We are grateful for the careful, proficient and helpful review of our manuscript. Our replies are inserted in blue in the following.

Interactive comment on

"Rain erosivity map for Germany derived from contiguous radar rain data"

By Franziska K. Fischer et al.

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I really like the fact that the authors have used rainfall radar data to map rainfall erosivity at the national scale. This is a great follow up from earlier papers that advocated the use of gridded rainfall estimates for this purpose, with the present ground-based radar data having a clear advantage over satellite-derived data in terms of their accuracy and spatial and temporal resolutions. As such, I much support its publication. Nonetheless, I do have a number of concerns regarding the methodology and also the write-up of the work, which at times is unclear and poorly structured. The main concerns are:

1. My main issue with the manuscript is its poor readability. Many statements are un- clear, often lacking precision; for example a reader sometimes needs to guess what data the authors refer to precisely. Further, I also note a mixing of results in the methods section, and a repetition of methods in the discussion section. I realize that my statement regarding the poor readability is rather general, but I try to specify as good as I can specific instances in the detailed comments section below. However, in general the feeling I obtained was that the authors should do one step back from their research and make their text more accessible by taking more a perspective of readers that are less familiar with what the authors did. Finally, also the alignment between section headings in methods and results could be better for easier understanding of what results belong to which methods.

We carefully edited the manuscript in order to improve its readability and to remove any ambiguity.

2. The main analyses that result in the erosivity map were performed using 1-hr radar data. This is partially justified by the authors due to the amount of data to be processed (P5L9-12). While data reduction can be an advantage for calculations, I still wonder though whether this is the best effort possible. Rainfall erosivity is by definition dictated by intensity and intensity is much better captured with 5-minute data. The reported advantage of the adjustment to rain gauge measurements could also hold for 5-minute data, i.e. it should be easy to re-assign the 1-hr adjusted data to the 5-minute intervals. This leaves me wondering if we would not get to better estimates if we take advantage of the 5-minute intervals. I agree that data storage and processing requirements will increase, but with a smart computer code it should not be too hard to calculate through 17 years of 5-minute data. While I do not necessarily request the

authors to change this now (although I would applaud it), I would at least expect a discussion as to whether future improvements of their map is possible, given also that they admit that "high intensity peaks" (P13L4) are very important for erosivity.

We fully agree that we would get better results for maps of PAST erosivity (= "hindcast" erosion modelling) if we would use 5-min data and we have shown this in Fischer et al. (2018). Usually such maps are useful for shorter time scales (event, year) for comparison with recorded erosion damages. Long-term average maps are usually applied for PLANNING (= "forecast" erosion modelling). In this case we do not need the exact location of a thunderstorm cell in the past but the general pattern that can be expected. This requires smoothing of the stochastic locations.

We added an extensive overview over the two types of R factor use and the smoothing that is inherent in present R maps.

3. I do not fully understand why the authors chose to present erosivity at the daily time scale, and this raises two questions.

This is a misunderstanding. We did not consider days (see below, your Point 3.B). Days were only considered for the seasonal EI distribution where it is necessary. In this case an event, even if it extended over several days, was assigned to that calendar date that was in the middle between the start and the end of the event.

A) Yes, we know that erosivity is stochastic so the scatter in Figure 5 is hardly a surprise. However, the main question eventually is how erosivity is combined with other factors (e.g. vegetation) to estimate erosivity. Arguably this could be at the daily time scale, but also weekly or monthly could provide sufficient temporal detail and as an additional advantage would have a smoothing effect in itself.

Yes, this is the basis for the crop stage period of the C factor. However, the crop stage periods between crops appear at different dates and current developments (RUSLE) allow for a continuous calculation of the soil loss ratio. A classification of the seasonal distribution (e.g. monthly means) will always decrease the accuracy of C factor calculations (except for the unlikely case that crop stages change only at the beginning of months) compared to daily values (note that these daily values are not daily events but the fraction of annual erosivity that on average can be expected to occur at a certain day).

