Dear referee #1,

Thank you very much for your effort, your constructive feedback and your interest in our study. Your comments are very much appreciated and helped us to improve the manuscript. In the following, your comments are written in black and our responses in blue. Citations from the paper are given in italics.

Best regards,

Magdalena Uber on behalf of all co-authors.

In the submitted paper authors investigate past, present and future rainfall erosivity in relation to soil erosion in Central Europe. Authors also compare rainfall erosivity maps derived using 1h precipitation data and maps derived based on the annual precipitation data with the consideration of simple empirical DIN equation. Soil erosion-sediment transport modelling is also conducted using the WaTEM/SEDEM model. The topics is very interesting and within the scope of the HESS journal. The paper is very well written, easy to follow. I only have some moderate comments and suggestions.

Thanks for your positive evaluation of the manuscript and for your comments and suggestions.

Firstly, authors only used the RCP8.5 scenario but this is only mentioned a few times in the manuscript. Authors should definitely state this more clearly in abstract and conclusions since probably RCP2.6 and RCP4.5 would yield smaller increase in the rainfall erosivity and also in the soil erosion rates. It would be definitely very interesting to include these scenarios if input data would be available. Hence, the presented results are significantly influenced by this selection (data availability actually since COSMO-CLM (CPS-SCEN) is only available for RCP8.5). Related to this I suggest that authors add some discussion in relation to using only RCP8.5 and try to elaborate a bit more about the possible results (i.e., deviations from the presented results) using also the RCP2.6 and RCP4.5.

Indeed, it would be very interesting to compare the results obtained with RCP 8.5 to the ones that would have been obtained with RCP2.6 or RCP 4.5. Unfortunately, so far only RCP8.5 was calculated due to the very high computational demand of the convection-permitting model. There are simulations run by other groups with COSMO-CLM using other scenarios, but to our knowledge they are not yet publicly available. Furthermore, it is not sure if these results would be directly comparable to ours, because of possible differences in model versions, model configurations and the like. Thus, we can only present results with one scenario, here. We agree that results would be different if other RCP scenarios were used but we can only speculate how these different results would look like.

Thank you for pointing out that it was not mentioned clearly enough that we only used only one scenario. We added the mention of the limitation that data is only available for RCP8.5 in the abstract (line 15), discussion (line 514-515) and conclusions (line 531), now.

Secondly, part of the results is influenced by the selection of the CMIP5 model ensemble. Since CMIP6 is also available authors should at least add some discussion about the impact of using CMIP5 instead of CMIP6. This is another selection done that has probably quite significant impact on the derived results.

We agree that the choice of model selection surely influences our results. The reason why a CMIP5 model was used is that there are CMIP6 simulations available, but so far only global climate models with a coarse resolution of about 100 km. Within the framework of (EURO-) CORDEX, the global climate models are downscaled with regional climate models to a resolution of approximately 12 km but this has only been accomplished for the CMIP5 simulations, while the ones for CMIP6 are being calculated now. This is also done with ICON-CLM (which is replacing COSMO-CLM) but it will take at least 1-2 years until convection permitting simulations are available.

It was shown that CMIP6 simulations give different results than CMIP5 at the global scale. For Central Europe they are relatively consistent concerning changes in mean seasonal precipitation and extreme precipitation (Palmer et al., 2021; Ritzhaupt and Maraun, 2023). The most notable difference is that mean summer precipitation is decreasing stronger in the CMIP6 ensemble than in the CMIP5 ensemble. This is less the case for extreme summer precipitation (Ritzhaupt and Maraun, 2023).

Following your comment, we added the sentence *"Furthermore, the latest generation of CMIP6 global climate models suggests that the decrease of summer precipitation in Central Europe might be stronger than previously estimated by the CMIP5 model ensemble (Palmer et al., 2021; Ritzhaupt and Maraun, 2023) but these global models are only being downscaled by regional models now."* (line 516-519) in the discussion.

Thirdly, authors used median of the model ensemble, can you add some additional results (e.g., 25% or 75% or 10-90% quantiles) to the Supplement in order to show what is the variability among the included models.

