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Interactive comment on "Impact of karst areas on runoff generation, lateral flow and interbasin groundwater flow at the storm-event timescale" *by* Martin Le Mesnil et al.

Anonymous Referee #2

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The authors present an interesting study about karst characteristics of catchments at storm-event timescale. The merit of the study is the treatment of multiple sites and the storm-event time-scale, where the case study region in France provides a good example and good data.

The overall article is well written and of scientific interest, but the scientific argumentation should be improved. The literature study is very short and lacking some other approaches, the research question is not clearly stated, the concept is not sufficiently brought into the context of other methods, and the benefits and shortcomings of the developed method against other methods should be better elaborated in the theoretical

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part, and later on discussed based on the results.

In the literature study, other concepts for describing the hydrological characteristics of larger scale karst systems should be mentioned. Gárfias-Solizet al. (2010) found system memory, response time and mean delay between input and output as indicators for karstification but emphasized that the physically founded regionalization needs to account for structural complexity, heterogeneity of the lithology and the degree of karstification. Hartmann et al. (2013) described the responsive behavior of large-scale karst systems by system signatures derived from hydrodynamic and hydrochemical observations based on data from five different karst systems. Basha (2020) presented six recession curves for the classification of karst aquifers, describing the dichotomy of the dual flow characteristics between the fissured matrix and the conduit system. How do the descriptors found in this study relate to the indicators and signatures found by other authors? Is there a clear difference between daily and storm-event resolution at the size of catchment under consideration?

“Despite some studies describing significant IGF in karst areas (Le Moine et al., 2007; Lebecherel et al., 2013), the specific issue of IGF in karst has not been addressed as such.” There have been studies dealing with IGF, in particular the modelling of IGF at catchment scale or adding IGF to hydrological catchment models. Even if karst-based IGF is often neglected, it found attention among hydrological modelers, introducing or enhancing karst capabilities of e.g. SWAT (e.g., Nguyen et al., 2020). Apart from modelling and data analyses, geo-chemical methods based on analysis of chlorides and stable isotopes has been used to characterize karst systems.

The authors correctly mention that many hydrological studies neglect the characteristics of karst regions, but their own catchment delineation follows the “classical” orographic approach using surface water divides instead of trying to derive subsurface catchments based on hydrogeological data/maps. This is a major shortcoming of the method. In particular, the water balance descriptors could be more meaningful if the overall contributing area including the subsurface catchment would be regarded.

Whenever catchment sizes and rainfall amounts are related with stream flow, there would be a mismatch in karst regions and it is unclear for me how the authors have regarded that aspect. A detailed discussion is missing. In particular, for the description of storm peak flow, I would expect water balance descriptors, which include the areas connected by IGF. For arid basins, Wolaver et al. (2008) developed a method to delineate karstic aquifers based on data. Applying such delineation beyond the usage of surface catchments might help to improve the results of the characterization of the flow response to storm events. Here, one could think that the method is inconsistent and I ask the authors to explain why they did not try to delineate the karstic aquifers and include them into the water balance considerations.

The conclusions seem to ignore the efforts done in karst research over the last decades: “Existence of karst hydrological specificities has been known for decades, but is poorly quantified and documented.”. Here, I disagree. The authors should explicitly say, where their method provided methodological innovation and which characteristics of karst regions could be now better described. Literature cited:

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