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Interactive Comment

Interactive comment on "Rainfall erosivity in subtropical catchments and implications for erosion and particle-bound contaminant transfer: a case-study of the Fukushima region" *by* J. P. Laceby et al.

Anonymous Referee #3

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Laceby et al. establish RUSLE rainfall erosivities ("R factor") for the Fukushima region, which is not new as there are already earlier studies covering this area. However, Laceby et al. do this under a misleading title: "Rainfall erosivity in subtropical catchments and implications for erosion and particle-bound contaminant transfer: a case study of the Fukushima region". This title is misleading because neither the special situation of subtropical areas is considered nor are there any data on particle-bound contaminant transfer.

The wording ‘in subtropical areas’ lets the reader expect to learn how erosivity can be calculated in subtropical areas, e.g. whether different drop size distributions and hence differences in kinetic energy have to be considered in subtropical areas. However, this is not treated at all in the manuscript although the authors emphasize in several places the special situation of typhoons. But, they use equations that were developed in temperate climates without any examination of their validity. Hence at least, subtropical areas’ should be replaced by ‘Mid Japan’ or "Fukushima region" although this still does not prove the validity of the approach.

Also the reference to nuclear contaminant transport should be eliminated together with all parts of the manuscript that relate to this (e.g. the first two paragraphs of the introduction). It is pure speculation by the authors that the R factor (and the entire RUSLE) can be used to calculate contaminant transport after a nuclear disaster. The entire manuscript does not contain any attempt to prove this speculation. There are not even any radioactivity measurements included and thus the title is greatly misleading potential readers. In fact, the experience with the Chernobyl event has shown that the R factor (and the entire RUSLE) is a poor instrument for prediction of radioactivity transport because two main preconditions are not met during nuclear disasters (in contrast to the transport of nuclear weapon fallout). 1) The nuclear contaminants have to be bound to the soil matrix which takes time. During the first rain dissolved transport is still important. This is why – in contrast to the expectations, some material from the Chernobyl event quickly reached great depth (80+ cm) and was found below the global fallout. It had moved together with macropore flow. 2) The nuclear contaminants have to be mixed evenly into the top soil by tillage. This is not the case directly after a disaster but the contaminants are concentrated at the very surface. Hence a small erosive event that erodes the very surface would be sufficient to remove the contaminants and a larger event removing more soil would not remove significantly more. Both arguments let us expect that the correlation between rain erosivity (especially long-term erosivity!) and contaminant transport after a nuclear disaster to be poor.

Further, the referencing of the manuscript is more like lottery. Usually, references should give the first publication establishing the knowledge or expanding it significantly or giving a review over the state of the art. In contrast, the authors often cite rather insignificant articles or even articles that do not examine at all the statement after which they are placed. They may contain a certain statement in a subordinate clause but then this should be cited as opinion statement but not as if it had been proven in the reference.

The material and methods section is imprecise (see Details for more aspects) and poorly written. It is hence not possible to judge whether the applied methods are justified and correctly applied or not. Considerable doubts exist. The MAE is not sufficient to quantify the error for two reasons. First it does not sufficiently consider if the degree of deviation differs between individual data points. The RMSE is more appropriate to represent model performance than the MAE when the error distribution is expected to be Gaussian. Second, the MAE only gives the 50% deviation, while usually the 95% interval of confidence is needed (which will be much larger than the MAE; I guess in this case at least by a factor of 5; note that the RMSE can be used with the t table to calculate the 95% interval of confidence while MAE cannot be used for this). The MAE thus provides a wrong impression on the accuracy of the predictions. In several months the MAE for individual stations is in the same order of magnitude as the standard deviation among all stations. Hence the best prediction would be to just use the average over all station because this would not lead to worse predictions and follow the rule of parsimony.

Surprisingly, the authors even give wrong units to the R factor. The unit of erosivity of the long-term R-factor is MJ mm ha^{-1} h^{-1} yr^{-1} (or conversions of this) while the unit of event erosivity is MJ mm ha^{-1} h^{-1}.

