

Response to Reviewers (Round 2)

Reviewer 1

R1-General Comment: This is an interesting study as it combines the Rainfall erosivity with the effects of nuclear accident in Fukushima. It is a smart idea to combine the rainfall erosivity with other indicators or threats (e.g contamination). However, there are a number of issues that authors should address before gets published.

R1-General Response: Thank you for your comments and taking the time to make the suggestions outlined below.

R1-C1: Authors should somehow underline in the conclusions that Rainfall erosivity is an important indicator to determine the climatic vulnerability of a region (also from soil loss point of view) and future power plants design (or even maintenance) should take into account this indicator.

R1-R1: We added a sentence at the beginning of the conclusion indicating the importance of rainfall erosivity as an indicator of climatic vulnerability of a region to rainfall-driven soil erosion. Although we may agree with the reviewer about the power plant design statement we do not have sufficient knowledge on power plant design and impacts of rainfall on power plant design to add this information to our manuscript.

R1-C2: L66-67: Rainfall erosivity takes also into account rainfall amount and rainfall frequency (plus the duration and intensity which have been underlined by authors).

R1-R2: L65-66: Rainfall amount and frequency have been added.

R1-C3: L76-L78: “global application”: The study of Oliveira was done at national level (Brazil) and the study of Fiener at local level (in a province of Germany). The study of Vrieling was done with a different methodology than the one proposed by authors. Instead, at continental scale, authors should include the recent study of rainfall erosivity in Europe (Panagos et al 2015). This was done with the same methodology as here and was included in the first manuscript (while it was surprisingly deleted in this revised version).

R1-R3: L74-75: The previous reviewers requested that we removed several citations from the manuscript and the Panagos et al., 2015 was one of them (our apologies). To address this comment we added a reference to Moore 1979 to replace the reference of Vrieling, removed the Fiener reference, and added the requested Panagos et al., 2015 reference.

R1-C4: Equations 2: the 0 on the top is a mistake. Please revise it.

R1-R4: The 0 has been updated in Equation 2.

R1-C5: The impact of snow in R-factor: This is true also in European applications. See the insignificant impact of snow in R-factor estimation in Switzerland (Meusburger et al., 2012).

R1-R5: We have now cited Meusburger et al., 2012 on line 158 in the methods.

R1-C6: 2.4 title to be changed as “*R-factor spatial interpolation*”

R1-R6: 2.4 Title: This has been updated.

R1-C7: The use of WorldClim datasets as covariates for the rainfall erosivity is very similar to the application in Europe (Panagos et al., 2015) which was neglected by authors. Moreover, authors should be more precise regarding the temperature: Mean? Maximum? Minimum?

R1-R7: This wasn't neglected as we completed preliminary maps with this technique before the Panagos et al., 2015 paper was published. Nonetheless on L186 we added the Panagos citation. Regarding the temperature we originally didn't want to repeat mean twice in the sentence. To address this comment we added mean in front of both precipitation and temperature L187.

R1-C8: Congratulations for the use of GAM which is also recommended in spatial interpolation on a monthly basis.

R1-R8: Thank you

R1-C9: L229-238: Is the annual the sum of monthly? If not, which is the difference if you sum your 12 monthly layers compared to the total erosivity (modelled per se). Did you impose any restrictions in order to have the best sum? What you have done with outliers? Moreover, authors should discuss if the prediction at monthly temporal resolution has higher uncertainty than the prediction of annual R-factor

R1-R9: The annual map is derived from the annual mean R-factor values (as per lines 152-153). In response to this comment we checked and the the difference between summing the monthly layers and the annual erosivity was less than the both the summed monthly model error and the annual erosivity error. We did not impose restrictions to have the best sum as this may bias the results. Similarly, we included all the rainfall stations (i.e. no outliers were excluded) again to not bias the results or manipulate the dataset in order to improve the model fit. The differences in model predictions are discussed on lines 317-323 with an additional line added to emphasize the monthly and annual model predictive differences.

R1-C10: In section 3.1 and 4.1 you could even refer more (in text) in figure 4. This is not so much analyzed in the text.

R1-R10: In section 3.1 there are currently 7 lines discussion figure 4 (L268-274). A reference to figure 4 has been added in the discussion along with an extra sentence linking our findings to the literature (L382-384)

R1-C11: L280-282: “highest mean annual maximum daily precipitation” is not a term. I understand the meaning but it is not an appropriate term. On L283, delete ‘average’.

