Dear referee #2,

Thank you very much for your detailed review, for your interest in the study and for your valuable feedback on the manuscript. Your comments helped us to improve the paper.

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 study of Uber et al. is very interesting and of high importance for proper estimation of future rainfall erosivity and future soil loss in a changing climate. The topic of the submitted manuscript addresses well the scope of the HESS journal. Uber et al. use precipitation data from convection-permitting simulations (CPS) for estimation of future rainfall erosivity in Central Europe. The use of CPS-based precipitation data for estimations of future erosivity is the novelty of their study. CPS include explicit simulation of large deep convection cells what is not the case in conventional convection-parametrized climate models but important for erosivity estimations. CPS-based precipitation data are available in a spatial resolution of around 3 km and a temporal resolution of 1 h. This spatial and temporal resolution of precipitation data is considerably higher than it is the case for the output of convection-parameterized models. A high spatial and temporal resolution of precipitation is also of importance for erosivity estimations. Uber et al. describe well in their introduction the potential of CPS-based precipitation data for improved erosivity estimations for the near and far future and appropriately explain the limits of current CPS outputs for it. The major limit of CPS-based precipitation data for erosivity projections is the lack of ensembles of CPS. This lack results in an unknown uncertainty of erosivity projections. So far comprehensible, Uber et al. try to account for this by comparing single-CPS-based erosivity estimates with, first, erosivity estimations with precipitation data from CPS evaluation simulations driven by observation data, and, second, with erosivity estimations by a regression equation based on mean annual precipitation sums using precipitation data from conventional convection-parametrized climate model ensembles. Although their attempt is much appreciated, it must be pointed out that this equation (Equation 3 in the manuscript) is explicitly not applicable to current and future erosivity estimations. This is described in the DIN 19708:2022-08. In consequence, the validation approach of comparing single-CPS-based erosivity projections with erosivity projections based on Equation 3 of the manuscript seems incorrect. This approach might be possible for the past period but should be carefully applied due to the trend of increasing erosivity that has already existed in recent decades.

Thanks for your comment. We certainly agree that the use of the DIN-equation (Equation 3) established with past data for projections of future rainfall erosivity has many flaws and should be avoided. However, due to a lack of high-resolution data, it is quite common to estimate future rainfall erosivities based on regressions with lower resolution rainfall totals that were established in the past. We did not intend to validate our results by comparing them with the results obtained with the projections based on mean annual precipitation (i.e. results generated with Equation 3). Instead, we wanted to show that the changes in rainfall erosivity projected using CPS are much higher than estimated previously with Equation 3.

We fully understand your doubts on using Equation 3 and we argue that one of the main advances of using CPS for erosion modelling is that we don’t have to rely on regression

equations such as Equation 3 anymore. Nonetheless, we think that using Equation 3 also has advantages over using CPS, the main one being that it can easily be applied to (projected) climate data of basically any temporal resolution. In our case, it enables an estimate of changes in rainfall erosivity due to changes in mean annual precipitation from an ensemble of climate models. Thus, variability between climate models can be assessed which is not yet the case for CPS.

Apparently, we did not point this out clear enough in the manuscript. Thus, we changed Figure 6 so that it now includes results showing the bandwidth of the climate model ensemble. Furthermore, we added the following paragraph (line 377-392):

"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 erosion rates 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*."

Uber et al. also use their CPS-based erosivity estimates for estimation of future changes of soil loss rates by water erosion and sediment delivery. For this, they select the Elbe River basin. Their USLE-based soil loss estimates for the near and far future consider changes in the R-factor exclusively. Although Uber et al. calculated the erosion index from their erosivity projections (chapter 3.1.3), they don't consider changes in the C-factor for their future soil loss estimations. Also, potential changes in crop growth due to prolonged vegetation periods are not considered or changes of the other factors.

