Response to Reviewers

Reviewer 1

R1-General Comment: Dear authors. Your work seems very interesting and I encourage you to continue it. But I regret to say that in the current version, I find it lacking appropriate methodological strategy in order to fulfil its objectives.

R1-General Response: Thank you for your encouragement. In response, we modified the final paragraph in the introduction along with the title and abstract to more clearly articulate the objectives of our research.

In brief, our objective was to analyze rainfall erosivity in the Fukushima region. This objective is similar to what has been published in a similar HESS paper by Meusburger et al. (2012) for Switzerland and recently by Panagos et al. (2015) for Europe. These are just two recent examples of a long history of rainfall erosivity research spanning 50+ years. The majority of these papers focus specifically on rainfall erosivity and implications arising from rainfall erosivity.

Similar to the rainfall erosivity papers cited above, we analyze rainfall data and then we also provide all the event data for the research community. This will provide researchers the opportunity to have access to the rainfall erosivity data, including kinetic energy data, we generated for 21 years of events (1995-2015) that are available for 42 rainfall stations within 100km of the Fukushima Dai-ichi nuclear power plant. We believe that this is well within the scope of HESS.

Further, we analyze the rainfall erosivity data and generate annual and monthly R-factor maps for the region. These R-factor maps also have a long tradition of being published (for example in papers cited above and many more referred to in our draft manuscript). The spatially interpreted R-factor maps will also be provided to the research community.

Finally, several papers clearly indicate the importance of rainfall for driving radiocesium transfers in the Fukushima region. This has been articulated more clearly with the addition of a paragraph, the 2nd last paragraph, at the end of the introduction.

We believe this reviewer likely expected us to model radiocesium and sediment dynamics, when this was not the objective of our research. Accordingly, we adjusted the final paragraph of the introduction to our manuscript to clearly articulate our research objectives, and address the majority of this reviewer's comments. Individual responses to his comments follow below.

R1-C1: As you state in the Introduction, "the goal of this research was to improve understanding of soil and radiocesium transfers...". However, selection of USLE as the main methodology is problematic, considering that USLE is only appropriate for assessing soil loss and not transfer of soil (sediments) and moreover their concentration in surface waters. Therefore, the objective of the paper cannot be served by the main methodology applied.

R1-R1:

We fully agree with the reviewer that the relationship between local soil erosion and catchment scale sediment transfers is complex and needs to integrate different geomorphological models acting at different scales. This paper is part of an international project that aims to understand soil and radiocesium transfers within hydrographic basins and research teams are specifically dedicated to modelling the transfer of sediments from the hillslope to the ocean. In this context, rainfall erosivity is one of the key parameters required to accurately model sediment dynamics. Unfortunately this aspect is sometimes a bit overlooked, as geomorphologists have access to regional data that may not always be entirely representative of their study area, particularly at smaller spatial scales. Therefore the goal of our manuscript wasn't to apply a soil erosion model. To address this comment we have updated the abstract, title and final paragraph of our manuscript to hopefully avoid future misinterpretations about our research objectives.

R1-C2: Moreover, you are missing to justify the use of USLE for local to regional mapping (as the current), as USLE was originally developed for hillslope soil loss assessments -here, proper references are missing**.**

R1-R2: Again, we don't apply the USLE in this paper. We examine rainfall erosivity, as a qualitative measure of regional vulnerability to the particle-bound transfer of radiocesium. Further we do state that there are limitations for applying the USLE at the catchment scale. As we don't apply the USLE, the references regarding its limitations at the catchment scale already included in text seem sufficient to us. Further, rainfall erosivity is a parameter that needs to be included in all soil erosion models, either empirical or physically-based, and thus our approach therefore aims to be more generic than the potential application of a specific soil erosion modelling framework.

R1-C3: You state that "...characterizing the rainfall regime of the fallout impacted region is fundamental to modelling and predicting radiocesium migration". Even if USLE was considered proper for this work, rainfall is not the only erosion factor in USLE (or any other model). Equal important parameters (USLE is a multiplicative equation) are soil type, slope, vegetation coverage and management, and conservation measures. Taking only rainfall, it is assumed (?) that topography, physiography, and management of the entire study area is absolutely homogeneous (!)

R1-R3: Again, this comment likely refers to the fact that we have not been clear enough on the fact that we do not apply the USLE in this paper. Accordingly we do not feel it is necessary to respond to all the comments about the other USLE factors. Our manuscript examines rainfall erosivity and investigates the R-factor and characterizes the rainfall regime of the region.

Once more, there is a long-tradition of rainfall papers with the two mentioned above (one of which is well cited in HESS) and many more in the literature. We strongly believe that our manuscript and its data analyses and discussion fits well within this rainfall erosivity research tradition.

And yes characterizing the rainfall regime in subtropical landscapes is fundamental to modelling and predicting radiocesium migration. In the Fukushima region in particular, the influence of typhoons on rainfall erosivity is very pronounced (in response to reviewer 3 we have added a paragraph - 2nd last in the introduction - that emphases the importance of typhoons and the rainfall regime).

Importantly, we provide all the data, including kinetic energy, for all rainfall events for those who want to model soil and radiocesium migration with a USLE or another appropriate modelling framework. Finally, there have been USLE-based models applied in the Fukushima region with non-spatially distributed rainfall erosivity (Kitamura et al., 2014b; Yamaguchi et al., 2014). These models incorporated one fixed rainfall erosivity value, or a mid, a high and a low rainfall erosivity value in a sensitivity analysis.

Here we provide not only the data, but also spatial maps of rainfall erosivity which will help more accurately model and predict rainfall-driven soil erosion.

