

Interactive comment on “Spatial modelling of the variability of the soil moisture regime at the landscape scale in the southern Qilian Mountains, China” by C.-Y. Zhao et al.

Anonymous Referee #2

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General Comments This paper proposes that the soil moisture status at a point can be predicted using a modified wetness index. This wetness index multiplies the traditional Beven and Kirkby wetness index by the cosine of aspect and the monthly mean precipitation. The authors evaluate multiple methods to estimate the monthly mean precipitation and then test the method for a collection of 22 points within a catchment. By including these two new factors, the authors assert that soil moisture status predictions are significantly improved of using the traditional wetness index.

While I find this idea novel, interesting, and worth researching, I believe that the avail-

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Interactive Discussion

Discussion Paper



able data is simply inadequate to make any significant conclusions. This paper has several weaknesses in its writing and analysis, but even if those were overcome, I believe it would be better for the authors to strengthen this paper by collecting additional data and/or making further theoretical assessments of this idea before publishing. Again, I believe the idea has merit, but the available data are simply inadequate to make conclusions about its general applicability.

Specific Comments 1. The authors introduce the paper from the perspective of water management in the region where the analysis is conducted. However, the conclusions of the paper and main value of the paper for an international audience revolves around the proposed new wetness index. Because of this selected approach, the existing literature on the relationship between soil moisture and topography is sparsely reviewed. Considerable work has been done examining the relationship between soil moisture and topography, and much more context needs to be provided for this work. This context is critical for the reader to see the possible limitations of this approach, its testing in this paper, and its possible application other regions. This literature should also be referenced in the conclusions to state the limitations of what is shown here. I'm concerned that the paper might give people the impression that this method is generally applicable with the possible limit of variations in vegetation.

2. Much more detail is needed about the methods throughout the paper. For example, Fig. 6 is the central test of the proposed estimation method. The authors state that soil moisture was measured in 22 plots, but the figure only includes 15 points. Where are the other 7 points? Also, the soil moisture was measured over 4 months, but the time interval was not stated. How many observations were collected at each point in the 4 months? Also, what is meant by observed soil water content in the figure? Is it an average? Also, the authors state that soil moisture was measured at 4 depths. What depth are these measurements from? Why are the results only shown for June? Why only one depth (or an average of depths)? These are important details to understand the strength of the analysis.

3. Figure 6 is also troubling due to the grouping of the data. There is a cluster of very low observed soil moisture values and another broader cluster of higher soil moisture values. Within these clusters, it appears that the method is totally unsuccessful at predicting the soil moisture variations. The method is only successful at distinguishing these two groups. That raises concerns for me about its general applicability.

4. Another important limitation is that the method only estimates the soil moisture “status”. In this paper, the authors have only shown that the modified wetness index is linearly correlated with the soil moisture, but it appears that the authors are performing a different regression against observed soil moisture each month in the comparison. In that case, the very definition of soil moisture status and its relationship to soil moisture is implicitly changing each month. In the end, the method does not allow direct prediction of soil moisture—only a soil moisture proxy.

5. I believe the conclusions are too broadly stated. Conclusion 3 does not represent a significant advance. It is a statement of the behavior of the soil moisture status, which is directly implied by its definition when applied to any topography. I would suggest removing it. Conclusion 2 is stated far too generally. The wetness index is probably not universal in the sense that many studies have shown that it does not reliably predict soil moisture patterns (the authors may mean that it is universal in a different sense, but this should be clarified). Precipitation may be important in this particular case, but no general conclusions about its role can be stated based on this analysis. This analysis offers a single data point in that respect. Conclusion 1 says the use of the new wetness index was validated. But this validation is only based on one catchment. The authors should caution that it may not apply beyond the limits of this catchment. They mention the role of vegetation, but what about soil texture, soil depth, latitude (and its effect on solar insolation angles), extent of spatial variability in precipitation, type of precipitation (snow, rain), etc.? Changes in these factors also limit the applicability.

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