We changed Figure 6 so that it now includes changes in rainfall erosivity with the 15th, 50th and 85th percentile of the model ensemble. Furthermore, we added the following sentences in the main text: "Furthermore, the changes in rainfall erosivity calculated from convection-permitting climate model output are considerably higher than the ones calculated with the low-resolution approach using mean annual precipitation from model output of conventional regional climate model ensembles (Fig. 6). *This is the case not only when future MAP is obtained from the median of the model ensemble but also for the entire plausible bandwidth of models. Figure 6 shows changes in rainfall erosivity estimated with the 15th and the 85th percentile of the model ensemble. Even though this approach only considers changes in MAP and not changes in rainfall intensity, it allows an estimate of model uncertainty due to the differences between the ensemble members. The results obtained with CPS are outside of the bandwidth of the model ensemble because they also represent changes in extreme precipitation in addition to changes in MAP*" (line 377-392).

Finally, the results are also significantly influenced by the data time step (1h) since conversion factor needs to be applied. I suggest that authors add more discussion about the selected temporal scaling conversion factor (i.e., 1.9) and try to elaborate about the possible impact on the derived results (i.e., rainfall erosivity and modelled soil erosion and sediment transport rates).

Thanks for the comment. This point was also made by reviewer 2 and the two community comments in the open discussion. Thus, we added an entire paragraph on this aspect (line 323-347).

Some specific comments:

-L161-162: Please add more details.

Thanks for the comment. We added an explanation why this intermediate nesting was necessary: *"This intermediate nesting was performed because it is not advised to perform direct downscaling from global models with resolutions of approximately 100 km or coarser to the very high resolution of approximately 3 km."* (line 174 - 175)

-Figure 3: Maybe add R^2 to the figure as well.

It was added.

-Discussion in section 3.4 is very useful.

Thank you for your feedback.

-Figure 4: Please add more details about the Erosion Index in the Material and methods section.

We added some more details in lines 223 (the unit, % d^{-1}) and lines 228-230: "*The erosion index varies strongly from one day to another and between grid points. Even averaged over all grid point and over 30 years, there still is a high remaining scatter, so a 13-day moving average is used for smoothing of the curves for the three data sets".*

-L461-465: From my perspective hourly resolution is actually quite problematic especially because the applied conversion factor (only one number (fixed for the whole period)), is used for different type of rainfall events (e.g., intense storms, longer duration events). In relation to this some progress should be made in future.

We understand your criticism about using a fixed conversion factor. We agree that the conversion factor is a source of uncertainty and that the assumption of a fixed value might not be valid. This was added in the discussion now (line 333-335):

"The assumption of a constant scaling factor for the entire model domain and the entire simulated time with different types of rain and shifting intensity patterns is certainly a simplification of reality that adds uncertainty."

For future work it might be considered to use even higher resolution data to avoid a conversion factor. COSMO-CLM model output for the variable precipitation exists at a resolution as high as 5 minutes, but it was found that this data is quite noisy. Moreover, using an even higher resolution would mean that computation times and data size would be even higher (For info: it took about a month of computation time, partly on several computers and partly on a high-performance computer cluster to calculate the rainfall erosivity maps presented here and the size of the hourly precipitation data is about 250 GB for the entire simulated time period).

-L466-471: This is only valid for the RCP8.5. It should be clearly mentioned and discussed.

Thank you for pointing this out. We now mentioned it here (line 531) and in the abstract (line 15) and discuss the limitations of using only one emission scenario in line 508-515.

References

Brown, L. and Foster, C.: Storm erosivity using idealized intensity distributions. TRANS ASAE, 30(2), 378-386, 1987.

Palmer, T. E., Booth, B. B. B., and McSweeney, C. F.: How does the CMIP6 ensemble change the picture for European climate projections? ENVIRON RES LETT, 16, 094042, https://doi.org/10.1088/1748-9326/ac1ed9, 2021.

Ritzhaupt, N. and Maraun, D.: Consistency of Seasonal Mean and Extreme Precipitation Projections Over Europe Across a Range of Climate Model Ensembles, J GEOPHYS RES-ATMOS, 128, e2022JD037845, https://doi.org/10.1029/2022JD037845, 2023.