R1-C4: From your statement "...it is important to combine rainfall erosivity layers with a cover factor that seasonally depicts soil erodibility based on land cover..." it seems that your team may not be quite familiar with erosion research (confusion in terminology like between 'soil erodibility', which is the inherent vulnerability of the specific soil type to erosion, and the 'management cover factor', which is the natural or human-induced coverage of the soil and thus protection by rain)

R1-C4: In response to this comment we removed this statement as we felt it may mislead future readers into thinking we are modelling sediment transfers. We re-integrated the need to relate land-cover and rainfall in the implications for radiocesium behavior section (4.4 – paragraph 4) that was added to the manuscript.

R1-C5: I would propose to use a different erosion model, which would take into account soil loss and sediment yield together and -moreover- risk of pollutant dispersion in an integrated approach. To my knowledge, an appropriate and modern spatial model for local to regional assessments of diffused pollution is G2 (module G2met) (Karydas et al., 2015), recently developed and published on http://www.mdpi.com/2073-4441/7/8/4323. G2 provides month-time step assessments, which you very correctly addressed as a necessity.

R1-R5: Again, we actually don't apply an erosion model in our paper. We provide analyses of rainfall erosivity. We do believe that this reviewer should be cognizant of rainfall erosivity research as one of his co-authors on his recommended G2 model in this comment is the author of the two rainfall papers we referred to in the introduction of this response to his comments.

Moreover, we noticed that the rainfall erosivity data used in the recommended G2 model, comes from the Panagos et al. (2015) rainfall erosivity paper. Further, both the Panagos et al. (2015) paper and the Meusburger et al. (2012) HESS paper are both cited in this G2 model paper in the link provided.

Importantly, rainfall data is not readily or easily available in Japan for an entire region. All the rainfall data had to be downloaded from a website. For the 10min data, for ~19 years, for 40 stations involved downloading 500,000 tables. In total over 1,000,000 tables were downloaded and incorporated in our analyses, consisting of gigabytes of data. After publication, this reviewer will be able to access our event dataset and potentially apply his G2 model in the Fukushima region, as he did with the rainfall erosivity dataset from Panagos et al. (2015) for Cyprus.

Essentially, the majority of the comments from this review were directed at a USLE modelling manuscript. Here, we submitted a manuscript on rainfall erosivity, similar to the two referred to in the previous paragraph, one of which is published and highly cited in HESS. To address this last comment we cite this reviewer's model as a demonstration how rainfall erosivity datasets are important inputs into models that predict downstream contaminant transfers (first paragraph of section 4.4).

Reviewer 2

R2-C1: Dear Authors, First of all, I believe that the topic you are dealing with is a very important one and much demanded by the earth system science community. The authors have worked intensively on the interesting and relevant subject of rainfall erosivity and particle-bound contaminant transfer in Fukushima region.

R2-R1: Thank you

R2-C2: However, the basic idea to improve understanding of radiocesium transfers in the soil only with the contribution of rainfall is too simple for drawing general conclusions.

R2-R2: Similarly to our response to reviewer #1, we feel that this is possibly an issue of phrasing. We believe that characterizing the rainfall regime and the patterns of rainfall erosivity both temporally and spatially is important for future research and even the management of potential radiocesium transfers.

There is indeed a long tradition of rainfall erosivity research that strives to help improve soil erosion studies through providing a stronger modelling foundation. As noted in our response to reviewer one, USLE-based modelling studies used a single rainfall erosivity value or a high/mid/low range in papers published in the Fukushima region after the accident (Kitamura et al., 2014b; Yamaguchi et al., 2014). Our spatially interpreted maps could therefore technically help improve the understanding of radiocesium transfers. To address this comment we have re-written the last paragraph of the introduction along with the abstract and the title.

R2-C3: The methodologies applied for the reflections, however, are modest and should be better improved. In particular, the main objective of the work was not achieved: the methodology used is not suitable to explain the loss of soil and does not consider the transfer of soil contaminants.

R2-R3: We are not modelling soil loss in our paper. We are examining rainfall erosivity. Indeed we are unaware of where we state we are modelling soil loss. Please see the response to the reviewer number 1 (R1-General Response, R1-R1, R1-R2, & R1-R3) as this comment is addressed there.

We believe our methodology is consistent with the methodology applied in rainfall erosivity papers published (i.e. Meusburger et al. 2012 in HESS). As our methodology applies to rainfall erosivity and not soil loss, our response to this comment and reviewer number 1 was focused on adjusting the language of the objectives section in the last paragraph of the introduction, along with the abstract and the introduction.

R2-C4: The authors are limited to aggregate precipitation data without carry out a quality control and homogenization of the series, especially vital to correctly calculate trends over time.

R2-R4: We spent a significant amount of time on quality control. We examined all the stations for consistency and searched for point-breaks and tested for homogeneity in the daily data set.

We found some non-significant point breaks in the long-term daily data set. We chose not to correct these. A recent study examining the long-term data in Japan (Duan et al., 2015 in Clim Dyn) did not correct any inhomogeneity long-term data sets for Japanese rainfall stations either. This supports our approach to the daily data. Further, we do not extrapolate the daily data or base any significant conclusions on the daily data. Our focus was on the 10-min data. We searched extensively for the best-practice for managing potential inhomogeneity in 10min data. Homogenization of 10min data is difficult and may actually add more bias to the data than you potentially may remove, particularly in regions with high spatial and temporal rainfall variability. Our opinion was that we actually risk negatively impacting the 10-min data through homogenization, particularly in a region that experiences extensive temporal and spatial rainfall variation. To support our position Shiono et al., (2013) also did not perform any homogeneity tests in their publication 10-min JMA data. We have added a paragraph (4^{th} in methods) to discuss quality control.

Indeed, this comment from the reviewer made us also think twice about internal quality assurance and potential compilation and analyses errors in our own dataset. To examine this, we repeated every data process and compilation for 10 randomly selected stations backwards (e.g. data analyses back to downloading) and then forwards (downloading to data analyses) and realized that during our initial data compilation we had a language issue with excel that dropped commas (i.e. periods) for some of the stations after a computer was retired from service. This resulted in initial rainfall erosivity values in our first manuscript being low. After correcting this error, we repeated the internal quality assurance (backwards and forwards analyses) for 10 randomly selected stations.