The discussion mainly contains speculations about radiotracer transport but there is little discussion about the own findings, the quality of the data, the strange regression that lack physical meaning, the reasons for the deviations to other publications about

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the R factor in the same area. This deficiency is not surprising, because the hypothesis of the authors (radiotracer transport after nuclear accidents can be predicted with the RUSLE) cannot be examined with the approach and the data used by the authors. Hence there is no hypothesis about what they really quantified that could be discussed.

The manuscript should be language checked by a native speaker.

Details:

Page 7225

The author list contains several names that did not contribute to this manuscript. They must be deleted.(see p 7246)

Page 7227

Line 2-5: delete; they are not related to the study presented in this manuscript

Line 10: As mentioned on page 7231, line 24ff, also 10 min data were used although not from all 60 stations.

Line 11: The time period of the dataset should be mentioned there.

Line 14: The unit of long-term erosivity is MJ mm ha^{-1} h^{-1} yr^{-1}

Line 16/ 17: Replace the wording “evolves positively” by “increases” to simplify the sentence or moreover change the sentence to: “In July and August, the most erosive months of the year, rainfall erosivity increased from the North to the South of the region while in the rest of the year, this gradient occurs from northwest to southeast.”

Line 19: Is the typhoon season in July and August?

Line 19 ff: The sentence should be reorganized, the important part of the sentence in the beginning and additional information in the end.

Line 22 to 25: Delete; pure speculation; nothing was studied regarding radiocesium transfer or the impact of typhoons.

Page 7228

Line 2 to 14: Delete

Line 18: Primary literature is missing. Where is the justification for these two references?

Line 19: Rather replace “fundamental to determining” by “fundamental to determine”.

Line 23: improper reference

Line 25ff: The sentences should be revised to achieve higher precision and less repetitions.

Line 28: Where is the justification to cite Lee and Heo 2011 and Lu and Yi 2002.

Page 7229

Line 1-3: The proportional relationship between soil loss and the R factor could be rather shifted to line 12-14 which refer to the high correlation of the R factor and soil loss.

Line 5: The wording “storm erosion values” is undefined and imprecise.

Line 5: Diadoto and Bellocchi did not find this; wrong citation

Line 8/ 9: The meaning of “To incorporate cyclical rainfall variations” is not clear. Intra-annual variations (seasonally) or inter-annual variations? The wording should be revised. In general, the authors often refer to temporal variation but it is not clear to which scale they refer.

Line 10: Meaning of the sentences is not clear. Better predictor than?

Line 17 – 18: It is not correct to write that the USLE was designed for the standard plot. The standard plot was used to gather a large part of the underlying data.

Line 22: Rephrase. In the field of erosion modelling, the term “erodibility” is reserved to the K factor of the USLE while this sentence refers to the calculation of the C factor

Line 25: Rather replace “to understanding” by “to understand”.

Line 26: Why is the R factor particularly important in subtropical catchments with cyclonic activity?

Page 7230

Line 14: Rather change “progressing westward” to “to the west” to simplify the sentence.

Line 18-20: Is the comparison to the fallout event in Chernobyl necessary to fulfil the aim of the study? What can I gain by this comparison?

Line 21-24: The period of occurrence of typhoons should be better mentioned earlier when it is in the focus of this study. But, where is the special consideration of the typhoons. Do they have the same drop size distribution and hence kinetic energy like temperate rains?

Page 7231

The chapter about the rainfall monitoring stations (2.2) is written confusingly. It should be exclusively focused on stations which were included in the calculation and interpolation of the R factor map and, too many details should be avoided. Line 5-9, e.g., could be simplified to “Fourteen stations were omitted within the 100 km radius of the DFNPP due to an operation period of less than 5 years or operation pause during winter months.”