Indeed, we focused on the R-factor and did not consider changes in the C-factor or the other factors, because -compared to the strong changes in R-Factor- they are less affected by climate change. As suggested by you below and also by Shuiqing Yin in her comment, we removed the part on soil erosion modelling in the manuscript.

Only future changes in the R-factor are considered in their erosion modelling. In consequence, estimates of future relative changes in soil loss are equal to relative changes in erosivity. This is because the USLE is a multiplicative approach. So, the purpose of the efforts of Uber et al. for the chapter on simulated erosion rates is unclear. I recommend to either consider, at least, changes in the erosion index or to strongly shorten the chapter on future erosion rates.

As suggested we removed the part on soil erosion modelling.

The manuscript would benefit from a concentration on single-CPS-derived erosivity projections and an in-depth discussion of e. g. the applicability of scaling factors accounting for spatial and temporal resolutions of precipitation data lower than those from e.g. 1-min precipitation data from rain gauges. This would also address the comment by Panos Panagos (https://doi.org/10.5194/hess-2023-120-CC1) who criticises the applied temporal scaling factor 1.9 for being too high. Panagos et al. (2015) found a lower temporal scaling factor, but

this was used to correct only to 30-min resolution. Considering in addition their temporal scaling factor to correct from 30-min resolution to 5-min resolution (they calculated no factor for 1-min resolution), it results in a factor of even slightly higher than 1.9. In consequence, the temporal scaling factor from Fischer et al. (2018) applied by Uber et al. seems to be in accordance with the temporal scaling factors found by Panagos et al. (2015). Nevertheless, the question could be discussed whether the temporal scaling factor is still valid when rainfall intensities further increase in the near and far future. Moreover, it could be of interest to explain/discuss why no spatial scaling factor was necessary to account for the underestimation of erosivity despite the spatial resolution of around 3 km. A possible reason might be the overestimation of hourly extreme precipitation intensities mentioned in the discussion (page 19, line 444).

Thank you for your very helpful comment. We included these aspects in the revised version of the manuscript by adding a paragraph on the effect of using different equations to calculate specific kinetic energy and the effect of the scaling factor as well as on the uncertainty related to the scaling factor (line 323-347).

Although the manuscript is very well written, it could benefit from a separation of the validation of precipitation and erosivity from CPS projection runs by precipitation and erosivity from CPS evaluation runs, and the results of erosivity from CPS projection runs and their discussion in context to results from other studies.

We are not sure what exactly you mean here. As the time periods that are covered by the evaluation run (1971-2019) is not identical to the one of the projection run (1971-2000 and the two future periods), the results from other studies could only be compared to the evaluation runs for the studies reporting past, observed erosivity or to the projection runs for the studies reporting projected future erosivities.

All in all, the manuscript could benefit from revisions as suggested above and in more detail in the follwoing list below.

Detailed comments:

page 2, line 32: The citation of Nearing et al., 2017 for the definition of rainfall erosivity gives the impression that rainfall erosivity was no topic before. I suggest referring to an appropriate earlier publication from the 1950s to 1970s, at least in addition.

The definition by Nearing is very precise and we did not find such a good definition in the early publications. So we added the fact that the concept is much older in a subphrase: "Rainfall erosivity *was first quantified in the 1950s and can be* defined as "the capability of rainfall to cause soil loss from hillslopes by water" (Nearing et al., 2017)" (line 31-32).

page 2, line 38: Same as comment above; an earlier publication is recommended at least in addition to Wilken et al., 2018

An earlier publication (Laws and Parsons, 1943) was added (line 38).

page 2, line 40/41: The "Thus," is irritating as it gives the impression that there is a conclusion, but it is not obvious from what this conclusion is taken. It doesn't seem to be taken from the preceding sentence. Please, make it clearer.

It was changed accordingly, the "Thus," was deleted.

page 2, line 42: It might be rather the suitability of the R-factor equations to express rainfall erosivity which depends on the temporal resolution of the precipitation data.

It was changed accordingly (line 42-43).

page 2, line 47: "This approach" is not clear as you refer in the preceding sentence to "low-resolution approaches" which suggests that there are several approaches.

