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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Seasonal Dynamics of Rainfall Erosivity in Switzerland" *by* S. Schmidt et al.

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Dear Anonymous Referee #3,

We highly appreciate your critical view on our submitted manuscript and are very pleased that you evaluate our manuscript of being of good quality and of "great interest for applications involving the soil loss and agricultural productivity". We appreciate your concerns, which we will consider in the revised version. Here, we would like to provide only a short comment to answer your main concerns, but we will upload a detailed author comment after we received all reviews.

Referee #3 suggested describing the used methodology and its application in greater detail (key point 1-).

Referee #1 already raised a similar request. We are going to explain the implementation of the regression by generalized linear model and ordinary kriging more detailed in the revised manuscript.

It was criticized by the referee that the title term "dynamic" is not clarified in the manuscript (key point 3-).

We use "dynamic" as an umbrella term for temporal variations of rainfall erosivity which are studied in the present manuscript. Referee #1 also suggested an adjustment of the title (doi:10.5194/hess-2016-208-RC1; doi:10.5194/hess-2016-208-RC2). We will adjust the title in the revised version taking into account the concerns of the reviewers. We propose a revised title for the manuscript and would be pleased to receive your feedback on it: "Regionalization of Monthly Rainfall Erosivity Patterns in Switzerland"

Referee #3 recommends the integration of further bibliography or a clear statement concerning the extent of references used (key point 4-).

The focus of the presented literature review lay on rainfall erosivity studies covering the area of Switzerland. Further, monthly or at least seasonal R-factor mapping studies on national and regional scale were considered. We are aware that we did not cover all studies. A quick search for "rainfall erosivity" on web of science yielded 661 results as such we had to narrow our literature review. Nonetheless, we will carefully check whether we missed studies that fall into the above-described scope and we will follow the suggestion of the reviewer to define the scope of our literature selection in the revised manuscript.

Next, the practical applicability loses some relevance due to the missing discussion of low coefficients of determination (key point 5-).

We yielded low R^2 of 0.10 for November and 0.26 for December. As stated in line 7 to 12 of page 7, that low rainfall erosivity values, as a result of low-frequency events, are difficult to predict by averaged precipitation covariates. This was also observed for

Greece (Panagos et al., 2016, p. 165). Regarding soil erosion risk assessments, we believe that the low R^2 in winter will not limit the practical applicability since both the erosivity and its spatial variability are very low in winter. We will extend the discussion on this point.

As Referee #2 already notes, the difference and improvement regarding the previous work of Meusburger et al. (2012) is not clear enough (key point 6-).

We would like to refer to our comment on the first remark of "Short Comment to RC #3 by Referee #2" (doi:10.5194/hess-2016-208-SC2).

Finally, Referee #3 marked and commented some inaccuracies, specific errors, and formatting issues in the supplement material (key points 2- & 7-).

Thank you for these valuable remarks. We will certainly consider them in the revised version of the manuscript.

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