B) Because the paper partially focuses on daily erosivity, I was curious to know how the authors dealt with night-time rain events. A storm can occur overnight thus belonging to two days. Nonetheless, as a single event, this should be treated accordingly as such when aggregating for a year. Any insight in this would help.

We did not distinguish between day-time and night-time but strictly followed the recommendations by Wischmeier. Often the events extended over midnight (because they often start in late afternoon and end in the early morning hours) or they extended even over several days. Our calculations did not use days but continuous temporal records over 17 years. We clarified this in the text.

4. Although I understand part of the reasons for smoothing, particularly for presenting average annual erosivity, I found little justification for the methods used in this study. Rather mechanistically it is described what is done, but reasons are mostly lacking. An example of this is P7L9-15: a sequence of three temporal filters are applied. I wonder whether a single carefully chosen filter would not suffice. The poor justification is also true for the procedure described in P5L20-22 on scaling: how were those parameters determined?

We added to the Introduction a better description of the twofold and contrasting applications of R maps and a description of the strong smoothing that has been unintentionally applied in previous maps. Furthermore, we replaced 'filtering' by 'smoothing' because filtering is often understood as removing certain (regular) frequencies while the occurrence of erosion events is stochastic.

To answer the question more specifically: An individual smoothing algorithm did not work (and cannot work). Even if one would exist, there would be no advantage for the reader or user of the data. The length of description of the method overemphasizes the importance of smoothing. The cumulative distribution function of the raw data correlates with the cumulative distribution function of the smoothed data with $r^2=0.9998$ (n=365). This information was added to the manuscript

5. I wonder why the authors' main interest seems to obtain a smooth average annual erosivity map. This leaves me wondering what they see as the main application of this map; the general statement in P1L25 (and P13L20) makes some sense but could be further elaborated. I would argue that if the interest is mostly (or partially also) in erosion monitoring, we may not need any smoothing at all, but rather we want to know when, where, and to what extent a surface is exposed to erosive rainfall. In this case we would not want to smooth out the stochastic nature of the erosivity, but rather retain it, because it offers important insights on erosion occurrence and could directly be combined with temporal vegetation assessments (e.g. from satellites). While I do not mean to necessarily promote own work, the discussions in https://doi.org/10.1016/j.gloplacha.2014.01.009 could be of interest in this regard (in which actually I also stated the potential interest of ground-based weather radars for the purpose of erosivity assessment!).

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6. Despite some of the comments above, Figure 5 is an important result in my view. I understand that this is an average within-year distribution for Germany, but I miss a discussion (and/or results) that show the seasonal distribution for sub-regions. Perhaps it would also be an idea to show monthly maps?

We added a sentence and a figure in the appendix to show the (non-existent) regional variation.

"There was no detectable difference in the seasonal variation between different regions in Germany (see Fig. A5 in the Appendix). The cumulative density functions of different regions correlated with at least $r^2 = 0.998$ ($n = 365$)."

7. The authors frequently refer to the R map of Sauerborn (1994). It would be helpful if this map could be provided (e.g. in the Appendix) using the same color scheme as used for the other maps.

We added it as Fig. A2

8. A trend in erosivity is proving through comparison with the older map, and (luckily) also comparing within existing rainfall stations. Because the authors also have a 17-yr series of erosivity, I wondered if that could also be a basis to say something about a (spatially-aggregated) time series for that period. Particularly as the authors report a large trend in the last few decades (P12L5).

We added to the discussion

A time series of 17 yr is regarded too short in meteorology for calculating temporal trends. The data in Sauerborn (1994) were derived from different periods for different states. If we calculate the state-wide mean R factors from her transfer functions relative to the state-wide mean R factors of the radar-derived map and plot this relative R factor against the mean year from which the state-specific data originated, a 23-yr long period can be covered by the means (Fig. 7; years < 1990; the total time period of individual years covers an even wider range, mostly about ± 5 yr around the mean year). During this period there was a slight but insignificant increase in erosivity with time. This increase smoothly leads over to the steeper increase in relative R for the radar-derived Germany-wide annual R factors if we express them again relative to the 17-yr mean (Fig. 7; years > 2000). Combining both data sets covers more than 60 years and yields a very highly significant regression ($r^2 = 0.6388$, $n = 28$). This regression indicates that at the end of the radar time series (2017) the R factor likely is already 20% higher than the values depicted in Fig. 2. There was no offset between both time series, which could indicate that high values obtained from the radar data are caused by the differing method. Rather, this time series again corroborates that the large increase in R is not a methodological artefact but due to accelerating climate change.