Thanks for pointing this out. It was changed to plural ("*These approaches …*", line 48).

page 2, line 49/50: Please add a reference to the expectation of changes in the frequency distribution of rainfall events.

The reference is given later in the text (line 62-65). Here we think that it would be misleading as it would suggest that the reference is referring to the entire sentence.

page 2, line 56: Every error needs to be assessed critically. This sentence has not really information. Please clarify.

Thanks for the comment. We agree and deleted the sentence. The potential and limitations of satellite-based precipitation products is not the topic of this paper.

page 2, line 57: Please clarify that an increase in precipitation (intensity and annual total) is likely rather for wet than for dry global regions according to your reference Sun et al., 2007. Moreover, to my understanding, changes in precipitation are the result of the changes in temperature and both, changes in temperature and precipitation are the changed climate. So, changes in temperature and precipitation are not the result of climate change but they are the changed climate itself which, simplified, results from increased concentrations of greenhouse gases in the atmosphere.

We rephrased the sentence. It now says: "*Globally, precipitation is increasing due to an increase in atmospheric water vapor in warmer air (e.g. Allan et al., 2020; Fowler et al., 2021)*" (line 59-60). Of course, there are regional patterns, but we prefer not to go into the details of global precipitation changes but focus on Central Europe.

page 2, line 59/60: Please clarify what specifically is increasing. Do you mean the number of extreme events?
It is not clear what you mean with an "intensified hydrologic regime". Please edit.

It was changed accordingly. We hope that the explanation is clearer now. It now says: "Furthermore, *warming and higher atmospheric moisture fluxes lead to an intensification of the water cycle causing an increase of the intensity and frequency of extreme precipitation, globally as well as in Central Europe (Allan et al., 2020; Brienen et al., 2020; Fowler et al., 2021). Strong increases in extreme precipitation are* due to the fact that the share of convective precipitation in total precipitation is increasing (Berg et al., 2013)." (line 62-66).

page 2, line 60/61: "This is due to the fact…" suggests that you are describing a cause-and-effect relationship, but, actually, it just clarifies the preceding sentence. I would suggest combining both sentences (line 59 – 61) into one but clear sentence.

It does describe a cause-and-effect relationship because convective precipitation often causes intense precipitation events while stratiform precipitation rather causes long-lasting but less intense rain events.

page 3, line 75: Do you mean 11 out of the 196 studies?

Yes. It was changed accordingly (line 80).

page 3, line 95: Do you mean with "at the time scales needed…" the length of the time series? If yes, it might be clearer to write "for the length of time series" as 'time scales' may be used rather in context to the temporal resolution of the data. If you agree, please keep it in this way throughout the manuscript.

Thanks for pointing this out. It was changed accordingly (line 105, 151).

page 4, line 108: Please add the reference where to find the published data.

The reference was added (line 118).

page 4, line 110: Was the temporal resolution of CPS already mentioned? If not, please add so that it is clear what you mean with high temporal resolution.

The temporal resolution of the COSMO CLM output used here is given later in the text (1 h, line 188). Unfortunately, it is not possible to add a typical temporal resolution of CPS here. This is due to the fact that the time steps of the model calculation are very short (a few seconds) while the temporal resolution of the model output depends only on the choice of the modeler at which time steps the output is actually written.

page 4, line 120: Which size of regions to do mean? In context of the globe (mentioned in the same sentence), a region could also be Europe, but you may rather think of smaller areas? Maybe it is possible to provide the typical spatial extent in $km^2$ or to refer to a specific geographic region as an example.

It now says "i)limited spatial extent of most CPS. While regional and global convection-parameterized simulations cover the entire globe, to date *CPS are only available for limited areas in most regions of the world (e.g. Central Europe) due to constraining factors like computing power*." (line 130-132). We hope that this makes the availability of CPS clearer. They are very different in extent and exist in most regions of the world, but don't cover the entire globe.

page 6, line 170: It should be specified that you mean the final CPS model output as you mentioned several different models before.