R2-C5: The slope and statistical significance of the trend are not calculated.

R2-R5: Regarding figure 4, our goal was not an extrapolation of the past to examine future potential trends. The long-term stations have lower than average rainfall for the region so they would not provide an appropriate dataset for long-term extrapolation for the region.

R2-C6: However, It is not possible to calculate a trend considering different periods of availability of the series: it's indispensable to select a common period to all stations to detect the temporal evolution and in order to permit a correct comparison of the amount of rainfall per year.

R2-R6: We have updated Figure 4 to use available data from the same temporal period. We did not include trends as our objective was the depiction of the inter-annual variability.

R2-C7: It would be better to use the SAI (Standardized Anomaly Index) that expresses the anomaly of the precipitation in respect to the mean value of the 30 years reference period. Consequently it is necessary to change the figures 3, 4 and 6.

R2-R7: We have provided both the SAI and the actual precipitation data for Figures 3, 4 &6. We provided the original figure format that depicts the data as SAI plots are uncommon in rainfall erosivity research (e.g. only 4 results when searching "Standardized Anomaly Index" and "rainfall erosivity" in Google Scholar).

R2-C8: Both the Results and Discussion chapters are very descriptive and unfocused. An extensive discussion of the involved factors (Pmm and R), processes and interactions should be provided with adequate references to the corresponding scientific literature.

R2-R8: We aimed for a descriptive results and discussion to allow for comparing rainfall erosivity in Fukushima in relationship to Japan and the rest of the world, including Ukraine in particular. Further, our discussions and results were based on other rainfall erosivity papers published in HESS and elsewhere. This may be a style preference. We have re-worked the discussion and the results to improve these sections in relation to our objective.

R2-C9: The results of your work are important and must be disseminated, but because these comments may mean some substantial reworking of the text and more modeling/data analysis, the revisions have been classed as major.

R2-R9: Thank you for your constructive feedback.

Reviewer 3

Before getting into our detailed response to reviewer 3, we would like to thank them for taking their time to provide an extremely detailed review. Their feedback has significantly improved our manuscript.

R3-C1: 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.

R3-R1: We disagree with this generalization from this reviewer. Although there have been select rainfall erosivity studies conducted in the region, and for Japan, there has not been an extensive examination of rainfall and rainfall erosivity in this important area. To assist demonstrating this novelty we have expanded our discussion on the rainfall erosivity studies in the region and Japan to clearly articulate the novelty of this research (Second and third paragraph of Section 4.2).

R3-C2: 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.

R3-R2: We have updated the title and the research objectives to avoid this confusion.

R3-C3: 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.

R3-R3: We have removed the reference to subtropical areas. We have also added a sentence in the implications section (last sentence Section 4.4) indicating that it would be beneficial for future research to examine rain drop distributions. The references included in the introduction indicate how this approach has been utilized worldwide, including subtropical regions. To address this comment we modified a statement to indicate that the debate regarding the universality of the R-factor is beyond the scope of this current research (3rd last paragraph of the introduction).

R3-C4: 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.

R3-R4: First, there have been USLE papers published in post-accidental contexts (Kitamura et al., 2014a; Kitamura et al., 2014b; Yamaguchi et al., 2014). Second, we don't apply a USLE model. The application of USLE models isn't typically the main objective of the rainfall erosivity papers published in HESS and elsewhere (our response to reviewer 1 further addresses this perspective). Third, large events have been demonstrated to transport significant proportions of the radiocesium in the region after the FDNPP accident.

We chose to approach the remainder of this comment with a contrasting approach. First, the title was updated. Second, we believe the problem lies in the fact that we didn't clearly articulate the importance of rainfall in our introduction and addressed this comment with the addition a paragraph near the end of the introduction (2nd last) that clearly indicates the importance of rainfall in the Fukushima region which addresses these points. Indeed the situation in Chernobyl and Fukushima are very different (particularly with respect to rainfall). Of course, we never state the R-factor and the USLE is the best instrument to model radiocesium transfers. That said, rainfall is fundamental to post-accident Fukushima radiocesium dynamics and we don't understand why you would want to remove the research context from this manuscript.

R3-C5: 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.

R3-R5: This is mainly a stylistic difference that applies to a few paragraphs in the introduction. Several of the references in these paragraphs applied to similar regions and included recent applications rather than simply relying on one or two classic citations. This approach was developed based on comments from previous reviewers on published articles. We have addressed this comment by revising citations in a few paragraphs in the introduction based on the comments below. Further, the classic citations were often located in the preceding sentence.

R3-C6: 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.

R3-R6: Again we believe the writing and imprecision results from a stylistic difference between us and the reviewer pertaining to level of detail provided. This has been addressed in the details section below where more details are provided.

As recommended, the material and methods section for the modelling procedure has been improved with more specific details provided. The models have been updated and consequently the results were modified. In order to depict the modelling results more appropriately, we now compare observed and estimated values through the RMSE and the coefficient of determination R^2 (see Table 2).

R3-C7: 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 .

R3-R7: Our apologies, this should have been caught prior to submission. It has been updated throughout the text.

R3-C8: 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 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.

R3-R8: We are unsure where this reviewer actually derived this particular hypothesis. To address this misunderstanding we have updated the objectives of our paper (last paragraph of the introduction) as well as changed the abstract and title of our manuscript. Further, we have updated much of our discussion.

R3-C9: The manuscript should be language checked by a native speaker.

R3-R9: Done.

R3-C10: The author list contains several names that did not contribute to this manuscript. They must be deleted (see p 7246).

R3-R10: After looking at p7246, we do not understand the intention or the point of this comment.

