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Interactive comment on "On the Use of Measurements from a Commercial Microwave Link for Evaluation of Flash Floods in Arid Regions" *by* Adam Eshel et al.

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First, we would like to thank anonymous referee #1 for his review.

Comment #1: Lack of control case: The main goal of the paper is to introduce a new method for combining CML and radar statistics to help identify potentially dangerous conditions for flash floods. The radar is used to assess the spottiness of the rain along a fixed path (using kurtosis k) and CML are used for rainfall intensity estimations. In itself, this is not a bad idea. But how do you demonstrate the value of such an approach? For starter, you need a benchmark against which the proposed technique can be evaluated. Secondly, you need a formal decision rule for distinguishing between

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Discussion paper

dangerous situations and normal ones. None of this is done by the authors. For these reasons, it is impossible to know whether the method has intrinsic value or not. For example, it could be that the CML data are not really improving the detection compared to radar alone. Or conversely, spotiness is not really needed to detect dangerous situations. More evidence and formal testing is needed to support the strong claims made by the authors.

Response to comment #1: We thank anonymous referee #1 for the careful compliment that our approach "is not a bad idea". Indeed, our new method is not an attempt to introduce the method as an on-the-shelf ready method, most importantly because the link data are presently unavailable online, whereas the ability to identify dangerous situations requires online availability, such as rainfall recorder and radar backscatter data. Rather, we introduce a concept that requires further development for implementation. Having said that, both the concept and the methodology are novel. As such, we contend that they are eligible to be brought to the attention of the public.

It has previously been shown that estimating rainfall by the use of CML is accurate, but the “long isolated link challenge” has not yet been considered. Doing so in an area where rain cells are much smaller than the link’s path emphasizes the challenge. The uniqueness of the studied area largely relies on highly spotty rainfall and strong, very localized rain bursts, characteristics which were aimed to be highlighted as they play a major role in desert surface hydrology. The study initially involved no radar data, but as integrated rain intensities along 16 km failed in providing sufficient information with regard to runoff response, we included the distribution along the link.

The challenge dealt with in this study is the insufficiency of radar in remote, dry mountainous areas as well as that of long CML when exposed to the aforementioned rain characteristics. Thus, consideration of the spatial distribution (rather than the quantities) of radar cells in combination with ground level observations (which have their own limitations) brings forth the strengths of each instrument. This is the justification.

Comment #2: No assessment of false positives and false negatives: One important aspect to look at when trying to demonstrate the value of a detection technique is hit rates and false alarm rates. How good is the technique at detecting rises in the hydrograph and how often does it fail? The paper mentions the case of January 11th 2015 where the radar was not working. On this day, a considerable water level rise was noticed but the link did not record any significant attenuation. So maybe the link in itself is not such a good predictor and most of the useful information is coming from the radar? Also, maybe other characteristics derived from radar such as spatial coverage of rainfall over surrounding regions would be more useful than the CMLR?

Response to comment #2: Miss detect and false alarm cases, by which one can effectively determine the contribution of the approach, will add to the solidity of our approach. The reasons for the absence of such tests are as follows: Data scarcity. As significant rain events in the region are rare (very few a year, if at all) there are simply insufficient data at this stage. For instance, Wadi Ze'elim has been monitored merely since 2015. In addition, finding a hydrologically monitored catchment with links in and/around its basin which store data in a suitable format (in our case fully dependet on the cellular services provider) is impossible in most locals. It has been shown that real-time, countrywide data possession is possible (Chwala et al., 2015). Therefore, at this stage the manuscript aims to demonstrate an idea which can potentially be developed for application.

Weather radars do provide spatial coverage, as limited as they are in these areas; these were obviously used only over the CML and not over the entire catchment. The contributions and limitations of radar in catchment hydrology are well known. Nevertheless, to demonstrate the triangular connection: radar-CML-flash flood, as a "concept demonstration", our agenda is to put the CML itself and the variations within it's path in focus, and thereby to extrapolate to the general case in the future. The suggestions proposed by the referee should be included in a future study which, albeit the difficulties should include additional catchments, more CMLs and additional focus on utilization of radar capabilities.

Comment #3: Poorly detailed methodology: the whole methodology for deriving the rain-indued attenuation from minimum and maximum transmitted/received signal levels is sketchy. There are 3 critical parts in the method: (1) the derivation of the baseline and wet antenna attenuation, (2) the removal of the quantization bias due to min/max and (3) the power law transformation. All aspects are poorly explained, with multiple references to non peer-reviewed conference papers. For these reasons, I think it would be good to give more details on the technical aspects of the methods used to retrieve the rainfall from the microwave link.

Response to comment #3: We thank the referee for illuminating the need for further detailed explanations. This chapter will be expanded. Concerning the quality of the relevant cited conference papers, all are full-length, peer-reviewed conference proceedings. In particular,ÂăICASSP (International Conference on Acoustics, Speech, and Signal Processing) is a major IEEE sponsored conference, and is the main annualÂăvenue for the IEEE SPS (Signal Processing Society). The ICASSP proceedings rank as the 4th top publication for the signal-processing category, with an H5 index of 67 (https://scholar.google.co.il/citations?view_op=top_venues&hl=en&authuser=1&vq=eng_signalprocessing).