It was clarified. Now it says "*The COSMO-CLM CPS model output…*" (line 188).

page 6, line 172: What is the 'FPS-convection contribution'?

It was rephrased, now saying "The overall configuration of our simulation has been taken from *a joint contribution of the CLM-community to a CPS experimental study for Central Europe* (Coppola et al., 2020)." (line 190-192).

page 7, line 182: Please add that the point that these must be 6 hours without any precipitation.

Thank you for pointing this out. It was added accordingly (line 203).

page 7, line 186: In case of $I \geq 76.2\ mm\ h{-}1$, the kinetic energy should be 28.33 *** $10^{-3}$**; please correct this factor.

Thanks for having noted this error. It was corrected (line 207).

page 7, line 194: If 'grid cell' is meant to be equal to 'grid point' then it may be good to use the same wording throughout the manuscript for the sake of simplicity.

It was changed accordingly (line 215 and elsewhere).

page 8, line 208: It is not clear why erosivity is also calculated by Equation 3. "For comparison" is not enough as a reason. Is it meant for validation of the R-factors derived from single-CPS-based precipitation data? This equation is not valid for calculation of current and future erosivity as described in the beginning.

We understand your criticism of using equation 3, see our response on your comment in the beginning. We did not mean to validate our results with this comparison but rather wanted to show that the new results presented here are considerably higher than previous estimates obtained by using Equation 3.

page 8, line223: The value 0.2 mm/a for simulated erosion rate should be based on more information. Is it a multi-year average and of which time period - past, present, future? What means 'locally'? Is it simulated for a single field? Or averaged for e.g. a county? Please specify to what this value relates.

The part on soil erosion modelling was deleted as suggested.

page 8, line 220-230: This text section may be rather part of the introduction. Think about shifting it to page five, line 131.

The part on soil erosion modelling was deleted as suggested.

page 9, line 245: You write that the aim is to identify the impact of climate change on soil erosion and therefore you just consider changes in the R-factor. But also, the erosion index changes and the soil loss ratios by e.g. prolonged growing season (even when the management itself is not changed). Climate change might also affect soil erodibility (e. g. by changes in soil organic matter). So, you should not write that you aim to identify the impact of climate change but rather the impact of changes in the R-factor by climate change on soil erosion estimates.

The part on soil erosion modelling was deleted as suggested. To answer your question nonetheless:

We agree that climate change also affects the other factors, but the main effect is on the R-factor. As in every modelling study, we had to make assumptions and simplifications that certainly introduced errors and uncertainties. So we think that – considering all the remaining

uncertainties – the impact of climate change on soil erosion is quite well represented by its effect on the R-factor.

It would be interesting to also consider the other effects of climate change that you mentioned and to attribute changes in soil erosion to the different effects of climate change. We will keep your comment in mind for future work.

page 9, line 247f: Sediment load measurements of which years were used for calibration and validation of the model? Please add this information.

The part on soil erosion modelling was deleted as suggested. To answer your question nonetheless:

The data covers the time period between 1963 and 2020 with the majority of data being available after 1990. We are aware that the sampling dates correspond not exactly to the reference period of the model but neglecting the data outside the reference period would have caused a loss of valuable data. This was discussed by Uber et al. (2022).

page 9, line 255f: Why don't you distinguish between past 1971-2000 and present 2001-2019? Would be good to have average annual erosivity of the entire modelling domain for both periods separately.

This information is given in Figure 5 and line 369-372. Here we focus on the spatial pattern where the differences between the past and the present (or more precisely the differences between CPS-hist from the projection run, CPS-eval-hist, CPS-eval-present) are smaller than spatial differences.

Moreover, don't you mean that average annual erosivity in the lowlands is between 50 and 90 N $h^{-1}$ $a^{-1}$ rather than "erosivity in the lowlands is on average about 50 – 90 N $h^{-1}$ $a^{-1}$"? The term 'on average' lets one expect a single value.

