Reply to the reviewer comments RC3: 'referee comments', by Anonymous Referee #3

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.

We would like to thank the Anonymous Referee #3 for the highly detailed review of our study and the constructive 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 comments are written in black, non serif font. The cited literature is added at the end of the document.

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.

We appreciate the positive comment. Yet, we think that this perspective on the agreement of the ensemble with respect on soil loss levels is a little too optimistic. Fig. 4 (and particularly panel a)) shows a strong agreement of the model ensemble for soil losses below a threshold of 10 t ha^{-1} yr^{-1} that was defined as “tolerable” soil loss in this study. Thus, the model ensemble is able to identify regions with no or a low erosion risk. In potentially erosion prone areas, however, a large spread is visible in the ensemble prediction of the soil loss classes. This is particularly visible for large parts of the Rift valley and the South-West of Uganda where large ranges from “tolerable” to “high” are visible.
Fig. 5 was intended to support the reader to assess the confidence of the model ensemble to predict a soil loss level. From that perspective we think, however, that the picture that is conveyed by Fig. 5 can be too optimistic and gives the impression of a strong agreement for the areas that were analyzed in detail (see panes c) to d)). Thus, we suggest to revise Fig. 5 to improve the readability of the probability of models in a class in the figure.
Despite the described limitations we think that the argument that the Anonymous referee #3 stresses is highly valuable. Section 5.1 briefly discusses the relevance of the model ensemble to provide information on the confidence to predict the severity of the soil erosion

risk. We suggest, however, to strengthen this argument and add a short paragraph on the potential of a model ensemble to support policy making in Section 5.1.

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 cal-culating 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 at 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.

We fully agree that a comparison of soil loss calculations with USLE type models to in-field data is problematic. We think, however, that it is relevant to illustrate potential issues that arise from a comparison of soil loss calculations to in-field data that may stem from different types of erosion monitorings, simply because it is frequently performed in erosion studies and we think that such common practice must be critically evaluated. As this point was addressed by other referees in a similar way we think that it is relevant to better specify the intention of the model validation as it was performed in this study. We therefore suggest to revise section 3.7 of the manuscript accordingly.

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.

Thank you for this advice. We agree that the selected terminology is highly critical, particularly due to the arguments that were stated by the Anonymous referee #3. The term “tolerable” seems to be too specific and implies that process relationships are known that are indeed unknown. We therefore suggest to add a paragraph in section 3.4 to indicate that the actual soil formation rate is unknown, but that we use this threshold as it is frequently used in other literature. We additionally suggest that we change the term “tolerable” to e.g. “slight” in the revised version of the manuscript and indicate that we refrained from using the term “tolerable” due to the above stated issues.

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.

We agree that the analysis of the mean soil loss on an administrative level is not specifically outlined in the introduction. Yet, often erosion studies perform the analysis of the soil loss (or any related measures) for defined administrative units (see e.g. Gourevitch et al. (2016), or Karamage et al. (2017) for districts in Uganda, or Panagos et al. (2015b) for provinces at the NUTS3 level in Europe). In theory one could assume that local disagreements of the model combinations will be reduced by a spatial averaging and as a consequence the input uncertainties are less critical for a soil loss assessment on the administrative level. Yet, the uncertainty analysis on the administrative level shows that large uncertainties are present in the aggregated soil loss estimates. Thus, any decision making based on a single USLE model estimate is highly questionable. The comparison with the results of Karamage et al. (2017) for Ugandan districts clearly illustrates this issue. Therefore, we conclude that the results presented on the administrative level provide essential insights in the evaluation of uncertainties in soil loss estimation. We agree, however, that our intention with the soil loss assessment on the administrative level was not specified well enough. Therefore, we suggest to add a brief section to outline the intention sketched above.

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.

We agree that the found issues with ASTER DEM to calculate the LS factor could be elaborated with more detail. Due to the length of the present manuscript we, however, tried to keep this section concise. We suggest to add a section that illustrates the artefacts in the calculated LS factor realizations using ASTER DEM in the supplementary materials.

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.

The method to calculate the C factor that was proposed by Panagos et al (2015a) was adopted in several preceding studies (see e.g. Fenta et al. (2020) Batista et al. (2017), Borrelli et al. (2017), Lugato et al. (2016)). The main concept in this manuscript was to employ frequently used methods to compute the USLE input factors. Therefore, it was also relevant to consider the method of Panagos et al. (2015a) as a member in the set of C factor realizations and to follow the C factor calculation as it is described in Panagos et al. (2015).

Further, the options for the computation of the C factor are often limited by a lack of available crop data. (Nationwide) Agricultural statistics are usually not available for every year. As a consequence, data to consider inter-annual variations in crop statistics are usually not available. A typical assumption that is drawn in the calculation of the C factor based on crop statistics is that the available statistical data is a good average value for the entire analyzed time period.

Statistical agricultural data is not spatially distributed but provided aggregated on an administrative level (see e.g. the National Agricultural census data that was implemented in this manuscript). Also the agricultural data that is available from Monfreda et al. is not fully spatially distributed but aggregated with a spatial resolution of 5 minutes.

