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Hydrology and Earth System Sciences
Discussions

Interactive comment on "How rainfall event characteristics affect the applicability of I_{30} as an index of intense or erosive rainfall: a brief review with proposed new rainfall index" *by* David L. Dunkerley et al.

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Response to Anonymous Referee #2

Referee #2 raises three broad concerns. I address each of these in turn.

1. The reviewer suggests that in my paper I analyse I30 as a 'climatic parameter'. This is simply incorrect. The rainfall index I30 can be determined for one or multiple rainfall events, as originally done in the work of Wischmeier in the development of the USLE empirical soil loss model, and now widely used in hydrologic applications unrelated to

soil erosion. Rainfall event data comprise the evidence employed in the analyses of I30 that I present. This is all completely normal practice. Short-term rainfall intensity variation can certainly linked to climatic conditions but this is frequently not considered in analysis of indices such as I30. For instance, the seasonal and diurnal variation of I30 and similar parameters are commonly overlooked, and the mean value of I30 reported merely as an index of storm rainfall. This is sometimes done using data from only selected months, rather than entire years. Much could potentially be learned by examining the seasonal and other variations that are associated with I30 and related indexes, in order to build a more complete picture of the climatology of rainfall events. As an example, the annual maxima in short-period rainfall rates most frequently occur in the summer months, and indeed often in the late afternoon of summer months. Consequently, indices of short-term rainfall intensity cannot be regarded in any sense as reflecting conditions through the other periods of the year, and certainly not the overall climate of a place. Nevertheless, daily maxima, or monthly means, or annual maxima, of indices such as I30 can validly be calculated.

The reviewer also suggests that my paper criticises I30 as a climatic parameter. This is incorrect. What I do suggest is that among contrasting climates, where rainfall events may have strongly contrasting durations (such as Fowlers Gap and Millaa Millaa, the two field sites used in my paper) I30 may fail to reflect the climate-related differences in short-term extreme rainfall. As I demonstrate in the paper, this is at least in part because I30 reflects on average almost 10% of the 5.1 hour mean event duration at Fowlers Gap but only 2.7% of the mean 18.6 h events at Millaa Millaa. The I30 index thus reflects different percentiles of the rainfall intensity during events at the two locations, and cannot validly be compared between sites. As I argued in the paper, for events that are brief and for which 30 minutes is close to the event duration, I30 is in fact close to the mean rainfall intensity, and fails as a measure of the extreme intensity during an event. In summary, one difficulty with I30 that I attempted to explore in my paper is that it may reflect characteristics ranging from the mean intensity of rain during an event to perhaps the wettest few percent of the event duration; where the index falls

in this wide range varies from event to event, as well as differing between geographical locations with relatively short rainfall events and those, such as Millaa Millaa, that have long, orographically-enhanced rainfall events that may last > 200 h. In the case of such long events, I30 reflects the rainfall during the wettest ∼0.2% of the event duration. This results in I30 values at Millaa Millaa that appear numerically similar to those from Fowlers Gap, where I30 values reflect the depth of rain received in a larger fraction of the event duration.

Under the heading 'wrong concept', the reviewer appears only to refer to the estimation of erosivity. In contrast, my paper discusses I30 and other indices as descriptors of short-term rainfall intensity. As noted in the paper, areas of application of such indices include urban drainage and flash flooding; these have nothing whatever to do with erosivity. The I30 index does not have to be multiplied by kinetic energy, as the reviewer suggests, but is widely-used as a 'stand-alone' rainfall parameter. I make this clear in the paper.

2. The reviewer suggests that I30 can be readily calculated for rainfall events shorter than 30 minutes. Indeed the reviewer suggests that this is 'especially easy', since I30 can be found simply by taking '.. twice the total rain amount'. I have to confess being unable to follow the reviewer's argument here. I30 is defined as the wettest 30-minute period during a rainfall event. If the rainfall lasts for a shorter period, I30 simply cannot be calculated. Taking 'twice the total rain amount' and in some way processing this does not appear to constitute a rationally-based procedure. I am not aware of any published work in which the procedure advocated by the reviewer has been adopted.

The reviewer additionally suggests that it is difficult to calculate EDf5 for a 15-minute rainstorm. On the contrary, using the data presented in the paper, this is quite straightforward. At the Millaa Millaa field site, for instance, 36.5% of the 45,737 inter-tip times of the tipping bucket rain gauge were less than 60 seconds in duration, and about 30% were of shorter duration than the 45 seconds referred to by the reviewer. This amounts to approaching 14,000 tip events, which presents no restriction on the analysis of inten-

sity over periods of less than 1 minute. Indeed, even if there were 5 bucket tip events in 45 seconds, this would represent an intensity equivalent to 80 mm h-1, which is not exceptional in the records analysed my paper. Peak intensities were in fact > 200 mm h-1.