R3-C11: Page 7227 Line 2-5: delete; they are not related to the study presented in this manuscript

R3-R11: Similarly to our response above (R3-R4) we don't understand the utility of removing the context of this research from the abstract, introduction or throughout manuscript.

R3-C12: Page 7227 Line 10: As mentioned on page 7231, line 24ff, also 10 min data were used although not from all 60 stations. (Be clearer in the abstract)

R3-R12: We did use the statement "analyzed" and thus did not specifically refer to all stations specifically used for rainfall erosivity calculations in the original manuscript. That said we have updated the abstract for clarity.

R3-C13: Page 7227 Line 11: The time period of the dataset should be mentioned there.

R3-R13: Owing to different time periods for the different analyses this was omitted. The time periods have now been included in the abstract.

R3-C14: Page 7227 Line 14: The unit of long-term erosivity is MJ mm ha−1 h −1 yr−1

R3-R14: Our apologies for missing this, it has been corrected in the abstract and throughout the manuscript.

R3-C15: Page 7227 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.”

R3-R15: This sentence has been changed owing to the updated results.

R3-C16: Page 7227 Line 19: Is the typhoon season in July and August?

R3-R16: The most active period of the typhoon season was added to the abstract.

R3-C17: Page 7227 Line 19 ff: The sentence should be reorganized, the important part of the sentence in the beginning and additional information in the end.

R3-R17: This sentence pertaining to the typhoon season has been reorganized in the abstract

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R3-C18: Page 7227 Line 22 to 25: Delete; pure speculation; nothing was studied regarding radiocesium transfer or the impact of typhoons.

R3-R18: We have now quantified the contributions to rainfall erosivity from typhoons over a 21 year period and added this information to the abstract, methods (paragraph prior to section 2.4), results (section 3.4), and discussion (4.3). Thus we kept this in the manuscript.

R3-C19: Page 7228 Line 2 to 14: Delete

R3-R19: Similarly to our comments above (R3-R4 & R3-R11) we felt we needed to strengthen the focus on radiocesium, in general, and the importance of rainfall, in particular. Accordingly we added a paragraph near the end of the introduction (2nd last paragraph) to emphasize this point rather than simply removing the context of this research and this manuscript as this reviewer suggested.

R3-C20: Page 7228 Line 18: Primary literature is missing. Where is the justification for these two references?

R3-R20: The primary literature was cited on the lines (16/17) directly above. To address this reviewer's request we removed these two references, even though the primary literature was cited in this paragraph.

R3-C21: Page 7228 Line 19: Rather replace "fundamental to determining" by "fundamental to determine".

R3-R21: Stylistic – although the meaning of the sentence doesn't actually change with this recommendation by the reviewer, we made this correction (Introduction 3rd paragraph).

R3-C22: Page 7228 Line 23: improper reference

R3-R22: The only reference on line 23 is the Renard et al., 1997 link to the RUSLE which we believe is accurate. As for the Oliveira et al., (2013) reference at the end of this sentence, this is a statement these authors made from which we derived this idea/text in the manuscript. Accordingly we cited it to give the authors credit for their influence on this statement. We will leave it up to the editor and the other reviewers if they believe we should give credit for this particular reference and their impact on our manuscript or remove it as this is largely a stylistic nuance that varies from reviewer to reviewer, editor to editor and journal to journal.

R3-C23: Page 7228 Line 25ff: The sentences should be revised to achieve higher precision and less repetitions.

R3-R23: The repetition in this sentence provided a connection between two thoughts (stylistic). We modified these sentences when addressing the other comments from the reviewer regarding the introduction.

R3-C24: Page 7228 Line 28: Where is the justification to cite Lee and Heo 2011 and Lu and Yi 2002.

R3-R24: They were used as a reference to recent and regional studies R-factor in a similar region of the world. They have been now removed from this sentence and added to a sentence discussing global research and Asian research at the end of the 4th paragraph in the introduction.

R3-C25: 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.

R3-R25: This line has been shifted to the 4th paragraph on the introduction.

R3-C26: Page 7229 Line 5: The wording “storm erosion values” is undefined and imprecise.

R3-R26: This has been updated in the 3rd paragraph of the introduction.

R3-C27: Page 7229 Line 5: Diadoto and Bellocchi did not find this; wrong citation

R3-R27: This one citation we should have caught in our internal review process. Our apologies – this has been deleted.

R3-C28: Page 7229 Line 8/ 9: The meaning of “To incorporate cyclical rainfall variations” is not clear. Intraannual 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.

R3-R28: Changed to inter-annual (3th paragraph of the introduction) and in other locations in the manuscript.

R3-C29: Page 7229 Line 10: Meaning of the sentences is not clear. Better predictor than?

R3-R29: This sentence has been removed.

R3-C30: Page 7229 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.

R3-R30: This was updated for accuracy in the 5th paragraph of the introduction.

R3-C31: Page 7229 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

R3-R31: We removed this statement as we felt it may add confusion that we are combining different C-factor and K-factor layers and applying the USLE. We added the importance of land cover back in the 3rd paragraph of a new section on implications for radiocesium dynamics (Section 4.4).

R3-C32: Page 7229 Line 25: Rather replace “to understanding” by “to understand”.

R3-R32: Replaced.

R3-C33: Page 7229 Line 26: Why is the R factor particularly important in subtropical catchments with cyclonic activity?

R3-R33: We added a paragraph (2nd last in the introduction) about the importance of typhoons for radiocesium transfers and removed this sentence to provide more clarity as we probably didn't clearly make this connection in the original manuscript. We also removed the subtropical reference owing to previous comments from this reviewer pertaining to the title (R3-C2).

R3-C34: Page 7230 Line 14: Rather change "progressing westward" to "to the west" to simplify the sentence.

R3-R34: Changed (2nd paragraph of methods).

R3-C35: Page 7230 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?