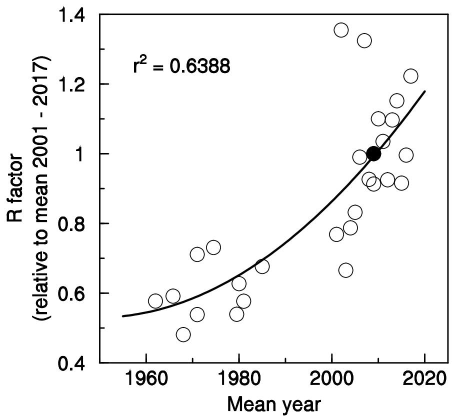

Fig. 7: Average R factor relative to the mean radar-derived R factor depending on the mean year of data origin. Data below year 1990 are calculated from state-wide averages determined from meteorological station records; year is the mean year of station records. Data above year 2000 are radar-derived R factors of entire Germany for individual years. The closed circle denotes the reference point (present map).

9. Station data are used in this study, but not to provide a direct validation of erosivity measures it seems. I would think that this could be a nice addition, i.e. to evaluate possible discrepancies between rain radar derived erosivity and station-derived erosivity.

Fig. 6 provides a direct comparison. A more detailed analysis (e.g., regional differences) seems not appropriate because the number of 33 stations and the unavoidable scatter in the data do not allow splitting the data. We also do not expect a large regional variation given the similarity between the new map and the old map (which we now provide in the Appendix). Finally, a comparison between station derived erosivities and radar derived erosivities has already been published for a large data set (115 stations and 19 944 events) by Fischer et al. (2018). In this publication, in which we neither intended to create a map nor to compare with the old map, we were not restricted to the 'Sauerborn' stations and thus could use 115 stations.

Detailed comments:

- P1L12: "for the first time". This seems incorrect as the authors also published work before that uses rain radar data to assess erosivity.

Rephrased

- P1L14: "extraordinarily large filtering"; this is a vague statement that needs rewording

We rephrased this in the Introduction and we added to the Discussion:

This pronounced stochasticity is due to the small size of convective rain cells. Just recently it has been shown by analysing radar derived rain pattern of the largest rainfall events that on average the rain amount is halved within a distance of only 2 km around the central point of a rain cell (Lochbihler et al., 2017). Given that rain amount is squared in the calculation of rain erosivity, the R factor decreases to one fourth within this distance. Larger areas can only be covered if there is movement of the rain cells. This small size of rain cells questions the use of rain gauges that only sparsely can cover space to derive rain erosivity. The inconsistent transfer functions among German states to derive erosivity from rainfall maps likely originated in the high stochasticity of rain gauge measurements under such conditions. It was only the unintended but unavoidable smoothing that was inherent in previous approaches that allowed deriving such maps. Radar technology enables us to replace this unintended smoothing by clearly defined statistical protocols and to quantify the effect of smoothing.

Lochbihler, K., Lenderink, G., Siebesma A.P.: The spatial extent of rainfall events and its relation to precipitation scaling, Geophys. Res. Lett., 44, 8629–8636 (2017). doi:10.1002/2017GL074857

- P1L15: "averaging 2001 to 2017" is not a precise statement. Probably the authors mean that the annual erosivity of 2001 to 2017 was averaged?

Changed as suggested

- P1L16 (and also L19/20): "the previous map" should be rephrased: "the erosivity map currently used in Germany, which is based on ... "

Changed as suggested; we introduced the name "Sauerborn map" in this place to replace "the erosivity map currently used in Germany" in the following occurrences.

- P1L20: unclear: do the authors mean to say that this is based on stations that were available in 1960-1980 and continue to report until present?

