#REFEREE 2

Audrey Douinot et al. (2022) Flood patterns in a catchment with mixed bedrock geology: causes for flashy runoff contributions during storm events

General comments:

The manuscript was significantly sharpened and improved. Still, the authors make strong assumptions, that I only partly share regarding extreme events, e.g. in line 127-128: “We indeed assume that the hydrological reactivity of the catchment is detectable independently of the magnitude of the precipitation.” As explained in the first review, catchments can show threshold behaviour at high rainfall magnitude that is not visible at ordinary events. However, since the assumptions are clearly marked as such, they can serve as a starting point for discussions in other publications.

We totally agree that a catchment will react faster when rainfall intensities are high enough. We would like to emphasize that some catchments can be more responsive than others for a given rainfall event. And we indeed conjecture that this higher responsiveness of a specific catchment can already be detected at the scale of smaller events. We propose to detail our conjecture by slightly rewording the following sentence (line 124):

“We indeed conjecture that the hydrological responsiveness of a specific catchment is detectable independently of the magnitude (i.e., volume and/or intensity) of the precipitation event.”
instead of:“*We assume that the hydrological reactivity of catchment is detectable independently of the magnitude of the precipitation.* ”

Specific comments:

Line 27: “catchment” instead of “cacthment” Ok

Line 28: “but they diverge” instead of “but diverge” Ok

Line 29: What do you mean with “opposite variations”? During the May-October period the average response time progressively increases in KOE in contrast to HM. This has been added to the abstract (line 29):

“During this period, the average response time increases progressively in the KOE catchment, as opposed to the HM catchment.”

Line 30: What do you mean with “concentrated (+- 48% +-87%)?” I do not understand the numbers. These numbers relate to the peak lag time differences between both periods (winter and summer periods, table 5). This has now been detailed in lines 30-32:
“The HM catchment exhibits similar TTDs during the mid-October to mid-April period, but they diverge markedly duringthe remaining part of the year, with opposite variations. During the mid-April to mid-October period, the average response time increases progressively in the KOE catchment. This behaviour is in stark contrast to the HM catchment, where response times are significantly shorter (peak discharge delay time decreases by -70% ± 28%) and more concentrated (runoff volume occurring in one hour increases by +48% ± 87%) during the mid-April to mid-October, in comparison to the extended winter period.“

Line 32: Is the water transfer time the same as the TTD? If so, please avoid different names for the same parameter and replace water transfer time by TTD. Ok, „water transfer time“ has been replaced by „TTD“.

Line 73: I would delete “(en)”, “(fr)” – this information can be obtained from the reference list. ok

Line 81: The validity of the sentence “in these catchments [Central Europe] climate forcing is not primarily controlled by topography” depends on the definition of Central Europe. I would say, that the Alps belong to central Europe. There, however, climate forcing is controlled by topography.
That‘s true, by Central Europe, we were referring Belgium, North-Eastern France, Germany and Poland (except the South), which are only part of Central Europe. We propose to change the denomination to „North Central Europe“.

Line 91: Do “9 years” have to be expanded as you expanded the data used in this study?
No, because here we refer to an already published study (Pfister et al, 2017)

Line 107: “extreme” instead of “extrem” ok

Line 205: “four“ instead of “4“ ok

Line 209: “21.7“ instead of “21,7“ ok

Line 215: “Germany) -“ instead of “Germany),“ ok

Line 215: “consists of” instead of “consists in” ok

Line 215: Please do not only report on the rainfall sum, but also on the rainfall duration so that one can infer the rainfall intensity. The duration has been added.

Line 217: The fact that the discharge volume for the event of 13 July 2021 is uncertain despite the measured discharge height is probably due to uncertainties in the water level-discharge curve. A short comment on this would be good. The uncertainty on the high discharge values stems from the fact that the water level has risen to the girder of the bridge. The evaluation of the peak flow has mainly an impact on the evaluation of the runoff coefficient, but less on the transfer time distribution. This is why we considered this as being not necessary to detail further.

Line 245: “corresponds” instead of “correspond” ok

Figure 5: Please point out in the figure caption that the y-axes are scaled differently. ok

Table 2: “[l . km-2 . s-1]” instead of “[L . km-2 . s-1] ok

#REFEREE 3

General comments

The authors addressed the reviewers' comments carefully. Although most of my questions were solved, I still doubt if the results in summer small events is applicable to summer large events. Large flash flood such as the events of 22-07-2016 and 31-05-2018 has not been observed in summer from 2019 to 2021 except for 13-07-2021 (Table 2). The event of 13-07-2021 had different characteristics of TTD (relatively high value of Vol1h) from summer small events at the KOE catchment (Figure 9; L396-397). Thus, I thought that rapid response may occur even in the KOE catchment during large summer events although the rapid response occurs only in the HM section during small summer events. This is related to my previous comment that not only TTDs but also runoff coefficients (or peak magnitude) should be considered to provide conclusion for flood risk management.