R3-R35: Those interested in differences in post-fallout radiocesium behavior in the Chernobyl and Fukushima contexts have much to gain from this comparison, which we believe is important. As this wasn't the optimal location for this comparison, we compiled all the comparison to Chernobyl in the last paragraph of the newly created section in the discussion on the implications for radiocesium dynamics (4.4).

R3-C36: Page 7230 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?

R3-R36: Other papers have used R-factors for regions impacted by cyclonic activity in Australia, Asia, Mexico, etc.. To address this comment we added a sentence to end of section 4.4 regarding drop size. Typhoons are now mentioned in both the final two paragraphs of the introduction. The peak season is mentioned in the sentence explaining the entire typhoon season has been added at the end of the second paragraph of the methods.

R3-C37: 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. (Different stations used for daily data)

R3-R37: The section on monitoring stations is not straightforward as the available data and the stations used for the different analyses originally differed. We have modified this section based on the reviewers comments (the first paragraph of section 2.2 was edited and 2 paragraphs that followed were compacted into one paragraph).

R3-C38: Page 7231 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."

R3-R38: This was simplified (First paragraph of section 2.2)

R3-C39: Page 7231 Line 8: "19 years were available"

R3-R39: Changed (First paragraph of section 2.2).

R3-C40: Page 7231 Line 11: "were operated"; I wonder whether this is really true. Does Japan have a Meteorological Data Acquisition System since more than 100 years?

R3-R40: "Meteorological services in Japan were initiated in 1875 by the Tokyo Meteorological Observatory (TMO) - part of the Ministry of the Interior. TMO was renamed the Central Meteorological Observatory (CMO) in 1887, and was transferred to become part of the Ministry of Education in 1895. In 1956, CMO became an affiliate agency of the Ministry of Transport (MOT) under the name of the Japan Meteorological Agency (JMA)."(From http://www.jma.go.jp/jma/en/Background/history.html)

They have indeed have been acquiring data for more than 100 years, nonetheless, as this sentence was not fundamental to the text, it was removed.

R3-C41: Page 7231 Line 16: Rather replace "multiple years" by "several years".

R3-R41: This section was removed as per comment R3-R37.

R3-C42: Page 7231 Line 17: It is not clear what "live" stations should mean.

R3-R42: This section was removed as per comment R3-R37.

R3-C43: Page 7231 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.”

R3-R43: We aren’t convinced the accuracy is overly improved with this suggestion though we did modify this sentence in response to reviewer 2’s comments about using the same number of stations for the long-term analyses and SAI plots (Section 2.2. paragraph 2).

R3-C44: Page 7231 Line 22: Change the sentence to “data from the long-term stations were additionally analysed”

R3-R44: This sentence was modified in response to reviewer 2’s comments about using the same number of stations for the analyses (Section 2.2. paragraph 2).

R3-C45: Page 7231 Line 24: Why “on average”? What do you mean with “consistently”? Continuously?

R3-R45: Why not “on average” - it worked in this context. This section was removed as per comment R3-R37.

R3-C46: Page 7232 Line 6: replace for by from

R3-R46: This section was removed as per comment R3-R37.

R3-C47: Page 7232 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).

R3-R47: There are rainfall erosivity papers where KE has been used for kinetic energy (e.g. Verstraeten et al. (2006)). Nonetheless we removed the KE acronym.

R3-C48: Page 7232 Line 16: The unit of er must be corrected to MJ ha−1 mm−1 .

R3-R48: Thank you for catching this - this has been corrected (our apologies for missing this) (Section 2.3. after equation 2).

R3-C49: Page 7232 Line 18/ 19: The “unit of rainfall energy” cannot be calculated. You calculated the “rainfall energy per unit depth of rainfall”.

R3-R49: Thank you for catching this - this has been corrected (Section 2.3. after equation 2).

R3-C50: Page 7233 Line 1: this is not completely correct. It only applies for intervals r of constant ir. Rephrase.

R3-R50: This has been updated (Section 2.3. after equation 3).

R3-C51: Page 7233 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.

R3-R51: This abbreviation was removed.

R3-C52: Page 7233 Line 5 ff: The difference between argument 1) and 4) is not clear.

R3-R52: This was edited where argument 4 and 2 were removed for accuracy and clarity, thus the citation was also modified (3rd last paragraph of section 2.3).

R3-C53: Page 7233 Line 11/12: This sentence is repeated similarly in line 15 and 16.

R3-R53: This section was edited to remove the repetition and include the results from a t-test between the examination of the impact of snowfall on R-factors control so we could remove this from the discussion and results to address other comments from this reviewer (See R3-C81 and R3-C111-115) (second last paragraph of Section 2.3).

R3-C54: Page 7233 The paragraph from line 11 to line 16 could be shortened.

R3-R54: The paragraph was shortened (second last paragraph of Section 2.3).

R3-C55: Page 7233 Line 17/ 18: Why don't you use the term 'precipitation' instead of 'rainfall' in the following when it is the correct expression?

R3-R55: We used the term rainfall for simplicity as it is the driver of erosion and there were no significant differences between erosivity when you accounted for snowfall. Nonetheless, we updated the text to use precipitation throughout the manuscript where appropriate.

R3-C56: Page 7233 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

R3-R56: All the covariates were updated to a resolution of 250m as justly suggested by the reviewer. Moreover, some of the covariates, especially morphological ones, were recomputed to better reflect the processes and associated scales at which they can influence precipitation patterns and intensities. Thanks to these changes, the quality of the R-factor final spatial models has been improved. (See changes to section 2.4)

R3-C57: Page 7234 Line 21: Rather replace “month by month” with “monthly”.

R3-R57: Change made (3rd paragraph of section 2.4).

R3-C58: Page 7234 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.

R3-R58: We doubt the reader would be left wondering; often people re-iterate the study region in the text. We made this stylistic change and removed the reference to the Fukushima region.

