

Dear referee,

Thank you very much for appreciation of our revisions, for your repeated effort to review the manuscript and your further helpful comments.

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 by Uber et al. about the study on using convection-permitting simulations (CPS) for estimation of future rainfall erosivity in Central Europe gained a lot by their revisions. The authors took carefully over several suggestions of the referees. By this, the manuscript improved, especially by removing the chapter on soil loss estimations and the focus on rainfall erosivity derived from the different precipitation data sets. The overview of the different data sets provided by table 1 is of benefit to understanding their descriptions. The added paragraph on the discussion of the correction factors is of high importance. Still, some minor revisions are suggested as follows:

The application of the regression in DIN 19708 for estimation of erosivity by using the mean annual precipitation (MAP) is still of critic. Your argument that the DIN 19708:2022 did not exist when you performed your calculations is not a good one as the DIN 19708:2002 should have already existed before your first submission of the manuscript. In any case, you bring in the arguments for no longer using the regression nicely yourself: I) In the lines 49 to 51 of the revised, tracked manuscript you write that ‘low-resolution approaches’ for estimation of future rainfall erosivity are not permitted as it is expected that the frequency distribution of rainfall events changes by climate change. II) In line 70 you are referring to studies which observed that rainfall erosivity already increased. III) In lines 83 to 85 you state that regression-based models using monthly or yearly sums of precipitation “are only valid for the time period for which these models are calibrated and lead to underestimations of the rainfall erosivity if extreme precipitation events increase with time, as suggested by many climate change scenarios.”. IV) In lines 123 and 124 you are writing that using MAP for estimation of rainfall erosivity may not be valid for future climate with precipitation frequency and magnitude different from that of the precipitation used for establishing of the equation. In consequence of these notes (I to IV), it is surprising for the reader that you used the equation in DIN 19708. Therefore, it needs a clearer justification of the purpose of nevertheless using this equation. Chapter 2.2.2 provides still just the explanation that you used the low temporal resolution approach “for comparison”. This seems not enough to understand the purpose of using the equation. This is still the case albeit inserting “15th and 85th percentile” in line 237 of the revised, tracked manuscript. I highly recommend clarifying earlier than in the results & discussion part (lines 388 to 392) the actual purpose and chance of including the low-resolution approach. In addition, I suggest referring that the current version of DIN 19708 (DIN19708:2022) explicitly states that the regression based on MAP can only be used for ‘historical observations’.

We understand your criticism and are convinced that the use of CPS is a good way to overcome the limitations of using MAP. To stress this, but also to explain why we used the approach nonetheless we added the following lines in the manuscript, section 2.2.2:

*“The low temporal resolution approach was used here because it allows a representation of the bandwidth of results obtained with a regional climate model ensemble and thus an estimate of model uncertainty which is not yet possible for CPS. Nonetheless, the main limitation, i.e. the neglect of the effect of increases in heavy rain, of the approach has to be stressed again. This shortcoming is overcome by CPS and is one of the reasons why the most recent version of DIN 19708 (DIN 19708:2022-08) recommends to use equation 3 only for historical observations”* (line 240-244 in the tracked manuscript in second revision)

The added section on the comparison of erosivities calculated by using ‘USLE-based’ and the ‘RUSLE- based’ equation is very interesting. Still, I suggest to remove or revise the detailed discussion of the discrepancy of your results and the results of Panagos et al. (lines 344-347). There might be more differences between your approach and the one of Panagos et al. than you mention there, e. g. the spatial and temporal coverage as well as the different criteria for defining erosive rainfall events. The criteria are of importance for the number of events. Lower thresholds for the maximum 30 min rain intensity result in a higher number of erosive events which sum up to higher annual erosivity. Your criteria might deviate from the ones used by Panagos et al. in the study to which you are referring to. Therefore, I suggest to either complete the detailed discussion of explanations for the discrepancies between the two studies by considering all possible causes, or to delete it (lines 344 - 347) and just discuss consequences of the differences between USLE-based and RUSLE-based R factors.

Thank you for this comment. We agree. As we cannot quantify all possible causes for the discrepancies, we deleted the respective lines as you recommended.

The chapter 3.1.3 is part of your results and discussions chapter but misses a more intensive discussion of possible reasons for the discrepancies of your results to the studies you are referring to. It would be of interest I) whether the scatter of the erosion index increased from past to near to far feature as one would expect from a possible increase in extreme events occurring on single days in limited areas, and II) what the reasons could be for your results not showing seasonal changes of the erosion index. Instead of deleting the sentence “Thus, there might be regions in which seasonal shifts occur, but that average out over the larger modelling domain used in this study”, I encourage you to discuss it and to clearly state that this is an open question which you cannot answer so far, if this is the case.

Thanks again for the comment. We will keep your remark I) in mind for further analysis. Concerning your remark ii) we actually do not know what are the reasons for the discrepancies, so we stated this in the manuscript as you suggested by adding “*The reasons for the discrepancies between this study which did not detect significant changes in the seasonal distribution and the other studies that did observe trends are not clear yet and an remain an open question.*” in line 367-369.

Some more minor comments referring to the revised, tracked manuscript are following:

Page 5, line 142: I suggest to already state the applied emission scenario (RCP8.5).

It was added as you suggested.

Page 11, lines 290-294: This paragraph doesn’t seem to fit here. Might be moved to the introduction.

We prefer to leave this paragraph here in order not to overcharge the introduction. It is supposed to show a further possible use of the data that is presented in this section. To make this clearer we changed the sentence slightly:

“As such, *the rainfall erosivity data presented here* can also provide valuable information for other hydrological applications dealing with extreme rainfalls such as the assessment of (future) risks for flash floods or landslides or identifying zones that are prone to these natural risks (Fiener et al., 2013; Panagos et al., 2015c).” (line 296-298)

Page 12, line 343: The wording “Our values” is confusing; maybe you can rephrase it to “The USLE-based R factors”.

It was changed as suggested (line 348).

Page 16, line 419/420: Do you mean with “...the trends in the two data sets can differ considerably” that the trends differ in some regions of your spatial extent? Moreover, what do you mean with “usually” in the following sentence? Do you mean in most of the area?

This refers to the comparison of the other studies presented in table S1 to our values. Using the data from the evaluation run differs from using the data of the simulation run with stronger trends being found in the simulation run in most cases. As the time period for these cases is always the same, “usually” refers to most of the considered regions. Thus, “usually” was replaced by “*in most regions*” in line 424.