R1-R11: L281-282: We edited the wording to adjust the language and address this comment. We also removed ‘average’ from L284

R1-C12: 3.2 Header: Delete “Spatial”. The sub-chapter refers to temporal variability.

R1-R12: Sub-chapter 3.1 refers to precipitation, and sub-chapter 3.2 refers to rainfall erosivity. We did remove the spatial and temporal variability from these section headings and simply refer to 3.1 now as precipitation and 3.2 as rainfall erosivity.

R1-C13: Sections 3.2 and section 3.3 should be distinguished easier. Find a better title for 3.3

R1-R13: Section 3.3 was renamed: The Spatial Distribution of Rainfall Erosivity

R1-C14: L303: “deviance” is not appropriate term. I would propose “variance”

R1-R14: L306: Deviance was been changed to variance.

R1-C15: 3.3 Section and relevant table 2: Authors should discuss the fact that R_2 is lower in the most erosive months where you have high un-predictable rainstorms. The prediction is much more difficult in months with very few–high erosive events

R1-R15: L320-323 This is now discussed.

R1-C16: L356: The name of Typhoons (or can you please define a Typhon for this region)?

R1-R16: The name typhoon has been updated to tropical cyclone throughout the manuscript. Further the definition of typhoons and tropical cyclones can be found on the lines 170-174.

R1-C17: Section 4.2 and L385-L386: The sentence is very generic, you can specify it regarding spatio-temporal variability of R-factor. Since your article has been submitted in 2015, you are not aware of the 2 studies which have published spatial and temporal rainfall erosivity at national scale in Greece (Panagos et al, 2016, Catena) and Italy (Borrelli et al., 2016, Int. Journal of Digital Earth).

R1-R17: L393: We added the word similarly to indicate the spatial-temporal variation. Thank you for highlighting these studies we were aware of one of the two studies and look forward to reading the other one.

R1-C18: section 4.3: there are more studies that should be cited as relevant to the influence of Typhons in rainfall erosivity: Brazil (Oliveira et al., 2012, Catena) and Colombian andes (Hoyos et al., 2005, Journal of Hydrology) .

R1-R18: The recommended Oliveira et. al. (2002) notes that the tropical areas in Brazil receive more rainfall from convection events compared to the temperate areas in Brazil that have mid-latitude cyclones (with a citation to Hoyos et al. (2005)). After examining cumulative tropical cyclone paths in these regions we decided not to include these two references as this region differs from the region we are comparing in text.

R1-C19: Section 4.4 is very interesting. The R-factor in Ukraine provided by Larionov (1993) is following the same methodology and same temporal resolution like yours?

R1-R19: The R-factor for Ukraine was from a major project for the Great Russian Plains. The methodology does not use the same GAM procedure though the R-factor values are derived following the Wischmeier and Smith (1978) approach.

R1-C20: Minor issues that should also be addresses:

- Results:

- Discussion:

Please delete the “:”

R1-R20: The “:” were removed from these two sections, and the method heading as well.

R1-C21: L326 ΔAIC : please explain in the text what is about? I have seen in the table but not in the text

R1-R21: L249 ΔAIC is now explained more clearly in text in the methods.

R1-C22: L344 “easternmost “ . I suppose it is a grammatical mistake.

R1-R22: L350: We believe this is accurate.

R1-C23: L401: JMA. Please specify...

R1-R23: JMA was specified on line 131.

R1-C24: Table 1: Attention to the MJ mm-1 ha-1 h-1 yr-1. It should be MJ mm ha-1 h-1 yr-1

R1-R24: Table 1: This has been updated – thank you.

R1-C25: Table 2: Deviance is not appropriate term.

R1-R25: Table 2: Deviance was changed to variance

R1-C26: Fig. 1: What the stations 47595, 47592...represent? For FDNPP , please use a different more visible symbol.

R1-R26: Fig 1: The symbol for the FDNPP has changed from a circle to a square. The long-term stations with the 47XXX series numbers are listed in the legend as the long-term stations which are discussed in the first paragraph of section 2.2.

R1-C27: Figure 8: MJ/mm/ha/h/month is not appropriate in the legend. Please correct.

R1-R27: The units of erosivity have been updated in this figure.

R1-C28: Figure 8: Cs activities is not appropriate (in the caption)

R1-R28: The unit of Cs activities has been updated in this figure and the others for consistency.

