Dear referee #3,

Thank you very much for your feedback, your interest in the study and your recommendation for publication after minor revisions. In the following, your comments are written in black and our responses in blue. Changes made in the manuscript are indicated with Italics.

Best regards,

Magdalena Uber on behalf of all co-authors

The manuscript is very interesting and I enjoyed reading it. The authors test convection-permitting climate simulations (CPS) on their ability to predict rainfall erosivities in Central Europe. The method is of relevance for the soil erosion community and readers of HESS. I generally encourage the publication of the manuscript in HESS after minor revisions. I see some room for improvement in (i) general information about CPS and (ii) clarifying the different data products that were compared.

(i) Many readers are potentially not familiar with the latest state of the art in climate simulations. CPS may bridge the gap between high spatio-temporal resolution that is relevant for process-based studies and long-term and large-scale studies. Hence, readers from the community of process-based studies probably appreciate information on the CPS product like: what input data is required for CPS, how are they computed, what is the methodological reason for them to perform better etc. Some of this could be added to the Introductions section L100ff, while the implementation of the CPS should be strictly placed in section 2.1.

Thank you for pointing this out. We added some information on CPS that will be helpful for readers that are less familiar with climate modeling. It now says: "*CPS are performed with regional climate models (RCM) on a high spatial resolution (usually ≤ 4 km). Due to the coarse resolution of conventional climate simulations, deep convection has to be parameterized as a sub-grid scale process, which leads to deficits in the realistic simulation of precipitation. This parameterization is* switched off in the model *setup of a CPS (Lucas-Picher et al., 2021),* allowing the model to simulate the precipitation explicitly in each grid cell. *A good representation of deep convection is crucial because it is the main source of precipitation in many parts of the world and especially important as it often generates extreme precipitation (Prein et al., 2015).*" (line 88-95).

We think that the details on input data to the climate models and the way they are computed is beyond the scope of this paper. For the model used here, these details can be found in the references that are given in sect. 2.1.

(ii) Within the paper a lot of different data sets and simulation periods were compared against each other. Assisting the reader with some reductions a structure might help to keep an overview.

Thank you for pointing this out. We added a table, giving an overview of the different data sets as you proposed. We hope that this will help the reader to better understand the data sets.

Introduction:

The state of the art is well highlighted, but I miss more information on CPS that helps to understand their methodological advantage and why this is the case (see above).

We hope that the additional information in lines 88-95 (see above) will help to clarify this issue.

Material and methods:

Numerous different simulation periods, (eval. and proj.) runs, rainfall kinetic energy - intensity (KE-I) relations, external erosivity products make it hard to get the main message. It might be worth considering to reduce the number of different comparisons to focus on the main message.

Unfortunately, it is not possible to reduce the number of comparisons. It is not possible to use only the evaluation or projection runs because the latter do not provide data for the present (which is important for the comparison with observed data) while the former does not provide data for the future.
External erosivity products are also important. Because the presented data is very novel and there are considerable remaining uncertainties, it is necessary to compare it to more established data sources.
Besides for the response to Panos Panagos' comment, we did not compare different KE-I relations.

An overview table providing information (name, period, spatial and temporal resolution, unit, type of data set (map, table), reference, etc.) on the different data sets that were compared might help a lot.

Such a table was added at the end of section 2.1 for the five data sets presented here. However, we did not include the spatial and temporal resolution because it is the same for all the data sets. This information is given in the text.

I cannot find a definition of the evaluation and projection run. It is not fully clear to mean what was done here.

Thank you for pointing out that this was not described clearly. The difference between the evaluation and the projection run is that the former was driven with reanalysis meteorological data (ERA40 and ERA5, based on observed data) while the projection run was driven with the output of a global climate model (MIROC5). We added the following sentences (line 183-187):
"The evaluation simulation driven with reanalysis data serves as a reference for the historical simulation driven by a global climate model. It quantifies how well the historical climate can be reproduced by the historical simulation and how large the differences of specific climate variables are between both simulations. In addition, Rybka et al. (2022) used the evaluation simulation for a comparison with high resolution observational precipitation data sets to analyze the model performance for extreme precipitation."

We hope that this makes the purpose of using an evaluation run clearer.

Can you provide a rough estimate on the sensitivity to use the KE-I relation by Wischmeier and Smith 1978? There are many but some are more frequently used like the exponential function by Brown and Foster 1987 as it is suggested in the RUSLE by Renard et al. 1996.

Following the comment by Panos Panagos, we tested the effect of using the Brown and Foster (1987) equation on a subset of our data. In this case, the results obtained with the equation proposed by Wischmeier and Smith (1978) were on average 1.23 times higher than the ones obtained with the Brown and Foster equation.
Nearing et al., 2017 and Hanel et al., 2016 compared different KE-I equations.

This information was added in a new paragraph in the revised manuscript (line 323-347).

Here we did not test different KE-I equations systematically because of the considerable computation time to calculate R-factors for the large data set.

Results and discussions:

A brief discussion on the usability of CPS for landscape or field scale studies could be interesting. Just out of curiosity, how good does the CPS work for specific points in space? How good is a comparison against long term rainfall gauges?

We are not aware of any field scale studies comparing time series obtained from CPS to the data of specific rain gauges. Rybka et al. (2022) compared sub-daily extreme precipitation from COSMO-CLM to i) the German radar climatology dataset RADCLIM and ii) the KOSTRA-DWD product that statistically estimates extreme precipitation in Germany based on rain gauge observations. This comparison showed that the CPS performs well in reproducing observational data for durations above 12 h but overestimates hourly extreme precipitation intensities.

I do not understand the point of calibrating the transport capacity to end up with same soil redistribution rates. In section 2.3 - L218 the reason to apply WaTEM/SEDEM is named as "To study the effects of changing rainfall erosivity on soil erosion [...]". From my perspective the benefit to apply WaTEM/SEDEM is to get a rough number on the differences in soil redistribution and sediment delivery to the stream network in Mg ha-1 yr-1.

On the scale of Fig. 8, the differences between the realisations are not visible. From my perspective, Figure 9 provides a good relative number of the effect in R-factor calculation in CPS and MAE.

Following the recommendation of reviewer 2 and Shuiqing Yin, we deleted the part on soil erosion modelling to focus entirely on rainfall erosivity. Thus, the two figures were deleted and the calibration is not relevant. To respond to your comment nonetheless:

The transport capacity in WaTEM/SEDEM has to be calibrated to ensure that simulated sediment yields are as similar as possible to observed ones. Of course, this is only possible for the past. This was done for both R-Factor data sets (calculated from CPS and from MAP) so the simulated erosion rates shown in Fig. 8 are very similar. For the future projections on the other hand the changes in rainfall erosivity differ strongly between the two data sets (CPS vs. MAP) so the projected changes in rainfall erosivity shown in Fig. 9 are very different.

The drawback of using a single CPS instead of an ensemble is highlighted multiple times throughout the manuscript. Would it make sense to assess if the model tends to under or overpredict rainfall erosivity in a comparison against rainfall ground observations (rain gauge or laser-distrometer)? See comment on comparison against rain gauge point data above.

We certainly agree that it is important to compare our results to rainfall erosivity calculated from observational data. Here we preferred using the R-factor maps published by other authors. We agree that a comparison to rain gauge point data would also be good. We will keep this in mind for future research.

Thank you for this nice piece of work!

Thanks for your appreciation of our work and thanks again for your time spent and your comments!

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