Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-602-RC3, 2020

Hydrology and Earth System Sciences
Discussions

Interactive comment on "A systematic assessment of uncertainties in large scale soil loss estimation from different representations of USLE input factors – A case study for Kenya and Uganda" *by* Christoph Schürz et al.

Anonymous Referee #3

Received and published: 31 March 2020

General comments:

This paper presents an analysis of the variability in soil loss estimates with the USLE equation due to different representations of its factors, and subsequent comparison of the predictions with field data. It is certainly not the first time that the uncertainty of erosion predictions with the USLE is questioned. Yet, the fact that the USLE is very often applied using very different data and methods to determine its input factors still make the study relevant. The study uses a representative selection of frequently used methods to determine the USLE factors based on readily available land use,

climate, soil and topography data. The paper is generally well written, but could be more concise at some points and there are some issues that require better explanation or justification, as explained below.

The authors rightly argue that there is a huge range in erosion rates predicted in function of the methods used to obtain the model input factors. What is interesting however is that the ensemble prediction shows relatively good agreement regarding the predicted erosion severity class. So, although agreement with measured erosion data is poor, in line with earlier studies, you might argue that such ensemble prediction is useful for qualitative description of erosion severity. This can be helpful to prioritize policies.

However, the comparison of predicted soil loss with measured erosion and sediment yield data is most problematic. As the authors also mention at some point, the USLE does not consider sediment deposition and transport so it cannot be compared with sediment yield from gauging stations. On the other hand, the erosion rates provided by De Meyer et al. (2011) based on reconstructing the historic surface level and calculating the lost soil volume from 36 farm compounds are extremely high. I am not sure which method was applied exactly by De Meyer et al and for what time and spatial scale the assessments are made for example. In any case, such high values can occur a certain points, but are probably not realistic for larger areas. So, the question is how useful are these comparisons actually, and do we need them to assess the uncertainty in USLE predictions due to variations in its factors? Model validation is very important, but only useful if the modelled and measured data refer to the same processes and the same scales of assessment.

I find the classification of the predicted soil loss values in four classes, below and above tolerable soil loss rate of 10 t/ha/yr, problematic and it does not add much to the entire discussion of the uncertainty of model predictions. First of all, the tolerable soil loss rate depends on a spatially variable soil production rate, which is unknown for the area. Secondly, the USLE soil loss predictions are gross erosion rates and do not account

for deposition during transport over distances longer than a standardized erosion plot. This makes it highly arguable to look at the USLE predictions in relation to tolerable soil loss rates. You can classify the predictions in erosion severity classes but I recommend to delete reference to tolerable soil loss rates.

What exactly is the aim of the comparison of soil loss estimates at the administrative level? How does this contribute to the research objectives explained in the introduction? While it can be an interesting exercise, and may provide relevant information for local policy makers, it seems the whole section 4.3 does not really contribute to the main objectives of your study.

Please explain and illustrate with quantitative data why you did not include the ASTER DEM for calculation of the LS factor. Previous studies have also highlighted that at higher resolutions problems can occur with LS calculations, but since you first projected the ASTER DEM on the 90 SRTM grid could be expected to be less problematic. It would be interesting to see what is exactly the cause of this problem and compare this to other studies that assess the differences in ASTER and SRTM DEMs and their application in erosion studies.

The methods used to assess the C factor rest strongly on the approach used by Panagos et al (2015) and Borrelli et al (2017), but I find the description quite difficult to follow. It is not clear why and how exactly you overlay the already spatially distributed 'crop shares statistics' with the land cover maps? Moreover, it seems the approach puts a lot of detail in differentiating between different crops, but disregards the possible importance of intra-annual differences in C factors due to crop rotations.

There are several other papers that also discussed the impacts of USLE factors and structure on outcomes (e.g. Sonneveld and Nearing, 2003) that would be interesting to include in your discussion.

Detailed comments (indicated per Page and Line):

P2-L17-18: can you add a line how the revised version was different?

P3-L8-11: you may add here a few words on the often used Sediment Delivery Ratio in combination with gross erosion to obtain sediment yield predictions, correcting for the fact that the USLE does not predict sediment deposition.

P3-L14: remove 'the'

P3-L20-23: please check and preferably simplify this sentence.

P3-L33-35: It is indeed not simple to do this kind of comparisons and most plot data do not cover 20 years, but there are by now relatively good and large datasets of measured soil loss available, such as for example the data presented by Garcia Ruiz et al (2015) and Maetens et al. (2012) for Europe. For many other parts of the world this is still more difficult though.