You are right, it was chanced accordingly (line 278).

page 9, line 257: Similar to the comment above; how to interpretate the range 90 – 96 N $h^{-1}$ $a^{-1}$ as a mean? Is this range arising from a confidence interval around the mean? "The mean of the entire modeling domain" let one expect a single value which is then also given for the near and far future. Please edit respectively.

It was clarified in the manuscript. It now says "*In the past, the mean of the entire modeling domain is 91 N $h^{-1}$ $a^{-1}$ in the evaluation run (CPS-eval-hist) and 96 N $h^{-1}$ $a^{-1}$ in the projection run (CPS-hist).*" The values for the near and far future are now given in section 3.2 (line 371-372).

page 9, line 258: You are inviting people to use your R-factor results for USLE-based soil loss modelling. But for this, the erosion index is missing. So, you would need to provide these data as well. Moreover, I like to recommend providing most important information about the data and development in a table and pointing out that the results are based on a single climate model and not on a model ensemble using the RCP8.5 scenario.

We added the phrase "*Data of the erosion index can be provided on request to the first author.*" In the Data Availability section. We are not sure which information you would like

to see in a table. We believe that you are referring to the description of the data on Zenodo. As the name of the model is given in the description, it is clear that the results are based on a single climate model. We added the fact that the emission scenario RCP 8.5 was used in the description on Zenodo. It now says "Past, present and future rainfall erosivity in Central Europe calculated from convection-permitting climate simulations in COSMO-CLM *using emission scenario RCP 8.5."*

page 11, line 294f: Is it possible to calculate in addition the mean annual R-Factor from the evaluation runs for the respective time periods used in the other studies and to compare these? Moreover, the differences could be discussed in context to changes of the R-factor in the last decades as discovered by other studies already.

It would be possible, but then the comparison would concern a "new" data set and not the one that was published on Zenodo and that is presented here. To make the point that differences in temporal coverage lead to different results in the context of changes of the R-factor in the last decades we edited the following sentence. It now says "Differences in temporal coverage are especially important *given the observed increases in R-factors in the last decades (e.g. Hanel et al., 2016a; et al., 2019a; Auerswald et al., 2019b)*" (line 319-321).

page 12, line 300f: What is the reason for comparing results of other studies for the R-factor with the results of the evaluation run but for the erosion index with the results of the projection run?

The calculation of the erosion index took a lot of calculation time, so it was only calculated for the data from the projection run, not the one of the evaluation run.

page 12, line 300f: How does the seasonal distribution of erosivities from evaluation runs and projection runs fit together for the two periods respectively? Please add information about this.

See our answer above.

page 12, line 316f: Should it not be relatively easy to analyse the erosion index restricted to the area of the individual countries? This should be done instead of guessing that the changes in the intra-annual distribution of erosivity level out across the modelling domain.

Indeed, this possible explanation is rather a guess than based on analyzing the data. Thus, we deleted 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". The question why the model doesn't confirm the observed changes in seasonality is still open and we will keep it in mind for further research.

page 13, line 321f: Mention the mean annual rainfall erosivities for near and far future so that it is easier to compare the results of evaluation and projection run.

The values were added (line 371-372).

page 14, line 339: It is not the age but changes in the precipitation characteristics and the fact that it does not consider rainfall intensities but only total rainfall amount.

The sentence was revised, now saying “Thus, *changes in precipitation characteristics* and the fact that it does not consider heavy precipitation raise concerns that the equation can be transferred to the future (Gericke et al., 2019)” (line 396-397).

page 14, line 341: The DIN 19708 also explicitly states that the regression equations are not suitable for calculation of R-factors of (even) the presence, not to mention the future.

We believe that you are referring to the updated DIN 19708:2022-08. At the time when we calculated R-factors using Equation 3, only DIN 19708:2017-08 was available.

page 15, line 364f: Discard the part of the sentence which refers to the comparison with the results from Equation 3.

