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Hydrology and Earth System Sciences
Discussions

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

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REVIEWER: 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.

RESPONSE: Thank-you.



REVIEWER: 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.

RESPONSE: Similarly to our response to reviewer #1, we feel that this is possibly an issue of phrasing. We do believe that understanding the rainfall regime and the patterns of rainfall erosivity both temporally and spatially will help model and manage radiocesium transfers.

There is indeed a long tradition of rainfall erosivity research that strives to help inform 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 factor or a high/mid/low range in papers published in the Fukushima region after the accident.

Our spatially interpreted maps could therefore technically help improve the understanding of radiocesium transfers.

To somewhat repeat our response to reviewer one, and directly respond to this comment, we believe that the text referred to in this comment taken alone may sound misleading. The best response to this comment is the next sentence, when taken together, summarizes our response supported with citations: "The goal of this research was to improve the understanding of soil and radiocesium transfers in a subtropical climate aïňĂected by typhoons. As improved estimates of rainfall erosivity result in more accurate modelling results (Renard et al., 1991; Lee and Heo, 2011), a comprehensive examination of rainfall erosivity will provide a concrete foundation for building a better understanding of sediment and radiocesium behaviour in the Fukushima region."

We definitely are open to more suggestions on how to improve our manuscript, particularly in this section in order to not be misleading (see response to reviewer number 1).

We will work on improving the language of this introduction to be clear and ensure our conclusions and goals are more clearly aligned and well-articulated.

REVIEWER: 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.

RESPONSE: We are not modelling soil loss in our manuscript. We are examining rainfall erosivity. Indeed we are unaware of where we state we are modelling soil loss. Please see response to the reviewer number 1 for this comment as it is addressed extensively 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, we believe a response to this comment and reviewer number one must focus on some of the language and structure in our introduction.

REVIEWER: 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.

RESPONSE: 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 and therefore did not state this process in the manuscript. 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.



Quality control on 10-min data is an interesting topic for debate. We searched extensively for the best-practice for managing potential inhomogeneity in 10min data. Most people we talked to state that homogenization of 10min data is difficult and may actually add more bias to the data than you potentially may remove.

The fact that the data we are using for the 10min rainfall is only a 19-year time series from one series of stations that has not changed over time, 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.

REVIEWER: The slope and statistical significance of the trend are not calculated.

RESPONSE: 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.

We will re-examine this plot and consider adding a slope and value of statistical significance but we generally feel this it somewhat outside the main focus of our analyses. We simply hoped to demonstrate rainfall variation over a long-temporal period with this plot.

REVIEWER: 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.

RESPONSE: We are not trying to examine long-term trends, rather variations. The long-term data was only available for a limited number of stations with lower than average precipitation for the region. Further, the recent paper by Duan et al., (2015) in Clim Dyn does a great job on analyzing the long-term trends in the Japanese rainfall data.

REVIEWER: It would be better to use the SAI (Standardized Anomaly Index) that ex-

presses 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.

RESPONSE: We will definitely look to incorporate the SAI in the formal response to these reviews.

REVIEWER: 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.

RESPONSE: We hope to improve the presentation of the results and the discussion. Any other suggestions would be appreciated.

We did feel that they are somewhat descriptive in a sense they focus the rainfall erosivity in Fukushima in relationship to Japan and the rest of the world, including Ukraine in particular. This may be a style preference to discuss erosivity more than the factors in the spatial analyses.

Further, our discussions were based on other rainfall erosivity papers published in HESS and elsewhere. That said, we definitely would like to improve our manuscript and will focus on incorporating these and other comments that arise in the discussion process.

REVIEWER: 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.

RESPONSE: Thank-you for the constructive feedback. We definitely will do our best to incorporate the feedback throughout the manuscript.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 7225, 2015.