R3-C59: Page 7234 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.

R3-R59: The sentence containing “straightforward” and “flexible” has been removed. The part describing the GAM procedure has been improved and references have been added in order to allow the reader to better understand the procedure and also where to find more information on the approach (See changes to section 2.4).

R3-C60: Page 7234 Line 25: The abbreviation GAM was already introduced, so it should be applied.

R3-R60: The acronym has been applied (4th paragraph of section 2.4).

R3-C61: Page 7234 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.

R3-R61: Precisions have been added to the text to avoid any confusion (4th paragraph of section 2.4).

R3-C62: Page 7234 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.

R3-R62 The dataset has been updated and now contains 42 stations which are all used during the modelling procedure. Moreover, the R-factor maps were produced to better understand the evolution of rainfall erosivity in the area surrounding the main soil contamination plume, and provide this data as a potential input data for soil erosion models. So, these spatial layers have to cover independent hydrological basins (on their whole). Thus, we applied a mask to the final R-factor layers considering all the basins contained entirely within the 100km area around the FDNPP. Considering this surface as the target area, many stations used for the modelling procedure were now outside this mask which is preferable for the quality of the mapping procedure as noted by the reviewer (This has been explained in section 2.4)

R3-C63: Page 7234 Line 28: Is there no larger DEM layer available so that the southernmost station could be included?

R3-R63: A DEM was derived from the NASAs SRTM mission database in order to cover a largest area and then include all of the 42 stations considered in this study.(2nd paragraph of Section 4.2, bullet point 2)

R3-C64: 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.

R3-R64: This station is now included in the analyses as per R3-R62.

R3-C65: 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.

R3-R65: The AIC has been calculated during the backward stepwise procedure and is used to support the choice of the final covariates considered in each model. Hence, this procedure follows the rule of parsimony. The AIC differences calculated during the procedure are presented in Table 4 and the procedure is outlined in section 4.2.

R3-C66: Page 7235 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)

R3-R66: The part describing the GAM procedure has been detailed in order to allow the reader to better understand this procedure and to avoid any confusion or misunderstanding in Section 4.2

R3-C67: Page 7235 Line 15: Is “fitted by penalized Maximum Likelihood” the general wording for this?

R3-R67: Again, the part describing the GAM procedure has been improved in Section 4.2, e.g. now we clearly indicate that the extra penalty added to each smoothing term can potentially be set to zero during the fitting process, especially in case of multi-collinearity or multi-concurvity of different covariates.

R3-C68: Page 7235 Line 24 + 25: Were ME and MAE calculated with the log data or with the original data?

R3-R68: ME and MAE were computed on the original data to avoid any misunderstanding. Following the advice of the reviewer n°3, we have calculated RMSE and R^2 for this revised version rather than ME and MAE (see table 3).

R3-C69: 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?)

R3-R69: Although this may be obvious to this reviewer, these details may be important to those researching contaminant transfers in the Fukushima region after the accident, particularly to those who are not rainfall specialists. For the wider readership, it may be interesting that such rainfall variability exists in this small region. To improve the precision of this sentence we have included some data on the results (first paragraph of the results).

R3-C70: 7236 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?

R3-R70: Our apologies for including the caption in text. This has been edited. Again, this data and these results may not excite this particular reviewer (based on the above noted sarcasm) but this information is important in Fukushima, for comparing the region to Ukraine, and for understanding the potential impact of rainfall variability on radiocesium dynamics in the Fukushima region.

R3-C71: 7236 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 EI30 for each station.

R3-R71: Both plots originally had different site datasets and they were plotted in rank order to demonstrate the variability and not compare plots/datasets. We don't see the logic of the north to south ranking in this region. We have adjusted the datasets to have matching sites presented in the tables and have maintained the ordering.

R3-C72: 7236 Line 8: a new paragraph would be helpful because the topic changes.

R3-R72: The topic, we believe is the same, presenting the results from the daily rainfall analyses. Accordingly we don't believe a new paragraph would be helpful.

R3-C73: 7236 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)

R3-R73: We have changed to mean annual to address this comment (first paragraph of results)

R3-C74: 7236 Line 10: you said this already Line 11 and following: "temporal" is not the appropriate term

R3-R74: On Line 10 we state the spatial heterogeneity to confirm the perspective, to address this we removed the first instance. One Line 11, "[A]nnual temporal variation in rainfall" included a reference to the scale. Although appropriate, we have updated this term to inter-annual variation in the first two paragraphs of the results.

R3-C75: 7236 Line 13: Do you mean "coefficient of variation" instead of "coefficient of variance"?

R3-R75: Our apologies, we should have picked this up prior to submission. This has been changed to variation (2nd paragraph of the results), as it was stated in the previous paragraph.

R3-C76: 7236 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.

R3-R76: The term was found in the next sentence (where it stated 124 years), temporal was simply removed and significant was added prior to the temporal inter annual variation (2nd paragraph of results).

R3-C77: 7236 Line 20-23: sentence not clear

R3-R77: "Of note, these long term rainfall stations plot near (n = 2), or below (n = 5) the annual rainfall mean for all of the stations, and they therefore may not be indicative of all rainfall trends in the region (Fig. 2)." We believe this sentence was clear. Nonetheless, the sentence was edited based on the revised results to agree to comments from reviewer 2 regarding the long-term analyses (2nd paragraph in section 3.1).

R3-C78: 7236 Line 24: What a surprise.

R3-R78: This sentence was removed. Importantly, this paper is not aimed exclusively at rainfall erosivity specialists; it's aimed also at a generalist crowd. Although this reviewer may well versed on rainfall and rainfall erosivity worldwide, others may not be up-to-date on this topic and may find this type of information useful to support their research. This is important for those researching contaminant transfers after the FDNPP accident.

R3-C79: 7236 Line 25: Delete "into" in the part of the sentence "during the summer months and early into fall".

