

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
<https://doi.org/10.5194/hess-2021-457-RC2>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.



Comment on hess-2021-457

Pedro Batista (Referee)

Referee comment on "High-resolution erosion susceptibility data for agricultural lands of Finland" by Timo A. Räsänen et al., Hydrol. Earth Syst. Sci. Discuss.,
<https://doi.org/10.5194/hess-2021-457-RC2>, 2021

Dear authors,

I enjoyed reading your manuscript "Improving the agricultural erosion management in Finland through high-resolution data". I appreciated the use of the high-resolution DEM and the field-parcel data for model parameterisation. However, there are some issues, which, in my opinion, need to be addressed before the manuscript can be considered for publication.

First, you state that the goal of your study is to produce 'erosion risk data for agricultural lands in Finland'. However, what you produced are soil loss maps, which do not translate into erosion risk assessments. I understand this is a common misconception in erosion modelling research, but the manuscript should not add to the confusion. For instance, you assume risk is the modelled erosion value for a given location, without stating the assets at risk, what negative consequences erosion could bring to these assets, and what are the probabilities of these consequences occurring.

Second, there are serious problems with methodology used for calibrating the C factor for the RUSLE. From what I understood, your approach considerably deviates from the original USLE or RUSLE methodology, neglects the influence of rainfall erosivity and crop stages, and relies solely on a deterministic parameter optimisation procedure, without considering the uncertainty in the input data and the calibration methodology. These issues are described in detail below, and please correct me if I am wrong.

Third, there is no uncertainty analysis. Although you dedicate a large amount of text to pointing out the uncertainties in the model, you did not attempt to quantify them. In my opinion, if you wish public policy to be guided by your results, you should at least provide

a forward error assessment to quantify the uncertainty associated to the model parameterisation. For instance, you state that your study provides a generalisation of the effects of management practices on erosion. Do you believe it is sound to provide such a generalisation, based on a limited number of observations, without a measure of uncertainty?

As pointed out by Christian Stamm, I also have some concerns regarding how tile drainage and field borders were incorporated (or not) into the modelling.

I believe you can address these issues in a new manuscript, but that would require a different submission, in my opinion. I hope these comments are at all useful and I wish the best of luck with your research.

All the best,

Pedro Batista

Detailed comments

L26: I did not understand what you meant with "the key process causing erosion is hydrological".

L34: Do you mean erosion is affected by the short growing period?

L36-37: I think superscripts are missing here.

L64: In my opinion, acquiring spatial data for parameterisation and calibration is more of a challenge than computational power.

L68: Could you also state some of the limitations of the USLE-family models here? For instance, you cite our paper to corroborate the ability of the USLE to simulate annual loads – I imagine you mean at the erosion plot scale. However, our review also shows how

spatially distributed erosion rates compare poorly to independent measurements.

L81: Wouldn't the RUSLE require longer time series for estimating the R factor?

L82-87: How are you defining risk? Can risk be expressed in mass area⁻¹ time⁻¹? It seems to me you are calculating erosion rates, which of course can be a threat to multiple assets (e.g. the soil itself, downstream infrastructure, etc). However, threats, assets, and potential consequences need to be identified in order to produce an actual risk assessment. This is a common misconception in model-based erosion risk assessments, in my opinion.

L96-97: With a 2 m resolution, couldn't you assess risk at field-block scale?

L112: These are not the sub-factors defined in the RUSLE (see Renard et al. 1997), correct? If so, please make it clearer you are using an adaptation.

L128-130: What is the ICECREAM model? In general I could not understand how the K factor was calculated. Are you taking single K factor values for mapping units in a soil map? This can introduce large errors to model outputs (see Van Rompaey and Govers, 2002).

L134-135: This is an interesting point about the sink filling. Have you made any tests with and without it?

L150: I have some questions about this calibration. Usually, we would calculate soil loss ratios for different crop management systems/crop rotations by use of erosion plot data and/or plant, soil, and residue measurements. These ratios would then be weighted with rainfall erosivity to calculate the C factors for specific locations. However, here you are using an optimisation approach – could you explain why? Moreover, did you perform any kind of split-off test, in which part of the data is used for calibration and another for testing? Would you agree that parameter calibration is necessarily conditional and that different parameter values can produce acceptable model responses? If not, why? If so, shouldn't you use a range of behavioural parameter values to estimate the uncertainty in your model outputs? Moreover, it seems like you calibrated the sub-factor C_{crop} , not the C factor. Did I understand this correctly?

L175-176: I agree model evaluation is difficult. However, particularly when models are being used to influence public policy or to guide decision-making, model testing is a necessary step. Our point in the paper you are citing was not to say it is okay not to evaluate models because it is difficult and rarely done. Instead, we wanted to incentivise

the erosion modelling community to improve how we perform model testing and uncertainty analysis.

