Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-602-RC2, 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 #2

Received and published: 18 January 2020

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.

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

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.

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).

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.

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).

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. 9 and S1 and S2 in the supplement material.

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

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

602, 2019.