R3-R79: into was removed.

R3-C80: 7236 Line 26: where can the periodic typhoons be seen in Fig. 5?

R3-R80: We have quantified the contributions of the typhoons and plotted them in Fig 2,3&5.

R3-C81: 7236 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).

R3-R81: These sentences have been connected (3rd paragraph of section 3.1).

R3-C82: 7237 Line 11: The maximum of annual maximum daily rainfall was already mentioned in line 7/ 8. Therefore this paragraph should be shortened.

R3-R82: We have now clearly indicated they are different (last paragraph of section 3.1).

R3-C83: 7237 C4608 Line 15: Snowfall has no influence on R (only snowmelt); be more precise. I do not know what you really mean.

R3-R83: This paragraph has been removed and succinctly summarized in the methods.

R3-C84: 7237 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. . .”.

R3-R84: This paragraph has been removed and succinctly summarized in the methods.

R3-C85: 7237 Line 25: The right unit of annual erosivity is MJ mm ha−1 h −1 y −1 .

R3-R85: Updated; again our apologies for this oversight.

R3-C86: 7237 Line 28: The link to figures should be more exactly; here: Fig. 2B.

R3-R86: The link to this figure has been made more exact (1st Paragraph of Section 3.2). Also we updated the other figure links.

R3-C87: 7237 Line 26-28: this is caption not Results

R3-R87: This has been updated, our apologies for not catching this in the editing stage (1st paragraph of section 3.2)

R3-C88: 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.

R3-R88: This statement was based on the data behind this figure. Also when looking at the original Fig. 2B, we don't see how this reviewer calculated there are 20 stations above one standard deviation on the mean and no stations below?

R3-C89: 7238 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

R3-R89: We are comparing two numbers here. We do not believe a test of significance is necessary in this circumstance. Therefore the comment about random variation we don't believe is applicable.

R3-C90: 7238 Line 10/ 11: It would be better when the mean average annual R factor is numerically mentioned here.

R3-R90: This has been updated – second paragraph of section 3.2.

R3-C91: 7238 Line 17: I cannot see the typhoon season

R3-R91: We have quantified the contributions of the typhoons and plotted them in Fig 2,3&5.

R3-C92: 7238 Line 19: Were the 16% tested? Are they significant? If not, delete the statement.

R3-R92: See response to R3-C86 above.

R3-C93: 7238 Line 26: Better "similar distributions as the observed values"?

R3-R93: This text was edited in the process to update this section in response to other comments from this reviewer.

R3-C94: 7239 Line 3/ 4: The percent values here don't match with the values in Table 3.

R3-R94: Table 3 has been updated and the text changed consequently.

R3-C95: 7239 Line 6/ 7: Table 3 shows that ME for models depicting R factors are close to zero from October to April.

R3-R95: Table 3 has been updated and the text changed consequently.

R3-C96: 7239 Line 5 to 14: The complete paragraph has to be adjusted with Table 3. Moreover the abbreviations ME and MAE should be explained in the table captions again.

R3-R96: Table 3 has been updated as well as associated captions.

R3-C97: 7239 Line 16: Is the "backward stepwise procedure" well-known in general?

R3-R97: We believe so - nonetheless we have detailed this procedure in the text in 5th paragraph of section 2.4.

R3-C98: 7239 Line 19: Is this correlation significant? The coefficient of correlation is missing.

R3-R98: Results have been updated considering the previous remarks and advice provided by the different reviewers. The text has been modified (section 3.3).

R3-C99: 7239 Line 20: Where is the similar spatial distribution shown in Fig. 7a?

R3-R99: Results have been updated considering the previous remarks and advice provided by the different reviewers. The text has been modified (section 3.3).

R3-C100: 7239 Line 20/ 21: Are these gradients significant?

R3-R100: Results have been updated considering the previous remarks and advice provided by the different reviewers. The text has been modified (section 3.3).

R3-C101: 7239 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?

R3-R101: The annual R-factor map has been updated and re-delineated according to the limits of the target area, i.e. hydrological basins contained on their whole within the 100km area around the FDNPP. The modelling standard error map has been replaced by a coefficient of variation map in order to better

demonstrate the relative importance of the modelling error compared to the mean predicted value for each pixel.

R3-C102: 7239 Line 1: what do you mean with “direction evolves”? Second sentence seems also to be wrong.

R3-R102 This was removed with the updated results.

R3-C103: 7239 Line 2: What is "it"? The previous sentence used plural.

R3-R103: This was removed with the updated results.

R3-C104: 7240 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.

R3-R104:

Does this comment imply that the majority of USLE research should have never been published as the plots used to develop the USLE were on a slope of ~9%? Based on this comment, not even reviewer one’s model in Cyprus should have been published, as it’s based on pre-published R-factor data. Further, why can’t factors influencing rainfall change monthly?

In response, all the covariates were updated to a resolution of 250m. Moreover, some of the covariates, especially morphological ones, were recomputed, e.g. by applying smoothing filters at different wavelengths, to better reflect the processes and associated scales at which they can influence precipitation patterns and intensities (section 2.3). To support the selection of the covariates to compose the final models, the AIC have been calculated during the fitting procedure (section 3.3 and Table 3).

The selection of elevation, slope and/or other covariates as significant explanatory variables in the final models can vary from one period to another depending on the effectiveness of their support to improve the fitting performance of the models. Elevation, slope, aspect and all the covariates proposed in this study can influence rainfall patterns and intensities. Their non-selection in the final model does not mean that they do not influence rainfall erosivity. Here, a covariate has the potential to not be retained in the final model in case of multi-collinearity or multi-concurvity between different covariates. From a modelling perspective, the non-selection of one of these covariates significates here, that amongst all the original proposed covariates, there likely exists another covariate showing similar, though more significant mathematical relationships with the target variable. Hence, in the purpose of parsimony, some variables from the original set of covariates are ultimately not included in the final model (section 3.3).

