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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Hydrogeomorphological analysis and modelling for a comprehensive understanding of flash-flood damaging processes: The 9th October 2018 event in North-eastern Mallorca" *by* Joan Estrany et al.

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The response of the questions and comments provided by the anonymous RC1 referee will be addressed in a point by point format, being the comments from RC1 in bold.

RC1 - Anonymous Referee #1 Received and published: 18 December 2019

GENERAL COMMENT. The paper of Estrany et al. provides an analysis of the devastating flood that hit the North-eastern side of the Mallorca Island in October 2018, considering: 1) the hydrological response of the catchment; 2) damage assessment;

and 3) geomorphic changes. The analysis presented is quite detailed, and represents a very good starting point that, linked to a study from a meteorological perspective and to a hydraulic study about the flooding dynamics (two aspects that –I acknowledge- go beyond the scopes of the paper), would provide a rather comprehensive picture of the event from a civil protection perspective. The data provided by the water level station are particularly interesting and valuable.

We are truly delighted with the comments of Anonymous Referee #1, due to is considering we are providing a valuable insight in improving the comprehension of extreme flash-flood events in Mediterranean environments, highly subject to human pressures. Regarding meteorological perspective and hydraulic study, we are also aware that they are both out of scope of this first approach carried out within this manuscript. However, the authors we are working to go beyond this first study, precisely (1) addressing a detailed 2D hydraulic modelling in which urban flooding dynamics are being investigated; and (2) evaluating geomorphic changes in two headwaters small catchments through high-resolution digital elevation models built from images captured by a UAV.

My major comment is that the paper has the potential to go beyond the 'simple' description of a case study, where three single pieces (rainfall-runoff modelling, damage assessment and geomorphic changes) are discussed separately. Discussion section could help to bridge this gap. It introduces several topics (e.g., land-use changes, fires, etc.), which, however, are treated in an increasingly qualitative and general way during the discussion itself. What are their actual (and relative) effects on this event? Can they be quantified? Also, the triggering effect of the karstic reservoir(s) should be somehow addressed with more detail (I mean, the authors should try to go beyond the conceptual modelling and provide insights about the physical process, which involves, e.g., specific geological features in specific areas). I wonder about the sudden increase in discharge from 120 to 442 cms (an impressive peak flow rate per unit area) in 15 minutes (very fast response time). The reason for this behaviour is not totally clear. Is it mainly due to the karstic environment or to other reasons (e.g., the failure of a temporary dam)?

It is a very challenging question to assess the effect of land-use change on the event and we believe this is out of scope of the current paper, as it would probably deserve a publication on its own. Our goal in the discussion was to highlight possible further investigation on this aspect. Concerning the triggering effect of the karstic reservoir, we acknowledge that this is a main part of uncertainty in the model. As modeled, the release of the karstic reservoir is very similar to the failure of a temporary dam, as in the karstic model, the water is stored in a karstic reservoir, which is then suddenly released by syphon effect. There is no evidence that it is solely due to karstic behavior. We mentioned this point through the Discussion section and will be showed in the following comments. We have deeply modified the Discussion section. Firstly, the two subsections have been unified forming a unique storyline where hydrological response, rainfall-runoff modelling, damage assessment and geomorphic change are integrated. We believe this new version of the MS is bridging the gap between them, because we have introduced several paragraphs especially focused on the predictability of this kind of flash-flood events in order to better join these different issues. In addition, the new Figure 1 is also useful to better understand the integrated approach. This is the paragraph written to better join the different parts of the Discussion section (see Lines 742-749 of the revised MS): "At present, Mallorca does not have any sort of early warning system to assist flood risk management, and nor of course has Sant Llorenç des Cardassar. Similarly, no hydrometeorological early warning was issued by the competent authorities, as the Balearic Islands have no operational hydrological control network releasing real-time information on discharges. In October 2018, Sant Llorenç des Cardassar was one of the four municipalities in Mallorca with a flood risk emergency plan. However, it was not operational at the time the emergency was declared. As a result, the population was completely unaware of how to defend themselves, even during the emergency phase, although Sant Llorenç des Cardassar municipality had significant social vulnerability to floods, as most of the casualties were tourists and the elderly". Secondly, we have addressed a qualitative –but also quantitative– discussion