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.

We were not able to identify studies in our literature review that employed similar analyses as the one presented in this manuscript. Thank you for pointing out the study of Sonneveld and Nearing (2003) which we will mention in this manuscript. We suggest to include further relevant literature in the discussion.

<u>Detailed comments (indicated per Page and Line):</u>

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

We will add the following to the sentence on P2 L16-17:

Further data were collected over the following decades and the methods to calculate the USLE input factors were substantially revised. This resulted in an update of the iso-erodent maps, the consideration of seasonality and rock fragments in the K factor, or a consideration of additional sub factors for the computation of the C factor.

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.

We will add a short section that acknowledges approaches that also account for deposition processes and employ the Sediment Delivery Ratio (e.g. Rajbanshi et al. (2020), De Rosa, et al. (2016), or Sharp et al. (2015)).

P3-L14: remove 'the'

Will be removed accordingly

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

We will revise the sentence on P2 L16-17 as follows:

The implemented remote sensing data products describe (or are a proxy for) features in the landscape (e.g. a DEM represent the topography and the NDVI is often employed to describe vegetation density). In large scale assessments methods are implemented that employ these large scale data products to infer spatially distributed estimates for the USLE inputs.

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 stillmore difficult though.

We agree. Observation data that was collected by García-Ruiz et al. (2015) in their comprehensive meta-analysis was implemented in the present manuscript to compare the soil loss estimates with. Nevertheless, this is one argument that we wanted to stress with this study, that although such data exists a comparison is not always feasible.

We suggest to additionally mention these data sets on P4 L1 in the following:

Large scale meta-analysis studies of soil erosion plot data and sediment yield records exist, such as García-Ruiz et al. (2015) globally, Vanmaercke et al. (2014) for Africa, or Meatens et al. (2012) for Europe.

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

We think that this is a question of style and preference and would prefer to keep the research questions. We will remove the second 'we'.

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

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.

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

We cannot identify where this statement applies in the text.

P7-L6: what about seasonality of rainfall?

As discussed in another reply to the comments by the Anonymous referee #2 we argued that the measures such as the Modified Fournier Index (MFI, Arnoldus, 1980) can provide valuable information to characterize the seasonality of the rainfall erosivity. Therefore, we suggest to calculate the MFI for the study region and analyze the spatial pattern. If the shown patterns strongly differ from the patterns of the shown long-term annual precipitation we suggest to add a panel to additionally show the MFI in Fig.1.

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 ï˝n ˛Arst four methods' which should be the 'first five'.

The simple reason why primarily long-term annual precipitation was implemented to calculate the rainfall erosivity factor, was that the literature on large scale soil loss assessments as well implemented primarily long-term annual precipitation products to calculate the rainfall erosivity. We agree that a comparison of long-term annual precipitation to the MFI can provide valuable insight. We suggest to apply the MFI (Arnoldus, 1980) for the study region and compare the results with long-term annual precipitation to put it into reference. We suggest to add any analysis in the supplementary materials.

We will change 'first four' to 'first five' in the text above Table S1.

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

Thank you. This will be changed accordingly.

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

P9 L29 - L31 will be modified as follows:

Two land cover products, the MODIS Collection 5 LC with a spatial resolution of 250m (Channan et al., 2014; Friedl et al., 2010) and the ESA CCI LC Map v2.0.7 with a spatial resolution of 300m (ESA, 2017) served as base land cover layers. The agricultural, forest, and naturally vegetated land cover in these maps were superimposed with C factor literature values land. The C factor values for agricultural land uses were calculated based on agricultural statistics.

Table S7: what does the first column 'value' mean?

The value represents the crop group ID which is the same as in the following table. Therefore, the column name "value" will be changed to "Group ID" in the tables S.7 and S.8

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.

The calculated values for the sensitivity index for all four USLE inputs was used to rank the inputs. Therefore, the calculated sensitivities values are not shown. Due to the length of the manuscript we decided to only keep Fig. 6 that shows the most dominant input in each grid cell. We suggest to add a figure that shows the sensitivity indices for all four USLE inputs in the supplementary materials.

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.

Please see our reply to the comment on the analysis of soil loss on the administrative level above.

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 setups? 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.

Numerically there should be a dominant input when we apply Eq. 4 to calculate the sensitivities. The only exception is when several inputs are 0 and therefore both inputs have a sensitivity of 1. This should however not be the case as no input is exactly 0 in any grid

cell. Nevertheless, we agree that Fig. 6 does not show if two inputs are almost equally relevant. Therefore, we suggest to add a figure that shows the individual sensitivity indices in the supplementary document as suggested above.
Further, we try to improve the readability of Fig. 5 to better distinguish between the percentages of agreement.

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?

This sentence in the figure caption will be changed to:
The cases A) to D) include the values of input factor realizations for grid cells, in which the respective input factor was the most sensitive one and the majority of models of the model ensemble predicted high to severe soil loss.

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.