We thank the reviewer for clarifying his point of view on the strong hypothesis of our manuscript, namely that the hydrological reactivity of catchments revealed from moderate precipitation events will reflect the vulnerability of these catchments to more extreme events. Since this hypothesis was discussed several times by the reviewers, we decided to add a section to the discussion outlining the limitations of this conjecture. We support its validity on the one hand, by the correlation analysis carried out on the KOE catchment, showing no dependence of TTD properties on rainfall characteristics. On the other hand, the events cited (23-06-2020, 13-07-2021) corresponding to the most intense events in terms of intensity do indeed show shorter reaction times by the KOE catchment, but without reaching the reactivity of the HM catchment (which remains twice as fast in its reaction). These events thus show that the vulnerability of the HM basin remains higher, even in the case of these more intense summer events.
We have added the following section to the manuscript to strengthen this point of discussion:

„5.3 Are the conjectured hydrological processes on the basis of moderate events analysis transferable to extreme events ?

An important hypothesis of our study is that the high responsiveness of a catchment can be detected from moderate rainfall events, which means that there is no threshold effect between the intensity of precipitation and the reactivity of a watershed. From the moderate events that we have analysed, we have evidence that the HM basin is more likely to generate fast floods because its hydrological response is about twice as fast and more concentrated in summer (in comparison to the KOE catchment). A first element supporting our hypothesis is the fact that the correlation analysis shows very little dependence of the hydrological responsiveness of the KOE catchment on general precipitation properties. This shows its resilience to precipitation characteristics, and thus a constancy in terms of responsiveness. Note that this statement only holds within the range of variation of the analysed precipitation properties and a possible threshold effect beyond this range of variation cannot be completely excluded. However, three events in the database include precipitation intensities of more than 15 mm in one hour. Without being extreme, this is close to the properties of the flash flood events reported in Table 2 and Figure 4.

A closer analysis of the hydrological responses of two of these three events with high intensities shows that the response times of the KOE catchment are relatively shorter than those observed in the summer period: the median TTDpk are 3.75h and 7.25h and the VOL1H are 11.6% and 11.1% for the events of June 23, 2020 and July 13, 2021, *respectively (in comparison to an average of 8.5h and 6.7% over the mid-April - mid-October period). It is thus possible that the correlation analysis via Kendall's indices may miss this dependence, by the fact that the strong main dependence on seasonality hides a minor dependence. Furthermore, these events are at the margin of those studied and Kendall's coefficients tend to minimise the influences of specific individual events. Assuming then that this influence is possible (despite the fact that it does not appear within the correlation analysis), we can still compare these response times and concentration rates to those observed for the HM catchment: median TTDpk are 1.30h and 2.0h and VOL1H is 13.9% and 11.9% for the events of 23 June 2020 and 13 July 2021, respectively. Thus, the response of HM remains both more concentrated, and above all more than twice as fast. The HM catchment still appears to be more prone to rapid/flash floods than the KOE catchment. Finally, if we compare the response times observed during flash floods in 2016 and 2018 (TTDpk = 0.1h and 0.9h; TTD50 = 5.2h and 1.1h, respectively), they are equivalent to the lowest response times observed in the HM catchment (TTDpk = 0.5h and TTD50 = 2h), supporting the fact that the high rainfall intensities of flash floods did not unequivocally generate faster runoff than moderate events (although the magnitude is not mentioned). Based on the correlation analysis and on the most intense summer events of our data set, we tend to conclude that the high responsiveness of the HM catchment (in comparison to the KOE catchment) prevails during more intense rainfall events and therefore corroborates our initial hypothesis."*

Specific comments

L375-376: Why did an event with TTD50 and TTDpk of >20h exist in May (Figure 9) although the authors wrote 17.7h and 13.7h for the maximum value of TTD50 and TTDpk, respectively? You are totally right. To my remember, we used the 5th and 95th percentile to give the range of variation, but the extrema are more apropriate in the quoted sentence. We therefore changed for the extrema values.