about the role played by rainfall intensity and its spatial distribution, complex geology and land cover disturbances, following the suggestion provided by the Anonymous Referee 1#. For this purpose, we have also modified the subsection 2.1. Study area (see Lines 178-190 of the revised MS), where a deeper assessment of permeability in lithology materials as well as a diachronic evaluation of land uses evolution and perturbations (i.e., wildfires) is sustaining the discussion on the role of physical parameters generating the flash flood. "The lithology is mainly composed of marls intercalated with limestone (60% of the area) of the Medium-Upper Jurassic (Dogger), dolomites (22% of the area) of the Upper Triassic and Lower Jurassic, and pelagic limestone marls (14% of the area) of the Lower Cretaceous (Fig. 2d). This lithological composition determines the surface water/groundwater interaction. On the one hand, a high degree of fracturing, fissuring and karstification of limestone favours percolation through karstic aquifers. On the other hand, the imperviousness of Dogger and Cretaceous marls (74% of the area) does not allow percolation, enabling runoff generation. The main land use in 2012 was agriculture (58%), mostly located in lowland areas. Forest (26%) and scrubland (17%) were predominant at headwaters. Terraced fields still occupied 10% of the catchment, although most of them were abandoned (Fig. 2e). In 1956, natural vegetation covered 21% of the catchment. This rose to 42% in 2012 due to an afforestation process of former agricultural land in the second half of the twentieth century. In combination with other factors, afforestation triggered a higher fire risk: two wildfires burnt an area of 1.7 km2: 17% in 1983 and 83% in 2011 (Balearic Forestry and Soil Conservation Service, http://xarxaforestal.caib.es; Fig. 2e)".

We have also placed special emphasis on the sudden increase in discharge from 120 to 442 m3 s-1, which has resulted from the combination of all these physical parameters. Please, see the Lines 675-703 of the revised MS: "This runoff response resulted from the combination of rainfall intensity and its spatial distribution, complex geology and land cover disturbances in generating a high Qpeak (i.e., 442 m3 s-1) with high potential for generating geomorphological changes. Thus, the Qpeak unit obtained (i.e., 19 m3 s-1 km2) can be classified as the third highest value of all the reported values in Marchi et al. (2010) and the highest of those values obtained from streamflow measurements in a hydrometric station and not by post-event analysis. The hydrologic response analysis in the course of a flash flood shows how storm structure and evolution result in a scale-dependent flood response (Borga et al., 2007). Consequently, spatial rainfall organisation, geology combined with orography and land cover disturbances led to pronounced contrasts in the flood response at the Begura de Salma River. Spatial rainfall on the catchment scale showed that the highest accumulation at the beginning of the storm was located at the headwaters of the catchment (at 15:00 h), whilst during the last part of the event the most important rainfall amounts were located in the downstream part. Examination of the flood response illustrated how the extent and the position of the karst terrain (Zanon et al., 2010) and soil conservation practices (Calsamiglia et al., 2018; Tarolli et al., 2014) provided major geological and anthropogenic control of runoff response. Impervious materials cover 74% of the Begura de Salma River catchment, mostly located at the headwaters, which are responsible for the highest values of topographic torrentiality (Estrany and Grimalt, 2014b), facilitating rapid overland flow generation. During the first part of the storm, when the highest rainfall amounts affected the headwaters, runoff response was delayed by the laminar effect of check-dam terraces massively constructed over Cretaceous marls (Calvo-Cases et al., 2020) and by the predominance of percolation in those areas covered by limestone, mostly in the intermediate parts of the catchment. During the last part of the event, when the highest rainfall intensities were in the downstream part, the excess of soil infiltration capacity and the collapse of headwater check-dam structures triggered the sudden increase in discharge from 120 to 442 m3 s-1 in only15 minutes at the hydrometric station. Moreover, the increase of 5 km2 (21% of the catchment area, see more details in section 2.1) of natural vegetation since the 1960s as a result of afforestation processes, increased fuel loads and the risk of wildfires led to 1.7 km2 (7% of the catchment) being burnt since 1980. The removal of vegetation by fires has a similar effect (less interception, less soil storage), which has been experimentally documented after major fires. These factors are a major reason why the history of the steady dev-

astation of plant cover in the Mediterranean is likely to enhance flood risk (Wainwright and Thornes, 2004) and increase desertification tendencies".

Another important point is that authors should take care of the English language and grammar. At the end of the review, I provide some examples, limited to the Abstract and the Introduction, but a thorough review should be carried out throughout the paper.

The authors we are grateful with detailed suggestions on English language and grammar. Following this advice, we have requested an external review on English language by a professional native speaker (see attached the certificate).

