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Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #1

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Summary:

A Method for combining microwave link data with radar statistics to identify conditions in which flash floods could develop is presented. The method is applied to a 16-km link and 5 storms over the Wadi Ze'elim catchment in the western part of the Dead Sea rift. Radar is used to quantify the spottiness of the rainfall field while the microwave links are used for rainfall estimation.

Recommendation:

I sincerely doubt that the method proposed by the authors offers added-value for flash flood detection and early warning applications. Most of the paper is speculative in

nature, with little hard evidence to support the conclusions. In particular, I was not convinced by the authors' strong claim that the combination of k (kurtosis) and CMLR leads to useful predictions for flash flood warning and surface hydrology. For example, I do not understand why the commercial microwave link plays such an important role in this story. The same radar data used to derive the kurtosis k along the link could actually be used to derive rainfall estimates as well, probably leading to similar results in terms of flash flood prediction. Sure, maybe the radar-derived rainfall estimates might not be as accurate because of height differences and other radar-related errors. But large radar returns in the region combined with strong spatial variability might still be a pretty solid warning sign for local flooding. So how much do you actually gain by including the microwave link into the method? The paper provides no insight into this, nor does it try to formally assess the usefulness of the method compared with other more traditional ways of doing these things. In summary: the scientific evidence falls short and frankly, does not convince.

Major Comments:

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 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.

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?

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.

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

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.

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.

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.

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