R3-C105: 7240 Line 26: It would be helpful to mention the months of typhoon season again.

R3-R105: The months have been added (First paragraph of section 4.1).

R3-C106: 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?

R3-R106: There are obvious benefits for the readership interested in nuclear accidents in this comparison. That said, researchers purely interested in precipitation and rainfall erosivity may not share this interest. We consolidated the Chernobyl comparison into one paragraph at the end of the discussion on section on implications radiocesium dynamics (4.4) to highlight the differences between the two situations in the discussion.

R3-C107: 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.

R3-R107: This has been updated along with the addition of some other research in the region (Section 3.2 of the discussion)

R3-C108: 7242 Line 24: When there are already two studies on R in the Fukushima region, where is the novelty of your study?

R3-R108: These studies simply quantified the R factor at one location or for shorter temporal periods. This has been explained in text in Section 3.2 of the discussion.

R3-C109: 7242 Line 26: Now I am wondering what is the difference between the study of Shiono et al. (2013) and your study?

R3-R109: The difference was clearly stated in this discussion. Shiono et al., (2013) focused on all of Japan and did not examine the region in a small scale. Further the Shiono et al. (2013) research does not provide any data, maps or an interpretation of results for researchers interested in the Fukushima region in general or on monthly spatial patterns or typhoons in particular. Hopefully, this is now more clear in Section 3.2 of the discussion.

R3-C110: 7242 Line 5-9: This paragraph could be shortened.

R3-R110: This paragraph has been shortened (paragraph 1, section 4.2).

R3-C111: 7243 Line 10/ 11: Are Mexico and Peninsular Malaysia also in the typhoon region?

R3-R111: Typhoons was changed to cyclones in section 4.3.

R3-C112: 7243 Line 17: Why is the Ukraine relevant?

R3-R112: Ukraine is relevant to the context of nuclear accidents (and Ukraine being the other nuclear accident). This is relevant for future researchers of nuclear accidents as rainfall is an important factor governing post-accidental radiocesium dynamics.

R3-C113: 7243 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)

R3-R113: The statement says where other factors are held equal, so if they had the same soils and other factors, then yes this statement would indeed be accurate. Further, there has already been published research applying the USLE in Japan and in the Fukushima region. Nonetheless, this statement has been updated (last sentence of section 4.4)

R3-C114: 7243 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

R3-R114: A results section (3.4) has been added to support our discussion (4.3) on typhoons.

R3-C115: 7244 Line 9: what are “higher volumes”?

R3-R115: This section (on Snow Control) was removed.

R3-C116: 7244 Line 14: this argument is nonsense. This would only hold if you would have measured snow depth.

R3-R116: This section was removed.

R3-C117: 7244 Line 16: which general relationship?

R3-R117: This section was removed.

R3-C118: 7244 Line 17: which ratio? What is “it”?

R3-R118: This section was removed.

R3-C119: 7244 Line 19: Are you mixing snow depth with water equivalent depth? This is nonsense. Delete.

R3-R119: This section was removed.

R3-C120: 7245 Line 21: Wrong unit of annual erosivity, it should be MJ mm ha−1 h −1 yr−1 .

R3-R120: Updated – Conclusions first paragraph.

R3-C121: 7245 Line 24: this conclusion is clearly unjustified because you speculate about R in Chernobyl.

R3-R121: We have updated this with more support based on research in the Chernobyl region.

R3-C122: References Many references have little relation to the study itself and should be deleted. 50% will be sufficient.

R3-R122: We deleted several references from the introduction, though kept ones that were important to our manuscript development or we felt are appropriate. This comment likely regards to this reviewers belief that we should remove the context of this research and also some their stylistic differences. Simply removing 50% of the references seems rather excessive and wouldn't give credit to researchers who have influenced our work. After deleting multiple references, several references were added to further support our response to this reviewer and the other reviewers.

R3-C123: Table 1: What is start and finish? The unit of rainfall should be mm/yr as used in the text.

R3-R123: Table 1 has been updated. Information on the stations has been streamlined and included below as per the comment R3-C37.

R3-C124: 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)

R3-R124: Temperature has been removed and the information on missing data has been summarized in text and the units updated. Table 1 and Table 2 were also combined to avoid duplicating information.

R3-C125: Table 3: poor wording. Rephrase.

R3-R125: This is now Table 2, and the wording has been updated.

R3-C126: Table 4: highly unlikely. Show proof.

R3-R126: This is now Table 3 and it has been updated with difference in AIC scores calculated during the fitting procedure. These results have also been discussed in the results.

R3-C127: Table 5: not necessary; These odd numbers are nonsense anyhow for a classification. Also the reference is missing!

R3-R127: Table 5 has been published and used in several locations elsewhere. Thus we felt it was a good way to classify rainfall erosivity data. Nonetheless we have removed this table per the reviewer's recommendation.

R3-C128: 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.

R3-R128: A new DEM with a higher resolution was utilized. The column headings were improved in the dataset 2 example.

R3-C129: 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?

R3-R129: The stations originally referred to all used at the start of the total (daily and 10-minute) analyses. Now as we use 42 stations and thus this has been updated. We have increased the size of the lettering and also added a clear map of the elevation and the catchment outlines that delineates the area of the spatial mapping analyses.

R3-C130: Fig. 2: I cannot see more than in Table 1. This is redundant. Delete

R3-R130: With the inclusion of typhoon data, the figure differs from the table.

R3-C131: 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.

R3-R131: The running average was recalculated for a 5 year period (to ensure it's centered exactly). We disagree that the figure would be made easier to read with markers rather than columns. Nonetheless as reviewer 2 indicated a preference to include SAI figures as well, we believe the SAI figure improves the readability pertaining to the deviations from the annual mean which is the fundamental portion of the story.

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