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'.

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.

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.

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).
Page 11, lines 290-294:	This paragraph doesn’t seem to fit here. Might be moved to the introduction.
Page 12, line 343:	The wording “Our values” is confusing; maybe you can rephrase it to “The USLE-based R factors”.
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?