Comment #4: Flawed baseline estimation method: In Section 3.2 Equation (5), the baseline (including wet antenna) proposed here is min(Amin(j-1),Amin(j)), which is a running minimum over the last 2 minimum attenuation values (i.e., corresponding to a time window of 30 min). The authors justify this approach by citing the paper by Ostrometzky and Messer, 2017 (in press). However, this reference turns out to be almost identical to another paper submitted to IEEE Transactions of Geoscience and Remote Sensing back in 2016, which at that time was rejected unanimously by all reviewers (including myself) for its multiple statistical fallacies and methodological weaknesses. It seems like the authors persisted despite the valid criticism and got their flawed paper published almost “as is” in another journal. Back in 2016 when I reviewed this paper, I pointed out that one of the crucial assumptions behind the technique was that the attenuation C3 ACPD Interactive comment Printer-friendly version Discussion paper measurements needed to be independent from each other. Moreover, the number of samples in the running mean needed to be large enough. Here, the method seems to be applied for the case n=2 (30 min) which, given the temporal dynamics of rainfall, means that two successive attenuation measurements will be highly correlated. As far as I see it, this is a clear violation of the assumptions behind the method. Please justify the approach or choose another more technically sound baseline estimation method.

Response to comment #4: This is a very interesting topic of using CMLs in different climate regions. n=2 (last 30 minutes) as a dynamic window of the minimum attenuation might be more debatable in high latitudes regions, but in a rain shadow area it is reasonable, to our opinion, especially with the very high spatial as well as temporal variability. Moreover, it is mentioned that the min max levels are constructed based on 10 s measurements. Thus, theoretically the max values of two successive measurements can be very close time wise but also 30 min apart, which makes them highly non correlative with respect to desert meteorology. We also tried n=8, with no significant difference detected from n=2. Regardless, additional methods of derivation of rain intensities were attempted, including: (a) subtraction of the median of the average values of min&max received signal levels in the past 24 h (Overeem et al., 2011), and (b) subtracting a 2.3 dB wet antenna according to the procedure detailed in Overeem et al., 2013 (including a local optimization of alpha). The chosen method appears to be more suitable for the area, most likely due to the different climatologic character than in the aforementioned studies. But finally, the rain derivation method is not the scope of the paper. The rain intensity in this study is a tool of demonstrating the focal point, so as long as it is derived by published methods, it serves the purpose. Discourse on rain derivation methods is important. Therefore, a quantitative comparison should definitely be conducted in the future.

Comment #5: Confusing discussion about outliers and change points: I found Section

7 to be very confusing and speculative. In particular, I could not follow the convoluted arguments given by the authors for justifying why 2 data points were removed from the analysis. Please provide a clearer more solid explanation for this. Moreover, I don't see any strong reason why one could assume that an upper CMLR threshold exists after which the CMLR-k relationship changes. If you think this is the case, please provide hard evidence in the form of an extra statistical analysis of kurtosis-rainfall relationships or give a mathematical derivation supporting the claim. Otherwise, this just looks like you are removing data points that do not support your theory.

Response to comment #5: Indeed, an interesting comment. The two "outlier points" are a product of the same event, in fact the spotty most event. We are thankful to possess these points as they strengthen the suggested theory that a further research (when sufficient data are available) should approach this issue using envelope curves or pattern recognition / non linear logistic regression, to state thresholds.

The claim of a threshold beyond which the CMLR-k relations loosen, in regard to runoff generation, derives from the physics of infiltration rates and ponding times. For the general case, a rain intensity lower than the lowermost value of the hydraulic conductivity of a given soil, will not generate runoff. When rain intensity is averaged over 16 km, this "lower value" doesn't imply that there are no areas along the line which exceed this value. This is where kurtosis comes in. For average rain rate values exceeding the hydraulic conductivity for minimal runoff generation, the distribution along the link plays a minor role (at least when classifying floods binarily) as it is theoretically possible that somewhere along the path the rain intensity is sufficiently high to generate runoff, regardless of the spatial distribution.

Comment #6: The quality of the evidence presented in this paper does not support the strong conclusions made by the authors: - For example, the sentence: "The long isolated CML used in conjunction with additional information collected by weather radars is of beneficial value for surface hydrology" is not backed by any data. The evidence you have is circumstantial, showing that pairs of k-CMLR for some selected events are loosely connected to hydrological response. But the relation between the two is not systematic and your analyses do not show you well this works, or how often it fails. This is essential for knowing whether it adds value or not. - Also, the statement that "It was shown that, even when the radar is located at great distance, with complex terrain and without calibration, radar can be used to complement the ground level observations of the CML in determining the ripeness of conditions for flash flood responses." is very misleading. In fact, you do not show any results without the CML. So how can you know whether the combination of CML and radar improves results compared with the control case where you just consider radar without the CML? Please reformulate or provide a control case where the CML data is not considered. - "Therefore, flash flood warning systems can possibly be improved through this approach." This is speculative. Please remove or show examples of applications where it helped improve flash flood warning.

Response to comment #6: We acknowledge that the sentence: "The long isolated CML used in conjunction with additional information collected by weather radars is of beneficial value for surface hydrology" will be written in a less confident manner.

Regarding the sentence: "It was shown that, even when the radar is located at great distance, with complex terrain and without calibration, radar can be used to complement the ground level observations of the CML in determining the ripeness of conditions for flash flood responses". We disagree with the referee's statement that this sentence is "very misleading". The horizontal axes of Fig. 8 demonstrate that the CMLR alone provides very little information about the expected hydrologic response, mostly due to it's length. It is prominent that the addition of radar data enhances the CML observations. Furthermore, and as shown, there is at least a 2 hour gap between the CML detection until the response of the river occurs at the outlet. This time window is crucial for people living in such areas, when preparedness at short notice is crucial. Calibrating radar data is time consuming, and using quantitative values obtained by it in such an area is insufficient, finely summarized by Brene and Krajewski, 2013.

Regarding the sentence: “Therefore, flash flood warning systems can possibly be improved through this approach.” The word “possibly” makes this sentence speculative in nature. Nonetheless, this is our speculation, and we will amend or remove the sentence if necessary.

References

Chwala et al., 2015.

Overeem et al., 2011.

Overeem et al., 2013.

Brene and Krajewski, 2013

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