We rephrased the sentence to make it clearer: "This increase in erosivity was confirmed by long-term data from rain gauge stations that were used for the previous map and which are still in operation."

- P1L21: "by weather changes that may already be ... 1970s." Avoid emotional wordings like "dramatic"; rather state that "but by a change in climatic conditions".

Reworded

- P1L22: erosivity does not "fall"

Reworded

- P1L22-25: I have the feeling that this issue is a bit overstated: still the erosivity during winter months is rather small. I suppose that this requires a joint analysis with vegetation cover, which is outside the scope of this paper. Probably the main erosion on cropland would still occur during spring (in May farmers in NW-Germany have only just planted maize for example) and late summer when crops like wheat are harvested. Now it is not possible to state that "for many crops" we have "higher erosion" (P1L22) - P1L25: the "thus" and "definite" suggest a very strong causal relationship between previous statements and what is said here. I think that the authors should be more cautious; while an important input, the work cannot make definite conclusions about erosion yet.

We show (although we do this only in the discussion) that erosion under winter wheat (which is the most often grown crop in Germany) will be four-fold higher than previously expected. Practically all other crops (except clover-grass and except the system of "mulch tillage") also pass winter in a susceptible state (which is well known; we give the reference). We do not see any exaggeration or speculation in these statements.

- P2L4-6: this is a bit vague; applied and used by whom? Does this refer to Germany or more general?

We have expanded the description of how R factor maps were usually derived in the past

- P2L29-30: I would at least shortly acknowledge existing efforts to do the same with gridded data of satellite-derived rainfall estimates.

We included two references to this method (Vrieling et al., 2010, 2014)

- P3L10-16: I feel somewhat uncomfortable with the present tense of "expect" here, given that this article is a report of a work already completed. I suggest removal, but highlighting this in the results/discussion.

These are our hypotheses (and two out of three turned out to be wrong). We replaced "expect" and the present tense by "Our hypothesis was".

- P3L19-26: I wonder if there may be any effect of changes in the network/systems on the erosivity estimates?

We have used the RADKLIM data set that is the best estimate of precipitation based on radar- and gauge-data in Germany. A sophisticated quality control, the merging of different data sources, and a reprocessing applying one software tool lead to an at most homogenous data set, where the influence of network changes on precipitation estimates is eliminated or at least strongly reduced. However, a distinct improve in quality is detectable due to improved quality control of the raw data.

The changes in the measuring system over time were mainly intended to improve measurements where former locations had specific deficits that became apparent over time. We insofar expect the later measurement to be better, also because of the technical developments in radar technology. Furthermore, we expect that the change of the locations improves long-term averages compared to long-term averages with fixed locations because local deficits level out. These improvements have been documented for precipitation (mainly in internal reports). How much they improve erosivity calculation is unknown. An evaluation for erosivity would be rather tricky (because of the much larger variability of erosivity compared to precipitation) and of little general interest because this would mainly reflect local effects and it would only be applicable to the past. Restricting our data to the latest configuration would also not be an improvement because of the shorter time series and because the restrictions given by the latest configuration would then not be dampened by measurements with the older configuration.

We did not expand on this in the text because (i) there is too much speculation, (ii) this is a different topic that would distract from our topic

- P4L8: RW is an acronym for what?

RW is an abbreviation of **R**ADOLAN respective **R**ADKLIM and **W**eighted as it is a weighted sum of two products adjusted by different methods. We use RW only as a name but not as an acronym to indicate, which radar data were used. The data set is freely available and can be found by this name. Otherwise a lengthy explanation would become necessary. This explanation can be found in Winterrath et al. (2018), whom we cite.

Winterrath, T., Brendel, C., Hafer, M., Junghänel, T., Klameth, A., Lengfeld, K., Walawender, E., Weigl, E., Becker, A.: RADKLIM Version 2017.002: Reprocessed gauge-adjusted radar data, one-hour precipitation sums (RW) DOI: 10.5676/DWD/RADKLIM_RW_V2017.002, 2018.

