Hydrol. Earth Syst. Sci. Discuss., 2, S848–S851, 2005
www.copernicus.org/EGU/hess/hessd/2/S848/
European Geosciences Union

Hydrology and Earth System Sciences Discussions

HESSD

2, S848–S851, 2005

Interactive Comment

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

Anonymous Referee #1

Received and published: 24 October 2005

The scientific merit of this paper is to show the difficulties of choosing the most appropriate model parameters to calculate the TWI. The authors demonstrate that the best performing parameter sets vary considerably from one site to another and for different variables. It seems to be impossible to find an optimal method with respect to all the measured variables. The paper is well written and the methodology is well explained. However, 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.

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 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.

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).

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?

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.

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.

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 foundĚ but will the global optimum and the specific optimums be alike?

p. 1815: are the tubes only 9 mm wide?

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.

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p. 1816 line9: predictor=independent variable

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 acceptableĚ

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.

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.

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.

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

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

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