Reviewer 2

R1-General Comment: The manuscript has been greatly improved but some improvements can still be suggested. The authors use rather advanced methods of statistics but clearly do not understand the basic concepts of statistics (standard deviation, population and sample, autocorrelation; see L 27, 237, 258 below). This leaves at least some doubt whether the statistical analysis has been correctly applied.

R1-General Response:

Thank-you for your comments - incorrect symbols were mistakenly incorporated into the revised manuscript (which have been updated). We removed some statements describing rainfall erosivity with summary statistics and we have also triple checked the statistical analyses to ensure that they have been applied correctly.

R2-C1: L27: Sigma is used as symbol for the standard deviation of the population. The authors clearly do not know the population but just have a sample. The symbol then is s in equations while SD is the accepted abbreviation in plain text. Hence either use s or preferably use SD but sigma is wrong (the same applies for μ , which also is used later on). This will also avoid present inconsistencies because later the authors switch from sigma to S.D.

R2-R1: All standard deviation calculations were based on our data being a sample and our apologies for using the symbol implying we used standard deviations based on the entire population. This is a mistake in symbols that does not relate to the statistical analyses in the manuscript.

R2-C2: L39: I wonder why the authors switch between typhoon and cyclone in some places. Although both are the same weather phenomenon, “cyclones” occur in the South Pacific and Indian Ocean but not near Japan.

R2-R2: There was one reference to cyclone in text (Old version of the manuscript L432 – updated version now L438) which should have read tropical cyclones (this has been updated to tropical cyclone in text). There was another place where it states cyclonic at the end of the abstract (L38) which has been updated to state tropical cyclones. In fact all references to typhoons have been updated to tropical cyclones with a definition of tropical cyclones added to lines 170-174 as per the response to reviewer 1.

R2-C3: L 54: Fixated is not the correct technical term. Better use “adsorbed” because Cs can be exchanged by other cations like K. If it would be fixated, plants could not take it up and we would not have to worry about it.

R2-R3: L53 Although fixated and adsorbed are both commonly used in the radiocesium literature, we have changed fixated to adsorbed.

R2-C4: L 66: Event duration is not a criterion of the R factor. The way it is put here, it can be misleading to readers and the authors should be especially precise in this case, because this is the center of their topic.

R2-R4: L65-66: We removed the event duration from this sentence.

R2-C5: L 72: the references do not fit to the sentence because they neither prove “around the world” nor do most of them correlate the R factor with measured soil loss. Either change the sentence or find appropriate references.

R2-R5: We removed this sentence in its entirety.

R2-C6:L 112: Shouldn't the unit be Bq/m² instead of Bq/kg. The spatial pollution should have space as unit because no mass can be associated with space (or you would have at least to define a depth and a density)

R2-R6: As the radiocesium has already been found below the soil surface in the Fukushima region, conceptualizing the spatial distribution of radiocesium in Bq/kg maybe helpful for modelling lateral fluxes and migration of radiocesium. This radiocesium layer is published elsewhere and referenced in text with the depth and density provided in this reference. As our current paper is on rainfall erosivity, we believe the readers can obtain further information on these conversions and the map in the reference provided in the Figure 1 caption.

R2-C7:L 115: Is the climate based on the classification or is rather the classification based on the climate?

R2-R7: L115 We removed the wording "based on these" classifications.

R2-C8:L 126: Radius is misplaced

R2-R8: L124 Thank-you we have moved radius up in the sentence to its proper position.

R2-C9:L 130: Blank after 115 is missing; comparable to what?

R2-R9: A space was added after 115 – and we have changed comparable to similar on L128 to indicate that these stations had similar data available.

R2-C10:L 131: What are other analyses? No analyses were mentioned yet.

R2-R10: L129: Other analyses were changed to R-factor and daily rainfall calculations.

R2-C11:L 132: Sentence not clear. I read: you exclude long-term stations because of their short measuring period??

R2-R11: L129: With the change from other analyses to R-factor calculations above we believe the sentence is now clear: "The long-term stations were excluded from the R-factor and daily rainfall calculations owing to high levels of missing data (μ 33% daily) and short temporal periods (7 years – 10-minute)."