If I understand their comments correctly, the reviewer seems to suggest that my analysis should be tied to the original procedures as used by Wischmeier 70 years ago. That work involved identifying erosive rainfall events as a subset of all rainfall events, by specifying a minimum event depth of 0.5 inch (12.7 mm) for the selection of rainfall events to be used to calculate the R factor, using a minimum inter-event time of 6 h. Wischmeier additionally included in the analysis of I30 any potentially erosive any events having less than 0.5 inch of rainfall provided that they included rainfall of at least 0.25 inch (6.35 mm) in 15 minutes and resulted in some overland flow. In fact, in contemporary practice, R factors are not often calculated in the way set out so long ago by Wischmeier. There are many reasons for this, not least the very limited availability of E (kinetic energy) data, which are far less readily available than rainfall data. Likewise, the empirical procedures used by Wischmeier were based on data from the soils and topographic conditions of experimental erosion plots located in the USA, and limited to sites east of the Rocky Mountains. Differing conditions elsewhere make these restrictions inappropriate as universal parameters. For instance, the 6-hour inter-event time was selected by Wischmeier because of the rate at which soil infiltration rate recovered following the cessation of rain, which is clearly a function of local soil, cover, and climatic conditions. In any case, my discussion was not focussed on the USLE, but rather on the use of indices such as I30 and EDf5 as stand-alone measures of intense rainfall. The reviewer argues that I used a 6 h MIT (correct, I did so) but ignore the other two criteria associated with the EI30 factor in the USLE. I do not use the criteria of Wischmeier (1959) because my paper is not a discussion tied to the USLE. As noted earlier, in my paper I explore the use of I30 and EDf5 as stand-alone indices of rainfall intensity. I therefore think that the comments concerning the procedures related to the USLE are misplaced.

3. The reviewer feels that my analysis is flawed because it does not use at least 30 years of data.

The reviewer argues that there are "errors in the calculation as it was done ignoring thresholds, ignoring weighting, ignoring short rains". The reviewer thus appears to think that the references to I30 made in my paper relate to EI30 as it is used as a term in the USLE. This is simply not the case. As the literature amply demonstrates, measures of short-term intensity such as I30 are widely used as stand-alone measures of rainfall intensity, without regard to restrictions of the kind imposed by Wischmeier in the development of the USLE 70 years ago. Specifically, in the potential applications that I explored in the paper, there are no required thresholds of storm rainfall depth or intensity. This is the sense in which I discuss measures such as I30 and EDf5: they are potential indices that may be useful in characterising important aspects of storm rainfall, especially in relation to soil erosion and the generation of overland flow, but also in relation to urban drainage and flooding problems, which quite clearly have nothing to do with soil erosion and the USLE.

I do not accept the reviewer's suggestion that a 30-year period of record is required for the analysis of short-term rainfall intensity data, for three further reasons. First, my paper deals with event-based I30 data, and in no way attempts to present a long-term 'meteorological' view of rainfall at the field sites from which the rainfall data come. As it is frequently used, I30 is derived from quite small data sets, often from just a handful of rainfall events, in studies of post-wildfire runoff and erosion, for instance, or of the causes of particular urban flash flood events. Second, in the context of environments with marked inter-annual variability related to ENSO and other phenomena (this includes the Fowlers Gap field site used in my paper), long-term averages conceal important temporal variability, both seasonal and inter-annual. Third, ongoing climatic change and variability are important reasons for thinking it unwise to employ 30-year records as indicators of rainfall character at the present day. The widespread re-calculation of IDF curves internationally, to update our capacity to predict short-term

rainfall extremes and manage related urban drainage problems exemplifies this. It remains unclear how I30 and other indices of short-term rainfall intensity are changing, and how they might change in coming decades, partly because there is only a small number of studies that identify the temporal context of such measures, including their diurnal and seasonal variability. As carefully noted in my paper, multiple field studies have shown the importance of even shorter-interval measures of intense rainfall, such as I10 and I15. For work seeking to understand the changing character of short-term rainfall extremes, parameters in addition to I30 (such as I10 and I15), as employed in these and many other studies, or EDf5 as proposed in my paper, appear to be necessary. Indeed, a major purpose of my paper was to explore the idea that reliance on a fixed, arbitrary clock period such as 30 minutes (used without regard to the temporally and geographically changing duration, intermittency, and other characteristics of rainfall events) may not be the best or the only approach in seeking to understand what is happening to erosive and hydrologically-important short-period rainfalls. The event-based character of indices such as I30 means that the local rainfall event characteristics provide an important context for their use. For instance, the length of the enclosing rainfall events might influence processes such as overland flow by affecting the antecedent soil wetness at the time I30 was recorded. The position of the I30 or EDf5 time periods within the rainfall events would thus be important – whether, for instance, they occurred relatively early on drier soils or late in the event on much wetter soils (an issue, incidentally, noted by Wischmeier in 1959 in relation to soil erosion but still awaiting a systematic analysis). There has been very little investigation of such phenomena. The important point is that research in contexts such as post-fire erosion cannot sensibly be based on 30-year observation periods: what is important is the nature of the rainfall in the days and months following the fire. Again, therefore, I do not accept the reviewer's argument that a 30-year record is some kind of pre-condition for the appropriate and informative use of statistics such as I30 or EDf5. They are not 'climatic parameters' requiring such long records; rather, they can validly be calculated for a single rainfall event if that suits the particular research need being addressed. I

would argue that it may be helpful to consider indices other than I30, perhaps including EDf5, in exploring such issues.

Finally, the review argues that in some way I30 and EDf5 are analogous to MAT. The reviewer suggests that whilst several locations might have similar values of MAT, they might differ in other climatic parameters like mean annual precipitation. This may be so. However, the argument raised in my paper is this: the dryland Fowlers Gap site is characterised by relatively short, intense (largely convective) rainfalls. In contrast, the Millaa Millaa site is characterised by much longer, but less intense, rainfalls. Nevertheless, owing to the differing fractions of the rainfall events included in I30 at each site (see earlier discussion), the values of I30 from the two sites are indistinguishable and thus do not reveal the differences between the two locations in terms of rainfall character. This is not a question of differing climatic parameters (e.g. rainfall and temperature), as the reviewer suggests, but one of different indices characterising the rainfall in inconsistent ways. The proposed new index, EDf5, does in fact successfully differentiate between the two field sites, and echoes the intensity and duration characteristics of each.

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