Reply to the reviewer comments RC1: 'Hess – USLE model Uganda' by Anonymous

Referee #1

We would like to thank the Anonymous Referee #1 for the very constructive review and the valuable comments to improve the quality of the manuscript. We appreciate the positive feedback on the manuscript. In the following, we addressed each comment individually. The reviewer comments are printed in *serif, italic font*. Our replies to the individual comments are written below each comment in black non serif font. The literature that was cited in the reply is added at the end of the document.

An interesting article proposing interesting aspects in USLE modelling: a) uncertainties b) comparison of factors c) validation. However, there are issues that authors should face in order to improve the quality and proposing it for publication.

An important issue is that authors did not propose 'solutions'. They did the 756 USLE simulations but they should also propose which is the most representative one per factor. For example , which is the best method for the R-factor?

All realizations that were developed for each USLE input are based on methodologies that were proposed and implemented in peer reviewed studies. In the summary of methods for the calculation of (R)USLE inputs that was compiled by Benavidez et al. (2018) most of the methods that were implemented in the present manuscript were listed as 'valid' methods to compute the respective USLE input. The basic rule for the input generation in this manuscript, was that if a method was implemented in a peer reviewed study in Eastern Africa (or regions with comparable climatic/topographic/vegetation conditions) before, it is considered as a plausible method for the generation of that input. From this perspective , all input realizations must be treated as equally adequate representations of that input.

The aim of this study was to assess the uncertainties which are inherent in the calculation of the long-term mean annual soil loss simply due to the choice of the methods for the calculation of the USLE inputs. It has never been the intention of this study to identify a 'best' realization for a USLE input. Any attempt to identify plausible or implausible USLE model combinations would have failed, as no measured (or other) data was available that could support a decision to verify or falsify a model combination. This was addressed in detail in the Sections 4.4 and 5.2 that highlighted the limitations of a comparison to the limited observation data that is available for the investigated study region. We suggest to revise these sections and try to strengthen the stated arguments to clarify the present limitations of model falsification.

Authors propose new approaches to check the plausibility of large scale assessments based on Bosco et al., 2014. This study has neither been peer reviewed nor published. So, I would suggest using published literature studies for such statements.

Bosco et al. (2015) briefly outline the methodology that is explained in much greater detail in Bosco et al. (2014). While Bosco et al. (2015) is a peer reviewed article, we agree that Bosco et al. (2014) is published as a pre-print version that was not peer-reviewed. In the present manuscript we always refer to both articles when we mention the applicability of remote sensing data for a plausibility check of the USLE simulations. Although Bosco et al. (2014) is not peer reviewed it provides valuable information that can be accessed via a DOI and therefore the same document is available to the reader of this study to which we refered to when compiling this work.

In a similar study, Estrada-Carmona et al (2017) made a global sensitivity analysis of USLE input factors. Please compare the results of your study with the ones of this study.

Thank you for drawing our attention to this study. Although the approach presented in Estrada-Carmona et al (2017) differs from the approach that was presented in the present manuscript we should and will refer to this study.

The most recent study that I found in East Africa is the one of Fenta et al., 2019 Science of the Total Env. How your results compare with their results?

The study of Fenta et al. (2020) was not published by the time this manuscript was compiled. The workflow that is presented in Fenta et al. (2020) was adopted from Borrelli et al. (2017), Panagos et al. (2015), and Panagos et al. (2014) for the computation of the USLE K and C factors. To calculate the LS factor the method of Desmet and Govers (1996) was implemented. These three realizations for the inputs K, LS, and C are also members of the input sets in this study and should therefore result in comparable ranges. Interesting aspects in Fenta et al. (2020) are the computation of R and more importantly the consideration of soil protection measures represented by the P factor. Thus, a comparison of the calculations presented in Fenta et al. (2020) with the calculated ranges of soil loss in this study is valuable. Thus, we suggest to refer to Fenta et al. (2020) in the revised version of the manuscript.

Soil losses estimates with USLE are long-term averages. You cannot compare the long-term findings against short-term findings in plot experiments.

We agree that a comparison of long-term soil loss to short term measurements of e.g. sediment yields is improper. In the manuscript we specifically indicate the limitations of the comparability of USLE simulations to an instream monitoring of sediment yield. Yet, it is a common practice to employ short term records of observed soil loss (or sediment yield) to validate the results of a USLE model. See e.g. Fenta et al. (2020) where a comparison of USLE results to ‘measured’ short term sediment yields was performed.