R2-C12:L 133: Isn't there any better data? Usually much better data are available at meteorological authorities that those made available to the public (for several reasons)

R2-R12: Originally, colleagues were interested in rainfall data, (10 minute and daily) for only a select number of sites in the region and for a short time period and thus we downloaded the data. Then as the research in the region progressed it became very clear that the rainfall regime is fundamental to radiocesium transfers. Accordingly, our project expanded and we became more interested in the spatial and temporal (both intra and inter annual) variability in rainfall and rainfall erosivity, so we downloaded a broader set of data that we used for this analyses. Sure, this data may have been available from the JMA. It should be noted that this type of data is not readily available to the public or research agencies

everywhere. Finally, the data was downloaded from all the stations available in the research region for the time period we were interested in.

R2-C13:L 135: not clear. Rain always has breakpoints (like the start of rain). I guess that something different was meant but guessing is not part of the game.

R2-R13: L133 Breakpoint is a common term in climate data homogenization research. Further a reference was provided to remove any guessing required by the readership. We did remove the space between break and point to use the term breakpoint.

R2-C14:L 150: unit energy is not the correct term. You use the correct description anyhow in L 152. Move it to L 150. This also avoids the mistake of defining a symbol twice and with different definitions.

R2-R14: L148 This has been moved – thank you.

R2-C15:L 166 to 169: This doesn't sound like reliable sources. I would expect JMA to be reliable but if you additionally need Google news even JMA seems not to be reliable. The whole typhoon story a weak already from the beginning.

R2-R15: L163-170: We believe that this is an effective approach to cross-referencing our rainfall dataset to news and information sources available, including the peer-reviewed and referenced Wikipedia. To address this comment we added references to this section and modified the text. Further it should be noted that the Wikipedia Pacific Typhoon Climatology database is exceptional and contains a vast amount of pertinent information. For example, please see the peer reviewed 2015 Pacific Season Wikipedia Page (https://en.wikipedia.org/wiki/2015_Pacific_typhoon_season) which has over 470 references. Peer-reviewed individual storms pages in Wikipedia often have 25+ references. To address this comment we emphasized our approach more clearly in text L163-170.

R2-C16:L 172: Blank after 250

R2-R16: L 177 A space has been added.

R2-C17:L 172-175: The sentence does not really provide an argument. I guess that you wanted to say that you do not have enough stations to perform kriging. This is, however not the case, as Goovaerts (2000, J Hydrol) has shown that even with 36 stations, kriging outperforms regression. I do not argue against the use of regression techniques, my point is that a sound justification has to be given (and sample size is not sufficient in your case). If I would have chosen regression, I would have written something like: Regression technique was preferred over geostatistical techniques even though this might be on the expense of a slightly inferior spatial interpolation (Goovaerts 2000) but it allows better identification of driving parameters.

R2-R17: Thank you for this feedback. We addressed this comment on L179-180. Of note we did run a autoKrige function on the annual data and two months for curiosity and the RMSE for these three cases was higher with the kriging than our regression approach.

For these three cases, RMSE was higher with kriging than with our regression approach.

R2-C18:L196: not clear in which dimension the resolution was applied (lateral or vertical?)

R2-R18: L202: There was no lateral or vertical dimension applied to the SRTM, the data was obtained from the reference provided. To be more clear we did update the term extracted to obtained.

R2-C19: L 237-238: this does not comply with the concept of autocorrelation

R2-R19: L242-245: Our apologies, spatial autocorrelation was the wrong term. We have updated the text accordingly.

R2-C20: L 258-259 and several places in the following: the fact that 26% of the values fall outside one SD around the mean does not imply large heterogeneity but it is an inherent property of any SD. In fact, for a perfect normal distribution 32% of all data should fall outside this range. This is true for very homogenous data (small SD) and very heterogeneous data (large SD). Your conclusions are thus wrong.

R2-R20: We removed this section.

R2-C21: L 263: see L 258

R2-R21: We removed this section.

R2-C22: L 267: Entire sentence not clear. Rephrase. Why do you subtract the mean from SD? Doing it the other way round could make sense.

R2-R22: L273 We changed 'minus' to 'on' and moved the 'they' in front of the 'therefore' to add clarity.

R2-C23: L 270: no references in the Results section

R2-R23: This reference has been removed from the results section.

R2-C24: L 272: do you really mean "periodic" (occurring in fixed intervals)

R2-R24: L276: We removed periodic to avoid any potential confusion.

R2-C25: L 275 and many places in the following: what is a maximum average? You need to be more explicit over which domain you average the data and over which domain you determine the maximum. Otherwise the reader has to speculate. This becomes even worse in the following.

R2-R25: L278-279 This has been updated to now read the mean monthly maximum rainfall instead of the average maximum monthly rainfall.

