Reply to the reviewer comments RC2: 'Referee comments and suggestions' by Anonymous Referee #2

General comments

The paper presents a large scale assessment of the uncertainties in USLE soil loss estimation as a consequence of different realizations and combinations of the corresponding input factors. A total of 756 USLE model setups were examined with a spatial detail of 90 meters (cell size). Moreover, the case study (Kenya and Uganda) is vast enough to include a great variability of topographical, climatic and land use conditions. For these reasons, the ranges of both input factors and soil loss are very wide, contributing to improve the scientific reliability and interest of the work. The spatial variability of the model sensitivity to the different factors was examined and discussed. An attempt to compare/validate the simulated soil loss with field soil loss data was also made. All the sections of the paper are very clear and the scientific background is well detailed and discussed. The degree of agreement between the estimates obtained by the different input ensembles was evaluated not only on the basis of the quantitative values, but also and above all on the basis of the soil loss category (tolerable,moderate, high and severe). This is in fact the most rational approach for a model characterized by high uncertainty.

We would like to thank the Anonymous Referee #2 for their positive and supportive feedback on this manuscript, the very constructive review and the valuable comments to improve the quality of the manuscript. In the following, we addressed all the comments made by the Anonymous Referee #2. 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.

Specific comments

Lines 3-6 pag. 3. I suggest to mention other recent promising modifications of the USLE, such as those proposed and tested by Bagarello et al. (2010) and Di Stefano et al. (2019): - Bagarello, V., Ferro, V., Giordano, G. 2010. Testing alternative erosivity indices to predict event soil loss from bare plots in Southern Italy, Hydrological Processes 24(6) , 789-797. - Di Stefano, C., Pampalone, V., Todisco, F., Vergni, L., Ferro,V. 2019. Testing the Universal Soil Loss Equation-MB equation in plots in Central and South Italy, Hydrological Processes 33(18), 2422-2433

Both suggested references describe the modification and improvement of the event based USLE-M model (Kinnell, 2010). In the manuscript the topic of event based soil loss assessment with USLE type models was not addressed, but we focused on long-term annual soil loss assessment to limit the breadth of the manuscript content. To write an additional paragraph and acknowledge all (or many) USLE derivatives would be out of the scope of this study.

Figure 1. I suggest to check the legend of the figure 1a, in which the erosion risk is represented according to a discrete classification based on only three colours (white,yellow and pink). However, from the figure, the colour grey is also widely present and gradients for both yellow and pink are evident. I think that a discrete classification/legend is not correct.

We understand that the maps together with the provided legends can be confusing to the reader. Therefore, we suggest to revise the legends and the figure caption of Fig.1 the following:

- The legend symbol for 'Very gentle inclinations…' will be changed to add a slash in the box, thus indicating that no color was applied for that class.
- Will will add the information that the hillshade is plotted in grey as a background layer.

The discrete erosion risk classes were taken from Ebisemiju (1988) and provide a reasonable first differentiation of erosion risk in the study area. We tested a continuous color ramp to visualize the soil risk while compiling the manuscript. The information was however not presented well in such a visualization. Therefore, we would prefer to remain with only three soil risk classes.

Figure 1. I understand that the purpose of Figure 1 is just to provide a rough description of the erosion-prone areas according to topography, vegetation cover and rainfall amounts. In relation to this last aspect, however, the authors could have chosen a proxy more appropriate than the annual precipitation: in fact it is well known that the distribution of rains has a determining role in soil loss. In particular, several studies in the literature have shown that in some areas, the annual soil loss is highly correlated with the erosivity of a few erosive events. Therefore, other synthetic indices (e.g. the Modified Fournier Index (Arnoldus, 1980) could be proxy more reliable than annual precipitation in the description of the susceptibility to erosion due to rainfall characteristics).

We think that the Modified Fournier Index (MFI, Arnoldus, 1980) can be an interesting index to characterize the erosion risk in the study region. 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.

Lines 5-13 pag. 8. As stated by the authors themselves (section 5.3), it is not possible to consider all the available methods for the calculation of USLE input factors and the authors made plausible choices in their selections. However, the authors started their analysis of the R factor by aggregating the long-term monthly amounts to the annual scale, thus losing the possibility of applying the methods that derive the R factor from both annual and monthly precipitations. The reasons for this choice should be provided.

We agree that the aggregation of long-monthly precipitation to long-term annual precipitation reduces the information that is provided by the data. 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 with this comment that this decision in the analysis must be discussed. We suggest to perform a comparison of the application of the MFI (Arnoldus, 1980) with the

methods that were implemented in the manuscript. We suggest, however, to add any analysis in the supplementary materials and add a section on monthly rainfall erosivity in the discussion.

Section 5.2. the discussion presented in this section was expected since the authors described in section 3.7 their intent to compare simulated yields with those collected from field observations. I agree that there are several limitations and difficulties, but the attempt is appreciable. I wonder if another possible reason for the lack of agreement could be represented by the differences between the land use at the time of field experiments and the average one considered in the simulations, (e.g. Sutherland and Bryan (1990) refers to experiments carried out before 1990, whilst the MODIS NDVI data are from 2000 to 2012).

We think that the Anonymous Referee #2 raises a very relevant point here. In the manuscript we addressed a few selected, but certainly dominant limitations for the comparison of in-field data with the calculated soil losses. Yet, other possible sources that can potentially limit a comparison were not mentioned (such as the addressed impact of land use change, particularly as deforestation in the previous century is a frequently mentioned issue for soil loss in Eastern Africa). We suggest to add other potential limitations for the comparability of in-field data to the calculated soil losses in the discussion section 5.2.

Fig. 8a. In order to improve the clarity of the boxplots in figure 8a, I suggest to eliminate the dots, whose presence is not much effective since the data spread can be derived from the length of the whiskers of the boxplots. A similar consideration holds for fig. 9and S1 and S2 in the supplement material.

From our experience the statistical summary measures illustrated by boxplots strongly reduce the information provided by data and can be misleading with small sample sizes and strongly non-normal distributions. We therefore prefer to also show the data that results in the illustrated boxes.

Technical corrections

Pag 1 line 8: "challanges" should be "challenges" Pag.19 line 32 check the sentence Pag. 26 line 9 replace ULSE with USLE

Thank you. We will revise the misspellings accordingly.

References

Arnoldus, H. M. J. (1980). An approximation of the rainfall factor in the Universal Soil Loss Equation. An approximation of the rainfall factor in the Universal Soil Loss Equation., 127-132.

Ebisemiju, F. (1988). Gully morphometric controls in a laterite terrain, Guyana. Geo-Eco-Trop, 12(1-4), 41-59.

Kinnell, P. I. A. (2010). Event soil loss, runoff and the Universal Soil Loss Equation family of models: A review. *Journal of hydrology*, *385*(1-4), 384-397.