- P4L25: other versions of this equation exist. See also: “van Dijk AIJM, LA Bruijnzeel, & CJ Rosewell (2002). Rainfall intensity-kinetic energy relationships: A critical literature appraisal. Journal of Hydrology 261: 1-23”. Why did the authors choose for this equation?

We added a justification:

“We used the equation by Wischmeier and Smith (1978) to calculate kinetic energy although several others have also been proposed (van Dijk et al., 2002) with none being superior (Wilken et al. 2018). Our choice retained comparability with the Sauerborn map. Furthermore van Dijk et al. (2002) had shown that kinetic energy as obtained by the Wischmeier-and-Smith equation did not deviate from measured kinetic energy in Belgium neighboring Germany.”

Another reason (not mentioned in the manuscript) is that the equation by Wischmeier and Smith (1978) is defined as standard among German authorities. Only recently this has been re-affirmed (DIN, 2017). An R map that is not based on the defined standard would not be used by German authorities.

DIN – Deutsches Institut für Normung: DIN 19708: 2005-02 Bodenbeschaffenheit – Ermittlung der Erosionsgefährdung von Böden durch Wasser mit Hilfe der ABAG. Beuth-Verlag, Berlin, 2017.

- P5L23: section 2.2 should be 2.3 (and also for next sections numbering should continue accordingly)

Error corrected

- P6L7/11: “neighbor” should read “neighboring”

Replaced

- P6L8: “krige” should read “kriging”

Replaced

- P6L11: “This ... pattern”. Sentence unclear.

We have expanded the sentence to better convey its message

- P6L31-32: this seems to be out of place, as it reports on results.

We added a reference to indicate that this is *a priori* knowledge (although the same was later found in our results)

- P7L12: “and the level shifts in the smooth”? I do not understand the sentence

This wording had been taken from the cited statistical reference. Now we reworded the sentence and used common language.

- P7L13: Hanning with capital H?

Changed as suggested

- P7L14-15: I think that after two reads I get the meaning, but could be formulated clearer.

Reworded and expanded

- P7L16-19: this seems to belong to results also.

We reworded this paragraph in order not to anticipate results.

- P7L26: if so, I would expect a clearer proof of this, e.g. by linking height from a DEM to erosivity, or at least show a DEM of Germany somewhere in the paper.

We added a detailed topographic map (Fig. A1 in the Appendix).

- P8, Section 3.2. I fail to clearly see a main message appearing from this section; what is the key lesson/result that the authors want to convey?

All existing erosivity maps (mostly unintendedly) employ pronounced smoothing. We explain this now in more detail in the Introduction. Due to our high data availability we were able to and had to replace the unintended and uncontrolled smoothing of existing maps by a statistical protocol because of the large small-scale spatial variability. This chapter is intended (i) to quantify the effects of this protocol, (ii) to justify the protocol and (iii) to highlight the large small-scale spatial variability that even exists for long-term averages. The third point is of particular interest because this large small-scale spatial variability of long-term averages was not known previously due to a lack in suitable data.

- P8L7: strange combination of present and past tense (smoothed). Specify that the 10-15km refer to this study.

Changed as recommended

- P8L7-9: the "disappearance" is quite logical from the description of winsorizing.

Yes; we just want to give a measure of the extent of the effect

- P8L11: "Rain erosivity from 5-min resolution data ..": it is unclear what erosivity this refers to: annual? Is this for 2011?

Reworded (and the caption of Fig. 3, to which this sentence refers, was improved)

- P8L15-16: probably this is described in methods but a bit unclear why 2011 and 2012 were chosen here.

The only reason is that this data set existed

- P9L32: "extreme" and "violent" sounds rather exaggerated. A more scientific formulation would be appreciated.

We followed the intensity classification by the UK Meteorological Office, which suggests using 'violent':

"For synoptic purposes, rain showers are classified as 'slight', 'moderate', 'heavy', or 'violent' for rates of accumulation of about 0 to 2 mm h^{-1}, 2 to 10 mm h^{-1}, 10 to 50 mm h^{-1}, or greater than 50 mm h^{-1}, respectively."

