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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. 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. RESPONSE: Thank-you for your encouragement.

REVIEWER: After reading the first two reviews, it is clear that we need to modify the introduction to our manuscript in order to very clearly and accurately articulate our

objectives.

RESPONSE: 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) in another journal 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 provide initial analyses of the rainfall data and also 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 and R-factor data we generated for all the events that are available for rainfall stations within 100km of the Fukushima Dai-ichi nuclear power plant. We believe that this is well within the HESS scope.

Further, we provide comprehensive analyses of the rainfall erosivity data with what we believe are cutting edge approaches to 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 the two papers cited above and many more referred to in our draft manuscript). These 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. Perhaps this is something we need to articulate more clearly in our manuscript. We believe this reviewer likely expected us to model radiocesium and soil migration, when this was not the actual objective of our research. Accordingly we need to adjust the wording of our manuscript to clearly and effectively articulate our research objectives, which would essentially address this reviewer's comments. Individual responses to his comments follow below.

REVIEWER: 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.

RESPONSE: First, the title of the paper reads: “Rainfall erosivity in subtropical catchments and implications for erosion and particle-bound contaminant transfer”

Second it was clearly stated in the abstract that “characterizing the rainfall regime of the fallout-impacted region is fundamental to modelling and predicting radiocesium migration” and, “Understanding these rainfall patterns, particularly their spatial and temporal variation, is fundamental to managing soil and particle-bound radiocesium transfers in the Fukushima region.”

Third, the full citation referred to in this comment from the introduction would read: “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.”

As stated in the manuscript we characterize rainfall erosivity in the Fukushima region to help provide a better understanding of radiocesium behavior. We believe our methodology is appropriate for our analyses and is similar to rainfall erosivity research published in HESS and elsewhere. Importantly, we never state we will quantify soil loss with a USLE model.

Regarding the USLE in particular, there is an entire paragraph in the introduction that speaks to challenges and limitation of USLE approaches (lines 16-28 Page 5). Our response to this section of this comment is found in text on lines 22-28 – Page 5 that states: “A debate on the applicability of the USLE within catchment scale modelling

frameworks is beyond the scope of this current research. What is important is that the R factor and a thorough characterization of the rainfall regime are fundamental to understanding soil erosion and riverine particulate transfers, particularly in subtropical catchments subject to cyclonic activity.”

Ultimately, our manuscript examines rainfall erosivity. We don’t apply a soil erosion model which we thought was clear throughout the introduction and conclusion. If the editors, reviewers or others joining the discussion have suggestions on how to improve this communication issue, we would be appreciate constructive comments and criticism.

REVIEWER: 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.

RESPONSE: Again, we don’t apply the USLE in this paper. We examine rainfall erosivity. Further we do state, as noted above, that there are limitations for applying the USLE at the catchment scale. As we don’t apply the USLE and have references regarding its limitations at the catchment scale already included in text, we do not feel it’s necessary to respond to this comment in our reply.

REVIEWER: 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 (!)

RESPONSE: Again, 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. This paper is a rainfall erosivity paper that analyses and investigates the R-factor while

characterizing 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 paper 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.

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 mapped rainfall erosivity. These models incorporate a standard number for rainfall erosivity or a mid, a high and a low rainfall erosivity value.

Here we provide not only the data, but also spatial maps of rainfall erosivity which will help more accurately model and predict rainfall erosion. Maybe, in countries with less temporally and spatially variable rainfall, one may not think this is important. In Fukushima, we would argue, it is fundamental.

REVIEWER: 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) We think this is simply a matter of terminology.

RESPONSE: Indeed, the C-factor in the RULSE is specifically a cover management factor. Going back to Wischmeier and Smith (1978, Page 17): "Factor C in the soil loss equation is the ratio of soil loss from land cropped under specified conditions to the

corresponding loss from clean-tilled, continuous fallow." We believe that the difference between our "soil erodibility based on land cover" and Wischmeier and Smith's (1978) "ratio of soil loss from land cropped. . ." is simply a matter of terminology. In response to this reviewer we could change "soil erodibility based on land cover" to "ratio of soil loss based" on land cover and include the Wischmeier and Smith (1978) citation.

REVIEWER: 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), 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.

RESPONSE: Again, we actually don't apply an erosion model in our paper. We provide analyses of rainfall erosivity. Maybe if the reviewer could inform us where we directly mislead him in text we could address this in our response to the review.

We do believe that this reviewer should be cognizant of rainfall erosivity research as one of his co-authors on the recommended G2 model in this comment is the author of the two rainfall papers we referred to in the introduction of this discussion response.

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 we referred to 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 $\sim$19 years, for 40 station 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 hopefully publishing our work in HESS, researchers such as this reviewer,

will be able to access this data and potentially apply their G2 model in the Fukushima region as he did with the data from Panagos et al., (2015) in Cyprus in the link provided.

Essentially, the majority of the comments from this review were directed at a USLE modelling manuscript. Here, we presented 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.

We definitely would be open to suggestions on how to correct this misdirection in our manuscript. In particular, we will examine the two rainfall erosivity papers that this reviewer cited and derived his modelled R-factor data from and see if we can improve the terminology in our paper and be less misleading. Our apologies.

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