Karamage et al. (2017) performed a soil loss assessment for the districts of Uganda. Therefore, a comparison of the model ensemble that we have calculated to their results can be easily performed. The comparison to the results of Karamage et al. (2017) is relevant, because it illustrates the central issue with soil erosion studies that we want to address in this manuscript. Karamage et al. (2017) implemented a single USLE model setup. The comparison with the model ensemble highlights that the USLE input combination that was implemented in Karamage et al. (2017) results in soil loss estimates that are in some cases even lower than the interquartile range that is provided by the USLE model ensemble. When single USLE model setups are implemented in erosion studies this information is simply not available.

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.

We admit that the individual generated input realizations were not analyzed for plausibility. All of the implemented methods were however implemented in previous peer reviewed studies that had similar study settings. Therefore, we considered all or the implemented methods as potential methods to compute the USLE input factors.
We agree that this statement implies that we thoroughly checked all input realizations for plausibility. Thus, we suggest to remove this statement.

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.

We provide the comparison to the study of Karamage et al. (2017) as an example here that describes, however, a general problem. In principle P24 L8-L10 supports the statement outlined by the Anonymous referee #3.

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.

We will add the following statement:

The terms that represent certain ranges of soil loss, such as “tolerable”, or “moderate” must therefore be interpreted carefully.

P24-L26-27: please correct this sentence.

The sentence will be revised accordingly:

Fig. 6 illustrated the most dominant USLE input factor realizations with respect to their impact on the uncertainties of the calculated soil loss. The dominant input factors revealed spatial patterns on different spatial scales.

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.

We do not fully agree with this statement. Yes, the patterns which are shown in Fig. 6 are not a property of the input factors. The patterns are however a result of the variability of the input factor realizations in each grid cell. In each grid cell the analysis that is illustrated in Fig.6 assesses the uncertainty range in the calculated soil loss that is caused by each set of input realizations for the four analyzed USLE inputs and shows the input with the largest impact. If neighboring pixels form a spatial pattern then we can assume that the employed methods to compute the respective input strongly disagree. This insight can help to understand what the dominant source for the simulation uncertainties are locally. We fully agree with the statement that the shown patterns follow the patterns that are given in the used input data that are combined with the methods to calculate the USLE input factors. But that is exactly what we want to analyze with this figure.

We will revise the sentence to specify the statement accordingly:

The patterns of the most dominant inputs follow the patterns of the input data that were employed to calculate the input factor realizations. Thus, the shown patterns can support in identifying the input data/method combination that introduced the largest share of uncertainties in the calculation of soil loss locally.

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.

We will add the following statement:

At larger scales, processes other than the ones that are assessed by the USLE, such as deposition processes, gully erosion, or bank collapses have to be considered in the quantification of the soil loss (Govers, 2011).

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

We agree. The statement will be revised accordingly:

Other reference studies, such as Sutherland and Bryan (1990) or Kithiia (1997) represent the average soil loss at the catchment scale. One could assume that the spatial scale of such studies better agrees with the spatial scale of a large scale soil loss assessment with the 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.

Thank you for addressing this article. We suggest to include the findings of this work (and others e.g. Sidle et al. (2017)) in the discussion on P25 as it contributes to provide a more differentiated view on the comparison of in-field data to our model ensemble calculations.

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

It was not our intention to provide new approaches for a USLE model evaluation in this manuscript. The key message was that model evaluation as it is frequently done is strongly limited due to the arguments that we have stated in the discussion. Yet, we wanted to indicate that we have to think of other potential options to evaluate soil loss estimates and simply provided the method described in Bosco et al. (2014) as an example.

P26-L9: ULSE = USLE

This will be changed accordingly.

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

Our analyses were indeed limited by the computational resources that were available at our Institute, at least when time resources are taken into account as well. Adding additional input factor realizations would not have been feasible with our available resources, as the required storage capacities would easily have exceeded 50+ TB.

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.

We agree that the statement on P26 L14 misses a clear argument on how to check the plausibility. We think that it overall does not contribute much to the paragraph. Thus, we suggest to delete this sentence.

P27-L4: please rephrase and simplify the sentence.

Will be rephrased as follows:

We generated sets of realizations for each USLE input factor and combined them to 756 USLE model setups to compute spatially distributed soil loss estimates for Kenya and Uganda. Based on the generated USLE model combinations we analyzed and quantified the impacts of frequently used methods to calculate USLE inputs on the uncertainties in the soil loss estimation with the USLE model.

P27-L11: increased soil loss = high soil loss

This will be changed accordingly.

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.

We agree that in theory this statement is true. Yet, it assumes that the employed USLE model was properly parameterized and that the used in-field data stems from a long-term plot experiment. Both assumptions might not hold in the presented study setting.

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?

We agree that the wording is too vague. We suggest to rephrase to:

We further question the aptitude of soil loss assessments based on in-stream sediment yields or small scale plot experiments to be valid data for the evaluation of soil loss estimates. We should think of new approaches to validate soil loss estimates that employ large scale data that is now available. Bosco et al. (2014) outline a method to employ satellite imagery to check the plausibility of large scale soil loss assessments.

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