UK Met Office (2007) Fact Sheet No. 3: Water in the Atmosphere. p. 6. https://web.archive.org/web/20120114162401/http://www.metoffice.gov.uk/media/pdf/4/1/No._03_-_Water_in_the_Atmosphere.pdf

We explicitly refer to this source now.

- P10L17-18: the previous sentences compared radar-based erosivity with meteostation erosivity. Therefore I believe that this sentence makes little sense here, because there may in fact be differences because of the different data used. I more trust the fact that in P10L19 the same stations (and hopefully the same methods) were used. So please do not conclude when the previous statements do not support the conclusion yet.

There are two differences in the Sauerborn approach, first she used meteostation data and second she interpolated via a regression. In L 17-18 we exclude that the regression approach can be the reason for the difference and in L 19 we exclude that the difference in data origin (station/radar) can explain the difference. We hence need both sentences.

- P10L29-30: location is not so clear: could it be indicated in Figure 1?

We added "of the North Sea" to the sentence to make clear where the German Bight is and now we depict "German Bight" and "Baltic Sea" in the map in Fig. 1.

- P11L3-4: perhaps it is me, but I fail to fully grasp what regression was precisely done here (what against what), and with what purpose?

We reworded this sentence; furthermore we explain explicitly in the Introduction now the transfer of point R data to full maps via a regression with precipitation.

- P11L9: "trains" should be "rains"?

This was not a typo. We reworded this sentence ('tracks of thunderstorm movement') to avoid this impression.

- P11L21: see previous points also: I think that this "most pronounced changes" is overstated: the erosivity is still small.

"the most pronounced" is a relative expression; the change may be much smaller than "pronounced"; we left this wording

- P12L28-P13L2: this seems too much repetition of results to me.

We deleted this part

- P13L2: link between sentence ending with "resolutions" and sentence starting with "The pronounced" is not clear; this seems to be another topic.

- P13L3-4 I do not understand this: how can orographic rainfall increase hourly but not the peaks? And with peaks the authors refer to sub-hourly?

We deleted the sentences in L 2-4 because they were speculative anyhow

- P13L16: expected by whom? Rather "than what existing erosivity maps showed"

Reworded

- Captions should be self-explanatory: in Figure 1 I do not understand the "for a range of 128km AND the 2017 configuration". Please revise to make the caption clearer.

We are not sure what is unclear. We added '(utilized radius)' after range indicating that the radar beam does not end but signals of longer travel distances were not used and we replaced 'configuration' by 'tower locations'; note that the following parenthesis explains 'locations of some radar towers have changed over time'

- Figure 2 caption: "Erosivity map" specify in caption if this is annual average erosivity. Also units should be reported in caption.

Wording was changed and units were added.

- Figure 3: also here it should be clarified if we are looking at annual erosivity, and which years of data and why. Also why do we see a lag up to 50km? Would it make sense to make it longer? Why or why not?

Information was added

- Figure 5: caption should specify if daily erosion index (circles and blue) is from radar data.

Information was added

- Figure 6: caption should specify if Sauerborn used the exact same methods

There are likely several differences but these are not documented. E.g., at the time of Sauerborn paper, hyetographs were recorded while nowadays tipping-bucket rain gauges or weighing rain gauges are used. Sauerborn used a manual approach to identify erosive rains, their beginning, their ending and breakpoints of intensity while we used automated calculations. This was done by many and unrelated persons differing in their subjective decisions. We didn't add this because the points are many and their likely effects are unknown. The documentation is rather poor. Insofar, the new map is also a major step forward.

- Table 1: last two entries (1h winsorized and kriged) are also 17-yr? Specify. Also what years are used for annual/biannual?

We added '17-yr'

- Table A2 is probably not needed because Figure 5 is presented. If still kept, it should be organized differently so that Jan-Apr are on one page.

We left Table A2. The figure is easier for the reader (this is why it was inserted in the main body of text) while the Table is required for C factor calculations. We rearranged the table.

- Take care with wording of high/higher/low/lower: usually this refers to altitude, whereas other parameters/values are small or large.

We checked all high/low