Thus, we see a relevance to critically address the issue of a USLE model comparison to observation data. Eventually, the analysis of the USLE model ensemble and its comparison to the soil loss data collected by García-Ruiz et al. (2015) clearly supports a critical view on such model validation and is therefore in our opinion a relevant contribution to the soil erosion literature.

Authors made a classification of soil erosion rates. The tolerable soil erosion rate cannot be justified according to literature findings as the soil formation rates are low. This means that sustainable soil erosion rates are lower than 1-2 t per ha per year. In addition to this, authors present some really extreme mean annual soil loss rates >200 -1,500 t ha-1 yr-1. This means that at least 2 cm of soil is lost every year. This may be the case for very limited areas; otherwise we risk to lose completely our soils in50 years. This means that some of the estimated combinations are not realistic. You should not be driven by the modelling outputs but somehow use also the common logic(you cannot lose 1m of soil in 50 years).

We have refrained from making judgments about tolerable soil erosion rates, but we do point out that such values exist in the literature. The concept of this manuscript was to employ the information that we acquired from the peer reviewed literature to represent the current status of knowledge on the topic of "soil erosion by water" and the assessment of water erosion with USLE type models. This idea also is applied to the selection of soil loss classes that employ what is common practice in the literature. At no point in the manuscript do we imply that a soil loss of 10 t ha^{-1} yr^{-1} is indeed the value where the soil loss is compensated by the soil formation. We stated that suggested literature values range from 5 to 12 t ha^{-1} yr^{-1} and that several studies in Eastern Africa used 10 t ha^{-1} yr^{-1} as a threshold value. Due to the absence of more reliable values, we based the classification on literature values that included threshold values of 10 t ha^{-1} yr^{-1}. Yet, we agree that threshold values of 1 or 2 t ha^{-1} yr^{-1} would be valid assumptions as well. Fenta et al. (2020), for example, classify soil loss by water as 'very slight' soil loss when the soil loss is in a range between 0-2 t ha^{-1} yr^{-1} and as 'slight' when the soil loss is in a range between 2-10 t ha^{-1} yr^{-1}. Yet, Fenta at al. (2020) do not provide any reference on which their classification is based. In conclusion the classification of soil loss always involves a highly subjective view on the calculated soil losses. A classification should primarily reduce information and support the reader in the interpretation of data and we are aware that poorly chosen class names can mislead the reader. Nevertheless, we decided to use a classification that was implemented in the literature before and should be considered to be as valid as any other classification.

We agree with the reviewer that calculated soil losses of over 200 t ha^{-1} $yr^{-1}$are extreme. Nevertheless, for the informative value of this manuscript it is relevant to keep these model combinations as potential USLE realizations. Two arguments for the value of these model members are as follows:

Indeed such high values were calculated only locally and only by a few model combinations. This is exactly what this manuscript tries to address. Models can be wrong. Thus, if other representations of the USLE estimate soil losses that are substantially lower then the modeler has a chance to evaluate such large soil loss estimates based on other estimates. Yet, with a single model approach (that is common practice in the literature) it is simply infeasible to evaluate large calculated soil losses if no reference by observation data or a model ensemble is given. Thus, if a model setup calculated excessive soil losses locally, what does this mean for the evaluation of the remaining areas in a study region?

USLE type models do not account for soil deposition and therefore do not reflect the sediment balance. Thus, the soil that is strongly eroded locally is also deposited and not completely lost but displaced.

Title: I would replace the word ‘representations’ with ‘applications’

We think that ‘applications’ is not the appropriate term for the entities that we analyzed in this work. ‘Applications’ could also imply an application of USLE inputs in for instance different independent studies, or locations and would thus be misleading. In this manuscript we represented the USLE input factors R, K, LS, and C for the same study by employing different methods to compute them and would therefore prefer to use the term ‘representation’ or ‘realization’ when we refer to one member in the set of realizations for a USLE input.

P4 L15-24: This paragraph is not needed.

We agree that this paragraph does not provide any new information, but outlines the structure of the manuscript. We think that this is a subjective question of style and preference and believe that it helps the reader to get an overview of the content of the paper at hand.

Fig. 3: attention in the measurement unit of soil erosion. It is better to use t ha-1 a-1. If you want to keep your proposed unit , then please put in parenthesis (ha a)

Thank you for pointing this out! We will revise the units in all figures to be consistent with the units in the text and tables.

Fig 7. It should be applications and not realizations.

See the response above. The same argument applies as above for the title of the manuscript.

