This paper presents an analysis of some high-quality rainfall and streamflow data collected in two adjacent catchments in Luxembourg (the Ernz Blanche basin). The objective, as reflected in the title of the manuscript, was to understand better the hydrological mechanisms resulting in flash flooding in this catchment. The paper is generally clear and straightforward to read, though I think that the main focus should have been more strongly on large rainfall events than on hydrologic response under more usual events.

We thank you for you review that was really helpful to improve our manuscript. Our main change according to your comments were:

- to include the 2016 and 2018 flash flood events in our study. The unit hydrograph model was applied on those two flash flood events. We can then connect our results on moderate rainfall-runoff events in the flash flood context. We hope also, that some rewording will help you to see the connection of our study with this scope.
- to insert a sensitivity analysis into the supplementary materials, in order to argue the small impact of our constant RC assumption on our results.
- Present more cautiously our explanations for the impact of the hydrophobicity of forest litter and the soil surface. We acknowledge that we do not have data to validate these explanations, but we believe that this open discussion could be beneficial to the scientific community, by opening up hypotheses to be tested.

In addition, we have taken advantage of this rewrite to add an event year to ensure the consistency of the correlation analyses and to work on two contrasting hydrological years (one being rather dry, the other rather wet).

Here below, we answer to each of your specific comments.

Oddly, though the authors mention the occurrence of several historical flash floods, including one in 2016 and another in 2018, they do not describe those events in any detail. They provide no discharge data, no runoff coefficients, and no rainfall event data. In order to find something of these events, I consulted an EGU Abstract (Iffly et al. 2018) by some of the same authors. There, I was able to learn that the 2016 event had much more intense rainfall than anything that the authors investigate in the present ms., recording 20 mm in 10 minutes (=120 mm/h), 50 mm in 1 hour, and up to 70 mm in 6 hours (=almost 12 mm/h). In contrast, in the present paper the most intense event reported had a maximum rainfall rate of ~ 27 mm/h. All but one of the remaining events listed in Table 2 had maximum intensities of < 10 mm/h. These seem unlikely to be responsible for flash floods. I was not able to locate information on the 2018 flash flood event for additional comparison. I think that it would help readers place the results of the current ms. in context, if some information on the historical flash floods could be provided, at least in summary.

As suggested, we added the 2016 and 2018 flash flood event properties (rainfall amount, flood peaks, runoff coefficients) in table 2, although the data is not exhaustive, with the highest impacts and flows of both floods being located downstream of the presented measurements. We also added a PCA analysis of the rainfall-runoff event dataset (Figure 4), on which the 2016 and 2018 flash flood statistics are positioned, which further contextualizes the study database in the context of flash floods.

I think that the focus on 'ordinary' events needed some comment. How can a study of much more ordinary rainfall events shed light on what occurs during the seemingly far more intense rainfalls that seemingly accounted for the historical flash floods?

We assume that there are not only the extreme properties of precipitation but the intrinsic catchment properties and its hydrological state that cause the hydrological response to be rapid and concentrated in a relatively large flood peak. In order words, we suppose that the catchment "hydrological reactivity" is independent of the rainfall magnitude enough (although this will make the high hydrological reactivity to be problematic) to be detected on a moderate rainfall-runoff event database. In order to clarify this assumption, we added two sentences in the introduction:

lines 104: "*Here, we ask – in the context of a Central European study area – what is influencing the specific flash flood event patterns beyond the extrem rainfall properties?*"

line 123-124: "We indeed assume that the hydrological reactivity of the catchment is detectable independently of the magnitude of the precipitation. The same model is also applied on the 2016 and 2018 flash flood events, with the aim of having reference transfer times characteristic of flash floods."

Additionnaly we added one year of rainfall-runoff measurements to our study which results to enrich the database with 17 additional rainfall-runoff events. Among them, the event that occurred on the 13th July 2021 consists in an extrem event in terms of rainfall amount (129 mm) and discharge peak (the highest water level was recorded during that event since the oldest hydrometric station has been installed at Larochette in 2014). Although this event is not a flash flood, it enables to apply the TTD properties using extrem rainfall statistics, making possible to question the study result independency from the rainfall magnitude.

Did these [the flash floods], for instance, occur when the soil had been thoroughly wetted by antecedent rainfalls? Does surface runoff overtop ground surface roughness elements when the rain is sufficiently intense (above some threshold?), allowing a smoother and more direct path downslope? What was the nature of the precipitation? I assume that the flash floods were the result of shorter, more intense, convective events, and therefore were likely to have occurred in summer (this information is missing from the current ms.). I imagine that these were late afternoon events, but this would also be relevant information. Were there very local runoff sources located close to the stream channels, perhaps? Could the movement of convective cells parallel to the long, narrow catchment be significant? Did that occur (perhaps Doppler radar might shed some light on this)? Catchment response to intense convective cells might be quite different from that in stratiform rain, for instance, and different parts of the catchment might show altered hydrologic responses under those different rainfall inputs.