Line 8: “19 years were available”

Line 11: “were operated”; I wonder whether this is really true. Does Japan have a Meteorological Data Acquisition System since more than 100 years?

Line 16: Rather replace “multiple years” by “several years”.

Line 17: It is not clear what “live” stations should mean.

Line 20: Change the structure of the sentence for more accuracy: “For the long-term stations, a minimum period of 60 years and a maximum period of 126 years was available.”

Line 22: Change the sentence to “data from the long-term stations were additionally analysed”

Line 24: Why “on average”? What do you mean with “consistently”? Continuously?

Page 7232

Line 6: replace for by from

Line 9/ 10: The use of the abbreviation KE and E should be revised. In the context of erosivity, the abbreviation E is always used for kinetic energy and not for ‘event’ as the term EI stands for ‘energy-times-intensity’ (Wischmeier & Smith, 1978).

Line 16: The unit of er must be corrected to MJ ha^{-1} mm^{-1}.

Line 18/ 19: The “unit of rainfall energy” cannot be calculated. You calculated the “rainfall energy per unit depth of rainfall”.

Page 7233

Line 1: this is not completely correct. It only applies for intervals r of constant ir. Rephrase. Is the assumption of constant ir justified?

Line 3/ 4: The abbreviation mj was already used for the number of erosive rainfall events in year j (page 7232, line 13). This definition is in contrast to the usage of the number of erosive rainfall events which were summed up for different periods like a month.

Line 5 ff: The difference between argument 1) and 4) is not clear.

Line 11/12: This sentence is repeated similarly in line 15 and 16. The paragraph from

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line 11 to line 16 could be shortened.

Line 17/ 18: Why don't you use the term 'precipitation' instead of 'rainfall' in the following when it is the correct expression?

Line 25: The use of a DEM with 10 m resolution is meteorological nonsense because 10 m cannot influence a cloud (otherwise a house would influence rain). Furthermore, orographic rainfall is not influenced by the altitude of a location but by the altitude in some distance (which forces the uplift of clouds). Finally it depends whether an area is in luv or lee position. The same elevation may thus have more or less rainfall than average. Elevation as such is hence a poor predictor of rainfall. See for instance Daly, C., Gibson, W. P., Taylor, G. H., Johnson, G. L., and Pasteris, P.: A knowledge-based approach to the statistical mapping of climate, Climate Res., 22, 99–113, 2002

Page 7234

Line 21: Rather replace "month by month" with "monthly".

Line 21/ 22: You don't have to repeat the area of research as you already mentioned it in the beginning of the paragraph (page 7233, line 21). Otherwise the reader is wondering whether there was another area for which another technique was used.

Line 22-24: What does it mean, "the GAM technique is more straightforward and flexible"? What means "straightforward", "flexible" and "more. . .in comparison to the other approaches"? The whole GAM procedure should be explained in a way that it can be understood by the reader. The references (e.g. Wood 2001) are clearly not sufficient because they do not write how you applied the GAM procedure. Line 25: The abbreviation GAM was already introduced, so it should be applied.

Line 25: Did you use the mean value for each month and station or did you use the monthly value of each year at each station? This must be more precise.

Line 27: Rather replace "from the dataset because its location" by "from the dataset due to its location". This is a strange argument anyhow. Why is it better to extrapolate than to have a station at the boundary of the research area? Statistically it would even be preferable to have stations outside the target area. Either give a better justification or include this station in your statistical analysis.

Line 28: Is there no larger DEM layer available so that the southernmost station could be included?

Page 7234, Line 26 to page 7235, line 7: Here, you refer again back to the stations which were included in the analysis. There was already a separate chapter about the stations (2.2. Rainfall monitoring stations). Maybe this can be included in chapter 2.2.

Page 7235

Line 7: To estimate the quality of each model, the AIC (Akaike information criterion) should be used as the number of explanatory variables is large and the number of stations is relatively low.