It was changed accordingly (line 422-423).

page 15, line 366: Please add the references which report higher future increase in erosivity in comparison with your results from CPS.

The references were added (Panagos et al. (2017, 2022), line 425).

page 16, line 370f: Please indicate the period to which your results on erosion rate estimates relate here.

The part on erosion modelling was deleted as suggested.

page 17, line 391f: It is unclear what the message of the first sentence is. Relative changes in erosion rate are equal or slightly higher than relative changes in sediment delivery to water bodies? Moreover, it is not indicated to which simulations (‘CPS’ or ‘MAP’ approach?) the mentioned changes in sediment load in the near and far future relate. Please revise.

The part on erosion modelling was deleted as suggested.

page 17, line 397: With respect, your results are not necessary to know that USLE-based simulations of future changes in soil erosion are highly sensitive to changes in the R-factor when just the R-factor is changed while the other factors are assumed to remain equal.

The part on erosion modelling was deleted as suggested.

page 18, line 410f: Please indicate that the resolution of precipitation data of 3 km is just high in comparison to most other projected precipitation data (as this is not the case in comparison to measured precipitation data by e. g. ground-based radar).

As this paragraph is on the advantages and the potential of using CPS, it would be contradictory to start it with a limitation (i.e. that the resolution of CPS is still lower than the one of radar derived precipitation products). Compared to the resolution of global (~ 100 km or more) or regional climate models (in Europe ~ 12 km) or typical densities of rain gauge networks, we think that it is justified to say that the 3 km resolution of the CPS is in the same order as the 1 km resolution of typical radar precipitation products.

page 18, line 427f: The conclusion of the results from Chapman et al. (2021) cited by you would be that projected monthly (? - what is “the same temporal resolution”?) precipitation

sums are higher from the convection-permitting model than from the conventional convection-parameterized model as changes in erosivity resulting from changes in the rainfall intensity are not necessarily reflected in changes of e.g. monthly precipitation sums. What is the reason for that?

We added the resolution (3h, line 486).

page 19, line 435f: Here, you discuss limitations of the USLE although the intension of chapter 3.4 is to discuss potentials and limitations of CPS for calculation of erosivity. Delete the part on limitations of the USLE and concentrate on discussing the limitations by CPS-derived erosivities as calculated in your study. For example:

Agreed, we deleted the sentences on the limitations of the USLE.

Do you expect an underestimation of erosivity in near and far future by using a constant temporal scaling factors of 1.9 for accounting of erosivity underestimation by using 60-min data?

We added this thought in section 3.1.2 where it now says "*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*" (line 333-335)

and

"*[…] and the temporal scaling factor might have to be adapted to future data with higher intensities of extreme events*" (line 339-340).

In the introduction you mentioned that CPS can simulate large deep convection cells but not smaller shallow convection (line 84). Which consequence do you expect from this for projected near and far future erosivity estimates?

CPS do simulate shallow convection but it is not calculated explicitly but using a parameterization. Because shallow convection doesn't generate heavy rain, it is assumed that the parameterization doesn't have a strong effect on rainfall erosivity.

Are there any indicators which allow a guess whether your single-CPS-based results of erosivity in the near and far future are laying in the lower, middle, or upper range of erosivities derived from future ensembles of CPS?

We assume that our estimates of rainfall erosivity would be rather at the upper end of future ensembles of CPS. The reason is that the driving simulation MIROC CCLM is rather at the "wetter" end of the bandwidth of the EURO-CORDEX ensemble. Also, the overestimation of heavy rain in COSMO CLM causes high values of rainfall erosivity.

But the research on CPS is very active and a lot of effort is made in the community to optimize these models. Thus, it is still unclear how future ensembles of CPS will look like and we don't want to speculate on that without having actual data to compare our results to.

page 19, line 442f: The comparison of modelled and measured precipitation data is an important result. What is the reason that you don't provide this information earlier, e. g. when you compare your erosivity estimations from projection runs with those from evaluation runs? What is the reason for the overestimation of hourly extreme precipitation intensities? How large is this overestimation?