You are right, these are general characteristics of flash floods. But again (and perhaps this was poorly expressed), we are trying to determine what favours rapid and concentrated flooding, beyond the properties of rainstorms. the fact that they are convective events, of high intensity are recognized characteristics that favour flash floods, the fact that the rainstorm is located downstream of the catchment where the hydrographic network is strongly defined also. Here we seek to understand why two catchments react differently to the same rainfall event, whether it is intense or not.

Iffly et al. 2018 refer to lag times to runoff peak of just 90 minutes, whilst in the present study these lags extend to many hours.

In this study, the runoff peak response (TTDpk) varies from 0.5h to 13.7h, and more specifically on HM catchment and during the dry condition (15th April - 15th October), TTDpk's average is 1.9± 0.9h. Those results are actually in agreements with the lag times to runoff peak of 1.5h mentionned in Iffly et al. 2018. Furthermore, in the updated manuscript, we added TTDpkvalues for the 2016 and 2018 flash flood events, which are 0.9 ± 0.1h and 0.1h respectively. Those values still corresponds to the order of magnitude of the HM section's TTDs during the 15th April - 15th October period

The study is weakened by the assumption of a constant runoff coefficient through the duration of rainfall (mentioned in line 200 and elsewhere). This seems particularly inappropriate for long events of several days duration, such as were examined in this ms., and even for events of a few hours duration, when breaks in rainfall (e.g. shown in Figure 6 and Figure 7) allow soil drainage and the re-invigoration of soil infiltrability. It would have been interesting and informative to have seen at least some preliminary sensitivity testing to see how important an effect a changing runoff coefficient might have been to the hydrologic modelling. Perhaps the authors have done such tests and could comment?

We haven‘t made sensitive analysis before getting your comment. Without making it exhaustively, we tested on the KOE catchment – i.e the one that seems to be more affected by the constant RC assumption – a variable RC along the event. Two Runoff Coefficients – RC^m and RC^p – have been defined: the first one characterizes the re-invigoration of the soil drainage at the beginning of the events and the second one characterizes the hydrological response in the heart of the flood respectively. Arbitrarily, the re-invigoration period is fixed to 20 h. RC^m and RC^p are calculated as indicated below:

- $$\frac{RC^m}{RC^p} = SCW\,20$$
- $$RC^m = \frac{RC \cdot V^{tot}}{V^m + SCW\,20 \cdot V^p}$$

where V^{tot} is the total rainfall amount, V^m the rainfall amount occurring during the first 20 hours, and $V^p = V^{tot} - V^m$. The impact on the FDC scores are presented in the supplementary materials, table S3. The TTD properties resulting in variable RC are compared to the TTD properties with constant RC on figure S4.

According to the FDC score, there is indeed an improvement of the results with a mean decrease of 2%. More specifically the results are significantly better with a variable RC for 11 out of the 40 events. Those events occurs during the November-May period.

Considering the TTD properties, TTD50 and TTDpk decreases in average by 0.5 h and 0.4 h respectively. The decrease is homogeneous on the data set. A largest difference appears during the April-Mai period, resulting in smaller range of transfer lag times uncertainties. Nevertheless, the seasonal variations of the TTD properties can be similarly observable on both unit hydrograph model simulations. The comments about TTDs properties thus based on the simulations with the constant RC hypothesis are still valid.

We recognize that the simulation results could be improved taking into account variable RC, in terms of scores and absolute values. Nevertheless, we assume – according to the presented test, that the general TTD properties variability observed over the seasons (and which is the subject of the paper) is consistent.

It would also strengthen the argument of the paper if the authors could present some data on hydrophobicity in the forested areas, that they appeal to as a mechanism to account for more runoff there. Was hydrophobicity actually present, or was this not investigated? If present, does it dissipate in longer events, so that perhaps it differentially affects runoff behaviour in short convective events in summer?

