
Comment 1: The authors rightly take the EI₃₀ measure as the reference given its wide-spread use. However, they fail to discuss properly the kinetic energy component of this indicator, and the issues of measuring/estimating it. The kinetic energy of rainfall can be measured (e.g. with disdrometers), but given the non-availability of such measurements for most stations, mostly it is estimated based on empirical equations. The authors simply present equation (2) but fail to give a rationale for it. Other studies exist that compare various existing empirical relationships (e.g. van Dijk et al, 2002, Journal of Hydrology 261, 1-23 and see also Salles et al. 2002, Journal of Hydrology 257, 256-270), and should at least be discussed here.

Response: We agree with the comment and we will add a short discussion of those references in the revision version.

Comment 2: It is not always very clear which erosivity values are taken as input for estimating the modelling error. E.g. if the authors refer to monthly, is this always “average monthly”? If so, why, and would it not be more useful to look at erosivity values for individual months? This would relate better to the ongoing discussion on ways forward for erosion monitoring (e.g. Vrieling et al, 2014 Global and Planetary Change 115, 33-43).

Response: “Monthly” in the manuscript refers to the erosivity values for individual month, which is $R_{\text{month}, y, m}$ in the Equation (6). “Average monthly” is $R_{\text{ave_month}, m}$ defined in the Equation (7).

$$R_{\text{month}, y, m} = \sum_{j=0}^J (EI_{30})_{y, m, j} \quad (6)$$

$$R_{\text{ave_month}, m} = \frac{1}{Y} \sum_{y=1}^Y R_{\text{month}, y, m} \quad (7)$$

where $(EI_{30})_{y, m, j}$ is the EI₃₀ value for the j^{th} event in the m^{th} month of the y^{th} year; $R_{\text{month}, y, m}$ is the R value for the m^{th} month of the y^{th} year; $R_{\text{ave_month}, m}$ is the average R value for the m^{th} month over the years of record.

We will clarify them in the revision and add more discussion on the potential use of erosivity values for individual months in erosion monitoring.

Comment 3: In relation to the last point, I would encourage the authors to contribute to this discussion and (based on their results) give more concrete recommendations for ways forward. Currently the authors refer in a very general way to “users” in their conclusions. In my view, end-users are never those that want just to make an estimate of erosivity, but rather they need erosion estimates and possibly a monitoring framework, e.g. for planning purposes and impact evaluations. Adding a clearer opinion on how to move forward with erosivity analysis, including its embedding in mapping/monitoring frameworks, would be a welcome addition to this manuscript.

Response: Sure we can do that in the revision version.

Comment 4: While the research seems well-embedded in existing erosivity estimation efforts in China, in my view the authors could make a better link with other ongoing efforts in other areas that look at different temporal resolutions of rainfall data. I am thinking for example about Panagos et al (2015, Science of Total Environment 511, 801-814) who normalize R-factor estimates for Europe based on recording intervals. Although the authors focus on rainfall station data, another line of research (i.e. application of satellite rainfall estimates) should be acknowledged, i.e. work by Vrieling et al (2010 in Journal of Hydrology, and 2014 cited above), but also for China (Fan et al, 2013, Journal of Mountain Science 10(6): 1008-1017). This is especially relevant for end-users that require spatially-consistent information on soil erosion. In fact, a performance evaluation for the stations in the manuscript of erosivity estimated from satellite rainfall products could be a nice follow-up study for the authors (but probably not for this paper).

Response: We will reference and discuss these papers in the revised manuscript.

Comment 5: Perhaps I misunderstood something in the paper, but it seems to me that the models are only evaluated for the temporal scale to which they are applied. In Tables 3 and 4, the event-based models are only evaluated on the basis of events modelled. While there is nothing wrong with that, I would also expect the models to be evaluated at the aggregate scale. I mean that EI_{30} estimated from event-based models should also be added up to monthly and yearly values, to evaluate if fine-scale temporal resolution data improves also the accuracy of aggregate erosivity measures.

Response: We will clarify this in the revised manuscript.