R2-C26: L277: there is no highest maximum because by definition the maximum itself is already highest. What do you mean?

R2-R26: L281 We re-orderd 'daily' and 'maximum' to address this reviewers comments.

R2-C27: L 278: what is a lowest maximum?

R2-R27: L282: We re-orderd 'daily' and 'maximum' to address this reviewers comments.

R2-C28:L 279: What is a highest mean annual maximum daily precipitation? Is it annual or daily, is it mean or maximum, what is a highest maximum...

R2-R28: L282-284: This has been updated with the same approach as the last two comments

R2-C29:L 286: Mean? Again it is not clear over which domain you average your data. This is especially unclear for SD. Is this the SD among station years or is it the SD among stations or is it the SD among years? These SDs will differ and carry different information.

R2-R29: L291: This now reads: 'The mean R-factor for the 42 stations' which based on the equations in the method implies that it is the mean of the R-factors (which is an annual value) for the 42 stations. Of note the SDs differ by around 5% when you change the analyses and the mean differed by less than 1%.

R2-C30:L 289: Again, unnecessary information that some data plot outside the SD

R2-R30: This has been removed.

R2-C31:L 294: Sentence seems to be incomplete

R2-R31: L298: The sentence was modified (a 'than' changed to a 'that of').

R2-C32:L 298-301: This is very typical also in areas where no typhoons occur. Hence this does not indicate a special effect of typhoons

R2-R32: L299-304: We did not quantify the impact of tropical cyclones in this section.

R2-C33:L 309: You claim that the SD of the predicted values was smaller than the SD of the observed values during the typhoon season. However, your data show the same effect also outside the typhoon season. Again, this is too weak to indicate any special effect of the typhoons

R2-R33: L311: In this section we do not discuss tropical cyclones. We just indicate that this period includes the tropical cyclone season. We quantify the impact of the typhoons on rainfall (20%) and rainfall erosivity (40%) in the region in other sections of the manuscript. We don't quantify the impact of the typhoon season on the predicted and observed values.

R2-C34:L 368: I found the whole chapter about typhoons not convincing and your concluding sentence trivial ("typhoons contribute a significant amount of rainfall erosivity"). If you would do a similar analysis for Sundays, you could also conclude "Sundays contribute a significant amount of rainfall erosivity". Or, you could conclude "Rain days contribute a significant amount of rainfall erosivity". What would we learn from this?

R2-R34: Our research indicates that while tropical cyclones contribute only 20% of the precipitation they contribute 40% of the rainfall erosivity in the region. The fact that an average of only 3.9 tropical cyclones a year contribute 40% of the rainfall erosivity is relevant for those modelling radiocesium and sediment dynamics in the region. To address this comment we redrafted this sentence and added the information about the amount of events and the erosivity contributed by tropical cyclones to this sentence (L374-375).

R2-C35:L 373: “Likely” is not acceptable here, because this is in the center of your topic; there should be enough meteorological knowledge on this.

R2-R35: We removed likely.

R2-C36:L 432: Better write “high”; otherwise you need to say what is higher than what.

R2-R36: L440: Changed to high.

R2-C37:L 442: I found the whole chapter (2 pages!) unnecessary (for instance, what has paddy rice to do with rain erosivity?), full of speculations, wrong or incomplete statements. Delete or reduce to one paragraph.

R2-R37: Section 4.4: First, it is very unclear as to what the speculations and wrong or incomplete statements are. We believe possibly many of these may be disagreements (similar to researchers' preferences for using the terms fixated vs adsorbed in the literature). As reviewer one found this section “very interesting” (R1-C19) and we believe this is important to the research community, we balanced the comments of reviewer 1 & 2 by removing approximately half of this section.

R2-C38:L 491: suddenly you change the abbreviation of year.

R2-R38: L474/477/478 This typo was corrected.

R2-C39:L 527: formatting of the references is inconsistent

R2-R39: We used the HESS End Note template for the reference formatting. Future corrections can be made as required.

R2-C40:Table 2: The SDs clearly show that no decimals are justified (for the SD and the mean).

R2-R40: The decimals have been removed from Table 2 with the exception of the R^2 .

R2-C41:Figure 3: What is a SAI? What is annul?

R2-R41: The figure caption included the SAI Definition.

R2-C42:Figure 4: typo (anomaly)

R2-R42: Anomaly has been updated to anomaly – thanks.