

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 unclear, often lacking precision; for example a reader sometimes needs to guess what

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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.

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.

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

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temporal detail and as an additional advantage would have a smoothing effect in itself. 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.

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?

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!).

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

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it would also be an idea to show monthly maps?

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.

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).

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.

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.

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

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

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

- 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?

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

- P1L22: erosivity does not "fall"

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- 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.
- P2L4-6: this is a bit vague; applied and used by whom? Does this refer to Germany or more general?
- P2L29-30: I would at least shortly acknowledge existing efforts to do the same with gridded data of satellite-derived rainfall estimates.
- 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.
- P3L19-26: I wonder if there may be any effect of changes in the network/systems on the erosivity estimates?
- P4L8: RW is an acronym for what?
- 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?
- P5L23: section 2.2 should be 2.3 (and also for next sections numbering should continue accordingly)

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- P6L7/11: “neighbor” should read “neighboring”
- P6L8: “krige” should read “kriging”
- P6L11: “This . . . pattern”. Sentence unclear.
- P6L31-32: this seems to be out of place, as it reports on results.
- P7L12: “and the level shifts in the smooth”? I do not understand the sentence
- P7L13: Hanning with capital H?
- P7L14-15: I think that after two reads I get the meaning, but could be formulated clearer.
- P7L16-19: this seems to belong to results also.
- 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.
- 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?
- P8L7: strange combination of present and past tense (smoothed). Specify that the 10-15km refer to this study.
- P8L7-9: the “disappearance” is quite logical from the description of winsorizing.
- P8L11: “Rain erosivity from 5-min resolution data ..”: it is unclear what erosivity this refers to: annual? Is this for 2011?
- P8L15-16: probably this is described in methods but a bit unclear why 2011 and 2012 were chosen here.
- P9L32: “extreme” and “violent” sounds rather exaggerated. A more scientific formulation would be appreciated.
- P10L17-18: the previous sentences compared radar-based erosivity with meteo-

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station 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.

- P10L29-30: location is not so clear: could it be indicated in Figure 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?
- P11L9: "trains" should be "rains"?
- P11L21: see previous points also: I think that this "most pronounced changes" is overstated: the erosivity is still small.
- P12L28-P13L2: this seems too much repetition of results to me.
- 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?
- P13L16: expected by whom? Rather "than what existing erosivity maps showed"
- 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.
- Figure 2 caption: "Erosivity map" specify in caption if this is annual average erosivity. Also units should be reported in caption.
- 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?

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- Figure 5: caption should specify if daily erosion index (circles and blue) is from radar data.
- Figure 6: caption should specify if Sauerborn used the exact same methods
- Table 1: last two entries (1h winsorized and kriged) are also 17-yr? Specify. Also what years are used for annual/biannual?
- 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.
- Take care with wording of high/higher/low/lower: usually this refers to altitude, whereas other parameters/values are small or large.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2018-504>, 2018.

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