P23 end of the page and P24 beginning of the page: I would propose that some applications of the factors can be excluded. For example, the NDVI application is known to have very low C-factor results and it is known to have incorporated some problems. The same applies for R-factor. For example the methods of Lo and Fournier are based on rainfall amount and do not incorporate the rainfall intensity.

As responded previously, the goal of this work was to provide a comprehensive assessment of frequently implemented methods to calculate the USLE input factors and to evaluate the uncertainties in the soil loss estimates that arise from the input uncertainties due to the impact of different methods, published in peer-reviewed journals, to calculate the model inputs. Thus, it was not our intention to judge the implemented methods, but to consider them as potential methods if they have been implemented in similar study settings. This also applies to the methods used to calculate the R factor and the C factor.

Both methods addressed by the Anonymous referee #1 (the implementation of the NDVI and the method of Lo et al. (1985)) were recently implemented in a large scale soil loss assessment by Karamage et al. (2017). In general, the implementation of methods that use long-term precipitation instead of rainfall intensity is common due to the absence of rainfall intensity records. Thus, in terms of a comprehensive uncertainty assessment we must consider these types of methods for the calculation of C and R as well.

Concerning the limitations of any implementation of the NDVI to calculate C, we were not able to find information in the literature that NDVI is known for a calculation of low C-factor values. Yet, the analysis that we illustrated in Fig. 7 of the manuscript would support that statement for the selected region in Uganda. Also, any well documented issues with the application of the NDVI to calculate the C-factor is not known to us (or is at least not reported in the literature at hand). However, we specifically address in the manuscript that the method of Van der Knijff et al. (2000) was never validated against ground truth data.

Concerning the C-factor value ranges calculated with the method of Van der Knijff (2000) we want to take the analysis in Fig.7 of the manuscript as an example. Although the mean values and the quantile values for the C-factor with the NDVI are significantly lower to the other methods, the absolute ranges of C-factor values when employing the NDVI are in a plausible range. For Europe Panagos et al. (2015) calculated C-factor values as low as 0.00116 for forests and C-factor values of up to 0.2651 for sparse vegetation. The ranges presented in this manuscript are in line with the range presented in Panagos et al. (2015).

We see however that a comparison to other large scale studies would provide valuable information and will consider that in the revised version of the manuscript.

P19 the same as above. Are values of K-factor 0.088 acceptable? Can be compared to other findings in the literature?

Panagos et al. (2014) for instance calculated a range for the K-factor of 0.004 – 0.076 t h MJ^{-1} mm^{-1}. Naipal et al. (2015) implemented the method of Torri et al. (1997) that resulted in such large K-factor values in this manuscript. Naipal et al. (2015) also applied a K-factor value of 0.08 to vulcanic soils as these are particularly erodible. Although single grid cells in the analysis in this manuscript exceed the mentioned literature values, the calculated values are not completely out of range.
Yet, we fully agree that it is relevant to set the calculated values into a reference with other literature. We suggest to add a comparison to the above mentioned litarture values and additional literature in the revised version of the manuscript.

P20 L1-10: The same as above. Can you prove that values of C-factor 0.03 are acceptable in agricultural areas?

Thank you for raising this point. The same arguments as in our replies to the previous two points apply here.

5.2 section. It is not proper to have a section with question.

We cannot verify this point, as the manuscript guidelines do not disallow questions as section headers., in essence, freedom of the authors. See Blöschl and Montanari (2010), Savenije (2009), or Schürz et al. (2019) as examples with questions as section headers.

P25 L30-34: Is It possible to validate large scale models with Google Earth? GoogleEarth can potentially verify permanent erosion characteristics (e.g. gully erosion) and not rill and sheet erosion. The plausibility of large scale studies can be verified with model applications at regional or local

We cannot confirm the feasibility of the implementation of satellite imagery for the validation of soil loss assessments. It was not our intention to illustrate such an approach as a verified method for the validation of large scale soil loss assessments. From a technical perspective it should however be possible to employ multi-angle high resolution imagery for different time steps to apply any stereographic analysis. Furthermore, intense soil erosion might be visible in satellite imagery and can be related to an erosion class, which would allow a use as 'soft' information for model validation. Regardless of the exact approach to employ satellite imagery, the conclusion in the presented manuscript was that our traditional approaches of model validation failed in this large scale study (and would likely fail in others) and new concepts for model validation would be valuable to be implemented in large scale assessments. The validation of a large scale model application with a model application at a smaller scale does not sound plausible unless the small scale model is validated, as any soil erosion model involves uncertainties.

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