P4-L10: Research objectives are now formulated as research questions; better write them as objectives. In the last objective correct 'we we'.

P4-L16-24: These lines do not seem necessary, and seem repetitive.

P5-L4: on the steepest slopes (>20) gully erosion can be expected to be an issue as well.

P7-L6: what about seasonality of rainfall?

P8-L8 and supplementary Table S1: It seems you only used relationships based on mean annual precipitation to estimate the R factor (not accounting for seasonality). It would have been interesting to include an equation based on the monthly data, for example those based on the Modified Fournier Index proposed by Renard & Freimund (1994) that you cite. The text above Table S1 states 'The ïňĄrst four methods' which should be the 'first five'.

Supplementary page 8 (above table S6) correct 'To compute the K factor realizations..' for 'To compute the LS factor realizations'.

P9L30: please correct sentence ‘served as base layers for the join with..’

Table S7: what does the first column ‘value’ mean?

P12-L16: this may be interesting, but where exactly do we find the results of this? I couldn’t find it in the results section.

P12-L19: it is not clear from this paragraph how the comparison of soil loss rates at the administrative level contributes to the papers objectives expressed in the introduction.

P14-L29: With ‘the dominant soil loss levels that a majority of model setups predicted’ you refer to the soil loss level for which most agreement was between the model set-ups? What if there was no majority for any of the soil loss levels? Unfortunately, in the figure 5, the lightness of the colours that should indicate the percentage of models that calculated a soil loss within the respective soil loss classes, cannot be distinguished.

Figure 7: in the heading it states that the values refer to those pixels for which ‘high to severe soil loss was predicted to be likely’. How is ‘to be likely’ defined here? Or does this refer to high or severe soil loss as predicted per model implementation?

P20-L20: why do you highlight and compare with the data from Karamage et a (2017)? Did you introduce this in methods? I don’t see the added value, especially considering that the data are already covered within your model implementations, so it seems there is nothing new.

P23-L18: But you did not really perform a plausibility check of the individual USLE model realisations, so the argument does not make too much sense.

P24-L8: the comparison with one particular study does not contribute anything to this interpretation; the wide variety between your results indicates that you cannot take conclusions based on only 1 model implementation and that an ensemble approach makes more sense.

P24-L15-18: This sentence misses a conclusive statement. Indeed, the tolerable soil

loss is controversial and does not seem to add much to your assessment.

P24-L26-27: please correct this sentence.

P24-L28-20. The detail in the patterns is not a property of the factor, or how important the factor is, but it just reflects the level of spatial variability that is present in the input data used. This does not mean anything for the relevance of one factor as compared to another or a scale influence. The interesting part of your result is the overall impact of each factor on total ensemble variability.

P25-L11: at larger spatial scales you will need to include not only different sources of sediment (rill, gullies, mass movements), but also deposition during transport, as explained in detail by numerous previous studies.

P25-L20: If the data are in stream sediment loads they are certainly do not ‘better meet the spatial scale of USLE’.

P25-L26-28: Various studies have dealt in detail with the role of spatial scale in erosion assessments, and how plot scale data compare to sediment yield (e.g. de Vente et al, 2007). Further, the difference between the plot data and USLE model predictions do not have anything to do with comparing plot data with landscape scale sediment yield. Plot data and the USLE assessments in theory both consider the same erosion and deposition processes at the same scale.

P25-L31-33: I think quantitative validation via google earth will be difficult and you do not really explain how this could be done.

P26-L9: ULSE = USLE

P26-L12-14: computer capacity for these kind of calculations should nowadays for most studies not be a problem anymore.

P26-L14: Ideally yes, like in any model you need to validate the predictions. But, how do you determine the plausibility if you don’t have field data to compare with?

Based on your assessment and comparison with field data would you say that the USLE assessments are plausible? You need data that can be compared with the USLE predictions, so representative for the same scale. I think the main interest is in the fact that the ensemble prediction shows relatively good agreement in the severity class of erosion, but quantitative validations are problematic.

P27-L4: please rephrase and simplify the sentence.

P27-L11: increased soil loss = high soil loss

P27-L26-28: Most important here is to make sure that the data are comparable, so representing the same erosion and sediment transport or deposition processes. In theory, USLE predictions should compare with plot data.

P27-L28: this recommendation is very vague. What kind of new approaches? How would google maps provide quantitative estimates that can be compared with model predictions?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-602, 2019.