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Hydrology and Earth System Sciences Discussions

Interactive comment on "Integration of remote sensing, RUSLE and GIS to model potential soil loss and sediment yield (SY)" *by* H. Kamaludin et al.

Anonymous Referee #1

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General comments

This paper presents an application of the RUSLE to a specific catchment in Peninsular Malaysia. While the paper is broadly well outlined and clearly structured, it is difficult to pinpoint the originality. It is an application of a well-established method (with remote sensing components) of predicting soil erosion to a case study catchment. Little (or no) attempt is made to generalise findings into a broader context, and I have various minor concerns with the method applied (see below). I suggest that this paper would need to be refocused with a more substantial original component before I would encourage

publication in HESS. It may be that there are aspects of the RS implementation that are novel: if this is the case, then they should be highlighted as such. However, I would be rather surprised if this were the case.

The topic under investigation certainly has potential. The possibility that these values might change over time is not really mentioned, although this frames the context of the study in the introduction. Framed within a temporal study of land-use change (perhaps repeating analysis based on historical data to ascertain some form of change related to land-use, specifically forest clearance) then I would consider this much more publishable. Clearly, however, this depends on data availability.

The annual soil loss is classified into categories. What is the justification for these categories? Perhaps a sensitivity analysis would be appropriate? For example, "the SDR for each sub-catchment was observed to be very low to high" (P4579) is rather meaningless.

The broader issue here is that a number of empirical equations, parameters and classifications are used in this analysis, and yet, the results are stated as fact, with no consideration of errors and, as far as I can tell, no validation. This impacts the reliability of the results and should be considered before publication.

Specific comments

There are some minor aspects of the method that require clarification or justification: (1) What is the physical meaning of the m parameter in equ. 2 and Table 2? (2) Is the 'cell value' in equ. 3 the cell size? (3) P4573 L8 suggests that some form of validation has taken place. This is not presented. (4) Parameter S represents soil structure in equation 7. This is not elaborated on. (5) What secondary data was used to evaluate K? (P 4574 L9) (6) A regression between NDVI and C is mentioned (P4575 L3) but nowhere is this presented. (7) In the calculation of sediment yield, should all upstream values of A (or SE) not be integrated and multiplied with the point value of SDR? I am not certain of this, but this seems to make more physical sense to me, given the calculation of SDR. At the very least, it deserves some thought. (8) P4582 L6: this correlation is most spurious, given equation 9!

Technical corrections

I have generally avoided making minor comments on the text, given the larger changes I propose above. However, I have indicated a few specific points below: - P4568 L21: reword first sentence. It is rather vague. - P4569 L14: I would avoid calling RS 'famous'! - Table 2 is referenced before Table 1. - P4573 L15: units of P? - P4573 L20: what is the source of this equation. Again, units missing. - P4578 L23: reference to Fig 5. This Figure is classified and does not show the value suggested. - Figure 4: classification is unclear - Figure 7: Is this the percentage of total soil loss? Is percentage of SDR meaningful? - Figure 8 is unclear. Which is upstream? Which are the specific tributaries? The data need plotting on different scales.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 4567, 2013.