The comparison of modelled and measured precipitation has been presented elsewhere (Rybka et al., 2022). The overestimation of hourly extreme precipitation is probably due to the configuration of the CPS.

We agree that this has to be kept in mind when the rainfall erosivity data presented here is used. Thus, we added a sentence in line 503-504: "*This* [the overestimation of hourly extreme precipitation] *leads to an overestimation of the rainfall erosivity presented here that has to be kept in mind.* Thus, it is important to compare the R-factors calculated here to the ones calculated from measured rainfall data."

page 19, line 446: What do you mean with "estimates of future precipitation"? Do you mean hourly precipitation sums, or monthly, or yearly?

This sentence is meant as a general statement, saying that future precipitation is harder to project than future temperature.

page 20, line 468: Which spatial scale do you mean with "locally"? Is it a single 'pixel' of a certain size? How can the increase in soil erosion be higher than the increase in erosivity although all other USLE factors remain constant? Do you refer once (line 467) to an average across an area and once (line 468) to a single 'pixel'?

It referred to the subbasins shown in Figure 9. But this part was deleted in the manuscript.

According to language/grammar:

Please write either "modelling" or "modeling" throughout the manuscript.

Thank you for pointing out this inconsistency. It was harmonized.

page 2, line 34: A comma might be necessary before "and" in "…its derivates and models…".

We did not use the Oxford comma throughout the Manuscript.

page 4, line 108: Please use either singular or plural of 'data' consequently throughout the manuscript.

It was harmonized.

page 5, line 131: "…the new rainfall erosivity maps" sounds as it will be official maps. I suggest to revise this sentence. Maybe like "Furthermore, modelled rainfall erosivities of these periods were used in the USLE-based model WaTEM/SEDEM to estimate changes in soil erosion and sediment delivery to the Elbe River."

We replaced "maps" with with "data".

page 5, line 137: With ‘or results’ you may mean ‘our results’. Please correct.

It was corrected.

page 7, line 182: Instead of “We use”, it may be “We used”.

It was corrected.

page 9, line 246: ‘r’ is missing in ‘future’.

It was corrected.

page 9, line 254: Should be ‘Alps’ instead of ‘Alpes’. Please adjust also Fig.1 accordingly.

It was corrected.

page 9, line 255: Should be ‘Alps’ instead of ‘Alpes’.

Thank you for noting this error. It was corrected throughout the manuscript.

page 12, line 315: Why do you write “…despite…” here? Isn’t it logical that the erosion index needs to decrease in other months when there is an increase in May to October?

You are right, we replaced “despite” with “and”.

Thanks again for your thorough review. We appreciate the time and effort you took to comment on the paper. We hope that we addressed all your comments satisfactory and we are convinced that your remarks and propositions helped us to improve the manuscript.

References

DIN-Normenausschuss Wasserwesen: DIN 19708:2022-08 Bodenbeschaffenheit - Ermittlung der Erosionsgefährdung von Böden durch Wasser mit Hilfe der ABAG, https://dx.doi.org/10.31030/3365455, 2022.

Fischer, F. K., Winterrath, T., and Auerswald, K.: Temporal-and spatial-scale and positional effects on rain erosivity derived from point-scale and contiguous rain data, HYDROL EARTH SYST SC, 22, 6505-6518, https://doi.org/10.5194/hess-22-6505-2018, 2018.

Panagos, P., Ballabio, C., Borrelli, P., Meusburger, K., Klik, A., Rousseva, S., Tadić, M. P., Michaelides, S., Hrabalíková, M., and Olsen, P.: Rainfall erosivity in Europe, SCI TOTAL ENVIRON, 511, 801-814, https://doi.org/10.1016/j.scitotenv.2015.01.008, 2015.