Unfortunately we have not carried out any hydrophobicity measurements on site, neither during the period nor elsewhere. We can only answer your question indirectly. There is indeed a notable difference between convective summer events and winter events: for the first one, the maximum intensities arrive at the beginning of the rainfall event, whereas for the latter there is a progressive increase in the intensity of the precipitation over time. We can think that the arrival of strong intensity without an initial humidification on a dry and therefore hydrophobic soil, inhibits infiltration. Runoff is then favoured all the more if the ground is sloping (hydrophobicity prevents water from attaching to the ground AND gravity leads to runoff). However, we have not yet been able to verify this hypothesis. While looking for references on this subject, I read your three interesting papers (Dunkerley, 2012, 2016, 2021), which tend, with experimental justification, to the opposite conclusions (a rain peak at the end of an event favours runoff). Nevertheless, as you mentioned in your article, your experiments are carried out on a flat terrain, and the results ultimately assess the variable infiltration capacity of soils. In a sloping configuration, there will be a first barrier to this infiltration which is the "adhesion" of the water to the soil surface before infiltration. If there is no adhesion (the hydrophobic property of the soil), then the relief might play a key role. An argue in this hypothesis is the fact that the very fast runoff are only observed on the HM section where steep slopes close to the drainage network are present. Finally, as those statement can not be verified with our current data set, we clearly specify at the beginning of our discussion that this is a plausible explanation that must be subsequently checked:

“Since our dataset appears to be (too) limited for validating our hypothesis, we propose here a list of plausible explanations – based on examples from scientific literature – for the drastic decrease in response times observed in summer on the HM section, as opposed to the KOE section.“ (lines 535 – 538)

The authors identify LAI as an important factor in the hydrologic response (TTD) (lines 491-492). Though without comment, the authors appear to use LAI data from 2002-2006, many years prior to their field data collection. This warrants some comment. Further, the LAI seems to be very small, to judge from Figure 9 (left panel), seemingly the only data presented on this variable. The authors only appear to link LAI to the speculation about litter layers and wettability, evidence for which is not provided. Could the authors offer a fuller comment on why LAI might relate to TTD? Do they consider this to be a real, physical effect, or merely a chance statistical correlation (for instance, via some other seasonally-varying parameter)? Their comments and thoughts would be helpful. They could also perhaps consider presenting LAI data for their catchments (as a map) if they have it available. It would appear to be very variable among fields, forests, etc.

We agree that the use of LAI as an indicator of any influence of the vegetation cycle was awkward. We then removed it and replace it by the calendar day (DAY) as an indicator of the seasonal state of the catchment. Having no specific indicator related to forest litter condition, its role discussed in the discussion is presented as a hypothesis to be tested, as said before.

Finally, I wondered whether there is a role for roofs, roads, drains, culverts, etc., in the catchment response. I do not know this area, but Figure 2 suggests that, at least locally, the villages may have impervious areas that are efficiently drained. The main stream channels also warrant at least some description. Have they been modified, perhaps to flow between artificial banks or walls? How significant is the channel travel time from the upper to the lower catchment? In the same way, landuse could helpfully be described, especially whether fields are tilled seasonally.

Relating to the soil sealing in connection with the presence of urban areas (essentially in Larochette), we know from our field knowledge that rapid flows come from both sides of the lateral tributaries, which are not very urbanised apart from a few villages with a few houses on the plateaus.

The transit time from the top to the bottom of the catchment area would require chemical or isotopic tracing measurements which were not carried out for this study.

Concerning land use, an additional figure has been added in the supplemental materials. The steep slopes are mainly covered by forests, the downstream part of the KOE catchment is mainly grassland. There are only crops (mainly corn) on the marly plateaus of the HM section. The seasonal development could have an influence. Nevertheless the shortest times are obtained at the end of September - beginning of October when corn is most developed. This is why we have not detailed their impact.

Overall, this is a solid study, containing some interesting results. However, I am not sure to what extent these actually bear on the factors accounting for flash flooding.

We added some information about the 2016 and 2018 events which must help to link our result to the flash flood issue. Furthermore we really believe that our study help to highlight how fast and concentrated runoff can be processed in specific catchment, beyond the rainfall properties. Consequently we think to give insights to identify catchment prone to flash flood, even if they are ungauged.

Minor errors:

- line 13: should be 30 km 2 (space is required between numerals and symbol for unit of measurement)
- line 46 and throughout the paper: 'et al' should be 'et al.' (as a contraction of et alia)
- line 93: should end sentence with a question mark
- line 120: omit the parentheses
- line 145: Captions are reversed (left to right)
- line 162: should say 'Figure 2 left', not right
- line 180: it would be preferable to refer to time-aggregated data as rainfall rates (they are equivalent mean rainfall rates, not true intensities)
- line 253: again, space required following numerical quantity
- Figure 9: there are two dashed lines, only one is listed in the legend
- line 500: Hortonian (capital H after the family name of Robert Horton)

The minors comments has been applied. Thanks for those detailed corrections.