Figure 9: Vol1h of flash flood event in July is not shown in the figure. If you could observe the value, please add it to the figure. Also, it would be helpful if it is written in the figure caption that the flash floods were observed at Larochette.
VOL1h of the flash flood of 22.07.2016 is extremely high (64.3 %) and makes the variation of the others not visible, if included in the graph. As the figure is mentioned in table 5, we decided to maintain the display scale of the graph. Nevertheless, in order to be more explicit

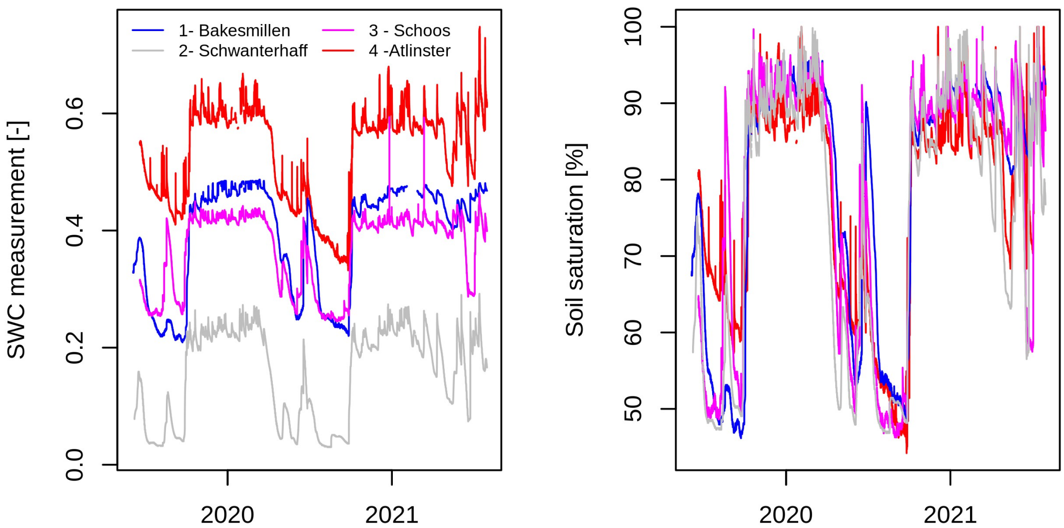
on this extreme flash flood value, we added an arrow which points out this score out of the panel. This has been also mentioned in the figure legend:

„*Figure 9: Properties of the simulated transfer time distributions: the median transfer time (TTD50 [h], left panel), the peak flow lag time (TTDpk [h], center), the runoff response concentration in one hour (VOL1h [%], right panel). The events are ordered by calendar day. The orange and green envelopes correspond to the average calendar values, based on the 3 closest estimates and taking into account the uncertainties of the metric (TTD50, TTDpk or VOL1h) assessments. The purple arrow on the third panel points to VOL1H of the flash flood of July 2016 which exceeds the graph scale (VOL1H = 64.3%).*"

Figure 10: Does soil moisture mean volumetric soil water content? If so, 90% seems too high. Did you conduct calibration of Campbell CS650 using soil in the study site?

Having four soil water time series with different saturation levels in winter (Figure below, left), we indeed calibrated the time series, according to the porosity and the field capacity of each soil, to get the soil saturation level [%] (Figure below, right). Figure 10 indeed shows the soil saturation level and not the soil moisture content. This has been reworded in the legend:

„Figure 10. Left: The catchment state at the start of each event (points): The minimum discharge during the 7 days before the event (Qbase, [$m^3.km^{-2}.s^{-1}$]), and the soil saturation at 20cm in depth (SWC20 [%]). The light blue colour corresponds to the weekly average discharge minimum at Koedange (solid line) and Medernach (dashed line) over the studied period. The red line corresponds to the soil saturation calendar day average at 20 cm depth in the Medernach catchment over the same period."



Left: raw soil water content measurements at the four raingauge stations (from north to south); Right: standardized soil moisture preserving the temporal variation and fixing the maximum value to 100 %.

Technical corrections

Figure 4: “May” instead of “mai”. This has been corrected.

Table 5: I think expression like “16 Oct – 14 Apr” is more understandable than “16.10-14.04”. ok. This has been reworded.

#REFEREE 1

This paper has been somewhat improved (and the title modified) over the previous version which was the basis of my report in February 2022. It is well-written and clearly argued, and I enjoyed reading it and considering the arguments raised.

Nevertheless, whilst it includes some interesting and informative material, the paper is somewhat frustrating to read. The authors spend a considerable portion of the paper describing the runoff modelling approach and the rain, soil moisture and discharge data.

We agree that a large part of the manuscript is dedicated to explain the methodology and the data, while the interest of the study is the analysis and the comparison of the TTD distribution given by the model, i.e., from the section 4.2. Nevertheless, we feel that this long section is necessary, both to understand the hypotheses and to validate the results before using them. The various reviewers' comments also supported this view, as additional descriptions were added in response to questions from the reviewers.