Line 7 to 11: not clear at all (e.g., it is not whether you multiplied g by μ because in other cases the multiplication sign was omitted)

Line 15: Is “fitted by penalized Maximum Likelihood” the general wording for this?

Line 24 + 25: Were ME and MAE calculated with the log data or with the original data?

Page 7236

Line 3, first sentence: What a surprise! (sorry for the sarcasm but don’t you have anything more interesting and more precise to tell?)

Line 6 to 8: this is clearly a caption. Isn’t there anything to tell about the data themselves? Why do you present them then?

Line 7: Does the “rank order” in Fig. 2 make sense? Maybe an order from north to south would be more profitable. Moreover, it would be favourable when the order in Fig. 2A is the same as in Fig. 2B to allow a comparison of annual precipitation and

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EI30 for each station.

Line 8: a new paragraph would be helpful because the topic changes.

Line 9: It is little confusing when you refer first to the mean annual rainfall, than to the daily rainfall and then again to the mean annual rainfall. It might be easier comprehensible to shift the information about the variation of daily rainfall to the end of the paragraph where you refer to the variation of the annual rainfall (after line 15)

Line 10: you said this already

Line 11 and following: “temporal” is not the appropriate term

Line 13: Do you mean “coefficient of variation” instead of “coefficient of variance”?

Line 16: Sentence is nonsense (e.g. period are always temporal; what is “longer”? Was there a period without interannual variation? I hardly can believe this.

Line 20-23: sentence not clear

Line 24: What a surprise.

Line 25: Delete “into” in the part of the sentence “during the summer months and early into fall”.

Line 26: where can the periodic typhoons be seen in Fig. 5?

Line 26 - 28: Connect both sentences to one sentence and use only rainfall OR precipitation when the same is expressed: “In fact, 60 % of the rainfall occurred between June and October, compared to only 17 % between November and February and 22 % in spring (March-May).

Page 7237

Line 11: The maximum of annual maximum daily rainfall was already mentioned in line 7/ 8. Therefore this paragraph should be shortened.

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Line 15: Snowfall has no influence on R (only snowmelt); be more precise. I do not know what you really mean.

Line 14 – 23: This paragraph should be shortened to the important information, e.g. “All stations were used as the influence of snow was lower than. . .”.

Line 25: The right unit of annual erosivity is MJ mm ha^{-1} h^{-1} y^{-1}.

Line 28: The link to figures should be more exactly; here: Fig. 2B.

Line 26-28: this is caption not Results

Page 7238

Line 3-5: Is this part still corresponding to Fig. 2B? There, 20 stations are plotting above one standard deviation of the rainfall erosivity mean and no station plots below. This would also have consequences up to line 7.

Line 6: you emphasize the 7% as if they would be significant. Have you tested this. If not, delete the statement because it is just random variation

Line 10/ 11: It would be better when the mean average annual R factor is numerically mentioned here.

Line 17: I cannot see the typhoon season

Line 19: Were the 16% tested? Are they significant? If not, delete the statement.

Line 26: Better “similar distributions as the observed values”?

Page 7239

Line 3/ 4: The percent values here don’t match with the values in Table 3.

Line 6/ 7: Table 3 shows that ME for models depicting R factors are close to zero from October to April.

Line 5 to 14: The complete paragraph has to be adjusted with Table 3. Moreover the

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abbreviations ME and MAE should be explained in the table captions again.

Line 16: Is the “backward stepwise procedure” well-known in general?

Line 19: Is this correlation significant? The coefficient of correlation is missing.

Line 20: Where is the similar spatial distribution shown in Fig. 7a?

Line 20/ 21: Are these gradients significant?

Line 20-26: How can you refer to a positive relation of annual R factor and elevation when the modelling error is most pronounced in areas with gradients in elevation?

Page 7240

Line 1: what do you mean with “direction evolves”? Second sentence seems also to be wrong.

Line 2: What is "it"? The previous sentence used plural.

