitude for the thorough work of the two referees.

Interactive comment on Hydrology and Earth System Sciences Discussions, 2, 1807, 2005.