But then it emerges that none of this really sheds much light on the flash flooding whose understanding was the stated objective of the work. As I suggested in my previous review of the ms., what is needed is analysis of the conditions resulting in the flash flooding events (e.g. rainfall intensities, durations, convective storm cell movement, etc.); the focus on far less intense and longer-lasting 'moderate' events seems inappropriate, at least to some extent, and unhelpful. As I pointed out previously, the very high rainfall intensities that seem to have been involved in the flash flooding might account for quite different runoff mechanisms and source areas. For instance, on those occasions, where was the most intense part of the convective rainfall located within the catchments?

The stated aim of our study is to find out „*what is influencing the specific flash flood event patterns, beyond the extreme rainfall properties*” (lines 105 – 106). We assume that it is not only the intensity of rainfall but also the properties of the catchments that will induce rapid runoff to the outlet. Furthermore, we assume that this high reactivity is in fact already present during moderate events, and thus we can base our study on average events to detect the reactivity of a watershed. This assumption of the article is presented in line 126: “*We indeed conjecture that the hydrological responsiveness of a specific catchment is detectable independently of the magnitude (i.e., volume and/or intensity) of the precipitation event* ”. It is true that considering this assumption is contrary to the usual assumption of a threshold effect on precipitation intensities. We have therefore added a section in the discussion questioning this strong assumption: ‘*5.3 Are the conjectured hydrological processes on the basis of moderate events analysis transferable to extreme events ?*’ In particular, we detail the fact that the reactivity on KOE appears to be independent of precipitation characteristics, and that on HM the shortest response times (TTDpk = 0.5h and TTD50 = 2h) are close to those observed during the flash floods of 2016 and 2018 (TTDpk = 0.1h and 0.9h; TTD50 = 5.2h and 1.1h), thus demonstrating that the threshold effect on the reactivity is not clearly present on this case.

The authors adopt a number of simplifying assumptions, such as constant event runoff ratio. But given their focus on catchment physiography and geology, I wonder whether instead of studying the hydrologic response of whole sub-catchments, sub-division of contributing slopes into sandstone, marl, etc., might not be necessary to understand the hydrologic response. In this context, the paper lacks

a discussion of the possible spatial variation of soil moisture and hence of runoff response. The authors employ only four soil moisture monitoring sites in the Ernz Blanche catchment (> 100 km2) or just one observation point for ~ 25 km2, and one appears to be located right on the catchment divide, rather than within the catchment. Might there not be zones of preferred runoff production along lower slopes, for instance, towards which gravity drainage might focus seepage and maintain higher soil moisture levels? In other words, are the available data sufficient to properly explore the hydrologic responses of the sub-catchments, or of hillslopes?
The locations of the soil moisture stations were chosen to be spatially representative and on different soil types, but not along a topographic profile. The measurements show few differences during the events suggesting similar infiltration processes. However, the 4 soil moisture measurement stations are located on the plateaus, and indeed the data are insufficient to explore the hydrological response at the slope scale. We therefore mention it at the beginning of the section 5.3 (5.4 in the new manuscript):
„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."

This leads the authors to speculate about what happens during dry summer conditions: possible soil hydrophobicity, the occurrence of biomat flow, etc. But they have no data on any of this, only noting in passing toward the very end of the paper (line 604) that some saturation overland flow had actually been observed. I looked for much more discussion about what the authors might have observed in the catchment during their fieldwork: for instance, did they see evidence of overland flow at many places?
It should be noted that the catchment area is located 1 hour's drive from the laboratory and was therefore little visited at the time of the rainfall. Nevertheless, runoff was observed twice during the event of 12 June and during the event of 26 June 2020. In the first case, runoff was observed upstream of Hessemillen on the slopes (between 10% and 20%) in the meadows bordering the main river. In the second case, runoff was observed on the urbanised plateau of the right bank at the Heffingen station. On the other hand, we got conductivity measurements of the water stream at the Medernach station and at the Koedange station on 12th August. The conductivity time series tend to show a dilution in the first cited station and a concentration in the other one case, thus suggesting different contribution processes (surface for dilution and subsurface / underground for concentration). These observations are not part of an exhaustive search (the KOE

basin for example has not been covered during rain episodes), so this is difficult to discuss them in the manuscript.

Do baseflow recession curves shed any useful light on the groundwater storage volumes, nature of the flow, and so on?
We have not thought of applying a baseflow recession curve analysis because we are primarily interested in the flow transfer time. Baseflow recession curve analysis will help to determine the proportion of base flows, but does not contribute to the scope of the study. Furthermore, it appears difficult to apply such an analysis on HM area, as it is a catchment section.