L180-187: I agree, and I appreciate how you are open about the limitations of your testing data.

L210-221: Why was rainfall erosivity not considered in any of the C factor calculations? This is crucial in USLE-type models, since erosion rates will vary largely for a same crop if the time in which the soil is exposed coincides with the time in which there is greater rainfall erosivity.

L235-242: Have you considered using a sediment routing model, which would allow you to deal with these issues?

L267: "Reasonable, with limitations" seems subjective. I suggest sticking to the numbers at this point of the results.

L270: The mean error shows you if the model is biased or not, but this metric is affected by cancelation. Could you also provide the RMSE, NSE, or such? Moreover, are you comparing the average soil loss from the entire period or individual annual losses?

L287-290: Would you not expect the (mean?) estimated erosion rates per catchment to deviate from the TSS measurements anyhow? These are two different things. I agree that if there is a correlation between them, there is an indication that the model might be consistently identifying the catchments where there is greater sediment production. However, since the RUSLE does not quantify sediment delivery to water courses and only represents rill and interrill erosion, do you believe there is any chance this correlation does not amount to causation?

L290-298: Do you believe mean values per catchment are good descriptors of your data here?

L313-315: Do you mean highly erodible soils?

Fig.4: By looking at your R factor map, I imagine the spatial variability of rainfall erosivity to have a large influence on the C factors for croplands.

L324-325: There seems to be a concern here regarding connectivity and sediment delivery to water bodies. Do you think the RUSLE is an appropriate model for looking at these issues?

L390-395: If you are using the same data for calibration and testing, considering average annual soil losses, and with a +- 50% limit of acceptability, how rigorous do you believe your model evaluation procedure to be? Moreover, this methodology for incorporating the effects of sub-surface drainage into the C factor needs more explanation, in my opinion. Is (tile?) drainage affecting rill and interrill erosion or the sediment export from agricultural plots?

L400-420: Considering all these uncertainties, do you believe it is acceptable to provide a single, deterministic, model output?

L403-404: Yes, quantifying uncertainty is challenging, but shouldn't we step up to the challenge? We have published a simple, open access, code to propagate the errors in the RUSLE (Batista et al., 2021b). Similar code using high-resolution data is available in Batista et al. (2021a). If these approaches are too computationally intensive considering the scale of the model application, I suggest using the sub-catchments where you have measured TSS data as case studies (see Tetzlaff et al., 2013).

L426-435: With a 2 m resolution, isn't there any other way you could incorporate the influence of the buffer zones in your model?

L436: How do you define reasonable quality?

L436-441: I am sorry, but I disagree that the calibration of the C factor provided a good basis for modelling. The main reasons being:

- The inclusion of sub-surface drainage into the soil loss estimates used for calibration is not well justified;
- The methodology for calculating the C factor is not in accordance with the RUSLE handbook (Renard et al., 1997);
- Rainfall erosivity is not considered in the C factor calculations;
- The parameter optimisation procedure does not consider the uncertainty in the data, and a single parameter value per crop type is used.

References:

Batista, P., Fiener, P., Scheper, S. and Alewell, C.: A conceptual model-based sediment connectivity assessment for patchy agricultural catchments, *Hydrol. Earth Syst. Sci. Discuss.*, (May), 1–32, doi:10.5194/hess-2021-231, 2021a.

Batista, P. V. G., Laceby, J. P., Davies, J., Carvalho, T. S., Tassinari, D., Silva, M. L. N., Curi, N. and Quinton, J. N.: A framework for testing large-scale distributed soil erosion and sediment delivery models – Dealing with uncertainty in models and the observational data, *Environ. Model. Softw.*, 137, doi:10.1016/j.envsoft.2021.104961, 2021b.

Renard, K. ., Foster, G. R., Weesies, G. A., McCool, D. K. and Yoder, D. C.: *Predicting Soil Erosion by Water: A Guide to Conservation Planning With the Revised Universal Soil Loss Equation (RUSLE)*, 1997.

Van Rompaey, A. J. J. and Govers, G.: Data quality and model complexity for regional scale soil erosion prediction, *Int. J. Geogr. Inf. Sci.*, 16(7), 663–680, doi:10.1080/13658810210148561, 2002.

Tetzlaff, B., Friedrich, K., Vorderbrügge, T., Vereecken, H. and Wendland, F.: Distributed modelling of mean annual soil erosion and sediment delivery rates to surface waters, *Catena*, 102, 13–20, doi:10.1016/j.catena.2011.08.001, 2013.