Line 8: is this likely? Why should elevation have an influence in one month but not in another? This is very likely a falsely significant variable due to multiple testing (did you control for this?). The models look over-parameterized. Have you really calculated AIC?

Where is the mechanistic justification for elevation being important only in February. A similar argument holds for “northness” (the correct term is northing!) and aspect.

How could slope in a 10 m DEM (or 40 m DEM) have an influence on precipitation or R? Do you think a lynchet (or any similar small and steep landscape feature) receives a different rainfall than the fields, which it separates? Extremely unlikely.

Furthermore, rain gauges have to be oriented horizontally. Hence the gauge never has a slope. Even if the site has some slope, always sites with little slope are selected for meteorological stations. In consequence, your data cover only a range in slopes that is much smaller than the regional range. This means that you apply your regression far outside the range for which it was developed. This is not allowed. In fact large parts of your maps would have to be white, because they exceed with some parameter the range covered by your original data.

Line 26: It would be helpful to mention the months of typhoon season again.

Page 7241

Line 17-26: What benefit do we earn by the comparison to the fallout event in Chernobyl? How can this be used to fulfil the requirement of the study?

Page 7242

Line 15/ 16: Why is the R factor of Yoshimura et al. (2015) so much higher than in your study? Your cannot leave the reader with this discrepancy.

Line 24: When there are already two studies on R in the Fukushima region, where is the novelty of your study?

Line 26: Now I am wondering what is the difference between the study of Shiono et al. (2013) and your study?

Page 7243

Line 5-9: This paragraph could be shortened.

Line 10/ 11: Are Mexico and Peninsular Malaysia also in the typhoon region?

Line 17: Why is the Ukraine relevant?

Line 21: Very unlikely that you have similar soils in the Ukraine and in Fukushima. The same applies for all other factors. There is no base for you speculation. (Please note that the K factor of Wischmeier et al. 1971 is not valid for andic soils, which likely occur in the Fukushima region; this may be a major obstacle in your further work)

Line 25 to page 7244, line 7: The chapter 4.3 (Typhoons) is exclusively a discussion of literature without any reference to your own results. Delete

Page 7244

Line 9: what are “higher volumes”?

Line 14: this argument is nonsense. This would only hold if you would have measured snow depth.

Line 16: which general relationship?

Line 17: which ratio? What is “it”?

Line 19: Are you mixing snow depth with water equivalent depth? This is nonsense. Delete.

Page 7245

Line 21: Wrong unit of annual erosivity, it should be MJ mm ha^{-1} h^{-1} yr^{-1}.

Line 24: this conclusion is clearly unjustified because you speculate about R in Chernobyl.

References

Many references have little relation to the study itself and should be deleted. 50% will be sufficient.

Tables

Table 1: What is start and finish? The unit of rainfall should be mm/yr as used in the text.

Table 2: What is temp (temperature?)? How can you know the missing data of erosive events? If there was not measurement, you do not know whether you missed one. KE should have yr in the unit. Is EI30 for event (as the unit suggests) or for the year (as the numbers suggest)

Table 3: poor wording. Rephrase.

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Table 4: highly unlikely. Show proof.

Table 5: not necessary; These odd numbers are nonsense anyhow for a classification. Also the reference is missing!

Table A1: a 40 m resolution is clearly overkill. There is no justification for inflating 40 numbers to >50 Mbyte. The accuracy of the whole approach by far does not justify this and there is also no physical justification given, how the long-term average R factor could change within 40 m. The column head must be explained and appropriate units must be given.

Figures

Fig. 1: Lettering is too small. This cannot be read. According to the text there were more stations omitted than those shown in the map. Which information is correct?

Fig. 2: I cannot see more than in Table 1. This is redundant. Delete

Fig. 4: How can you calculate a running average for the last year? Aren't the running averages centred? This does not make any sense. The figure would be easier to read (and data representation would be more correct) if the columns would be replaced by markers.

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