Other comments or queries in my original report seem not to have been addressed at all: the possible role of built drainage systems, pipes, and drainage, and the nature of the channels (have they been aligned, smoothed, etc., for management purposes?).
We apologize for not to having taken into account all your comments. The catchment area is little urbanized as a whole, but the major part concerns the HM section, and in particular its riparian zone (Figure FS2), which can play an important role. Therefore, we added the following sentences lines 591 – 595:
“. In support of the role of the riparian zone as a buffer to rapid flow, we notice the same hydrograph patterns (Figure 5) at the following hydrometric station (Heffingen, Figure 1), suggesting similar hydrological processes, whereas a river restoration project has been carried out along the hydraulic section between Koedange and Heffingen to improve the lateral connectivity with the major river bed. In contrast, the riparian zone of the HM section is narrower and more urbanized, which further limits the presence of buffer zones for surface runoff, such as wetlands.”

The possible role of moving convective rain cells is also not mentioned.
We do not mention the possible impact of the movement of the rain cell in the direction of flow of the catchment area, because we want to explain here a systematic difference in speed and not by event.

Despite the limited light shed on flash flood processes in the present paper, in lines 621-623 the authors first suggest that further effort needs to be devoted to simulation tools, focussing on catchment physiography and landscape characteristics:
"When targeting an improvement in flash flood ~~understanding and~~ forecasting in Luxembourg, our results suggest that the focus should be set on the development of a simulation tool adapted to catchments with physiographic characteristics similar to those of the HM sub-catchment – i.e., with fractured bedrock and limited riparian zones."

I found this to sit oddly with their finding that in fact simulation tools - as used in the present paper - proved to shed little if any light on the actual presence of hydrophobicity, biomat flow, etc., which the authors conjecture might account for (or at least make an important contribution to) the flash flooding. Nevertheless, they do go on to suggest that processes of this kind also require investigation.
Perhaps it was a misunderstanding, in that we wanted to say that the improvement of flash flood forecasts - which are always carried out from models, must specifically focus on catchments with similar characteristics to that of HM. As this is mainly related to the forecasting target, we deleted “understanding” adjective.

In the end, it was not clear to me to what extent sub-catchment physiography and geology, as detailed in this paper, are actually important contributing factors to flash flooding (as distinct, for instance, from exceptional rainfall intensities) or might rather exert a lesser, more subdued influence on catchment response than being critical determinants of it. Nevertheless, the paper is potentially a useful vehicle for highlighting some issues that remain unclear, even if it leaves the main hypotheses (lines 109-116) essentially unresolved as far as they relate to the conditions required for the occurrence of flash flooding.

Although we have few examples of flash floods in our database, we can compare the response times of the two catchments with those obtained for flash floods (table below). It can be seen that the impact of the catchment area of occurrence (between resilient = KOE, or vulnerable = HM, Larochette) is more important than the intensity of the events. For an accurate comparison, the impact of extreme events on KOE should also be known. This comparison is now detailed in the section 5.3, and we hope support the relevance of our study.

Table : Range of TTDpk (h] depending of the storm events intensities and the catchment

	Resilient catchment (KOE)	Catchment prone to fast runoff (HM, Larochette)	Difference between resilient and vulnerable catchment
Moderate rainfall events during 15 Apr. – 15 Oct.	8.5 [3.7 – 12.0]	1.9 [0.5 – 4.6]	6.4 [2.4 – 10.7]
Flash flood events (2016, 2018)	?	0.5 [0.1 – 0.9]	
Difference between moderate and flash flood events	?	1.4 [-0.4 – 4.1]	

There are a few minor errors:

- line 26: Mars should be March
- line 68: Miller et Dunne should be Miller and Dunne
- line 104: extrem should be extreme
- line 155: suppelmentary should be supplementary
- line 196 40rainfall-ruoff should be 40 rainfall-runoff
- line 200: use decimal point or comma (not both)
- line 409: propertiesof should be properties of

Thank you for the spelling corrections. We made the corrections as suggested.

line 495 (and many places in the ms.) I think that 'monotonous' should be 'monotonic' This has been reworded.

Figure 10: right panel horizontal axis needs units (mm). A reference to the section detailing the different properties has been added in the legend.

Figure 10: What are the coloured zones in the right-hand panel (not mentioned in the caption of this Figure)? We do not understand which coloured zones you are referring to. If it is the blue-gray background, it highlights the significant correlation (strong correlations (3 stars) as well as weak ones (one star)). But this is already mentioned in the legend.