Finally, please find below a list of other specific comments. I hope that my comments help to improve the quality of the paper. We thank to the reviewer for her/his dedication on providing accurate specific comments, which have all been carefully addressed.

Abstract: it could be much more concise, avoiding unnecessary comments (e.g., "comprehensive analyses of catastrophic events are crucial. . ."). It is of the foremost importance that the abstract is as much straightforward as possible

We think that the abstract is explaining in a concise way the different issues. However, we have deleted these unnecessary comments.

L24: maybe remote sensing is better

It has been changed.

L31: Copernicus EMS: it's better to avoid acronyms in the abstract without explanation

The acronym has been removed by "Emergency Management Service". See Line 30 of the modified MS.

L45: also the interaction with the (warming) sea surface is an extremely important and peculiar feature of the Mediterranean area (e.g., Cassola et al., 2016; Avolio et al., 2019)

We have modified this sentence in order to add the “warm of sea surface” as a driven factor, as well one these references. See Lines 47-50 of the modified MS: “However, catastrophic flash floods are much more frequent in some parts of the Mediterranean region than in the rest of Europe due to the interaction between geomorphology, climate, vegetation and the warm sea surface (Cassola et al., 2016), all combining to create a flood-prone environment”.

The new reference: Cassola, F., Ferrari, F., Mazzino, A. and Miglietta, M. M.: The role of the sea on the flash floods events over Liguria (northwestern Italy), Geophys. Res. Lett., 43(7), 3534–3542, doi:10.1002/2016GL068265, 2016.

L79: ok, but the main uncertainty in predictability is linked not only to hydrological uncertainty but also (mostly, I would say) to the meteorological uncertainty. This aspect should be also introduced.

As the reviewer has also recognized in some comments focused on the Discussion section, it is not the main aim of our study. Despite this, we addressed meteorological uncertainty in the Discussion section. However, following the own recommendations of the reviewer, we have reduced the meteorological uncertainty in the Discussion section and added the issue within the Introduction section with the following sentence in Lines 76-78 of the modified version: “The main source of uncertainty is related to the spatio-temporal scales of rainfall pattern. The forecasting of intense thunderstorms by numerical weather prediction systems to provide accurate rainfall information is particularly challenging (Alfieri et al., 2015; Collier, 2007).”. References used in this comment: Alfieri, L., Berenguer, M., Knechtl, V., Liechti, K., Sempere-Torres, D. and Zappa, M.: Flash Flood Forecasting Based on Rainfall Thresholds, in Handbook of Hydrometeorological Ensemble Forecasting, edited by Duan Q., F. Pappenberger, A. Collier, C. G.: Flash flood forecasting: What are the limits of predictability?, Q. J. R. Meteorol. Soc., 133(622), 3–23, doi:10.1002/qj.29, 2007.

L109: since the structure of the paper is complex, a brief introduction to the next Sec-

tions could be useful

We believe that the specific objectives deployed in the last part of section 1. Introduction are providing the conceptual structure of the paper that is performed by the typical structure of sections in a scientific paper such as Materials and methods, Results and Discussion. In addition, t the first paragraph of the section 2. Materials and Methods is really useful to provide a comprehension of the paper structure. It is true that the explanation provided in this first paragraph is in terms of methods, but they are completely related with the structure of the paper. However, to provide more consistence to the structure, we have completed this first paragraph of the section 2. Materials and methods, as follows (Lines 122-124 of the new MS version): “Finally, high-resolution digital elevation models (HR-DEM) were generated by LiDAR 2014 data from the Spanish National Geographic Institute and by imagery captured through a low-cost UAV just six days after the catastrophe to calculate a sediment connectivity index (IC) and measure geomorphic changes (Fig. 1)”. In addition, we have designed and performed a new figure with a workflow of the different steps and their relation to the objectives of the manuscript. This is the new Figure 1, captioned as “Methodological workflow of the research study”.

L136: please explain what you exactly mean with “torrentiality”. This index could be ignored by most of the audience We have added some words for provide a clear explanation to most of the audience, in Lines 144-146 of the revised MS, as follows:

“. . .which is topographically computed as a coefficient between the number of first-order streams and catchment area, multiplied by the drainage density; cf. Strahler, 1964). . .”.

L193: a reference is needed to justify the sentence

We added two references to justify the sentence, Lines 239-244 of the revised MS: “Two pair of coefficients were tested: (i) the pair a=200 and b=1.6 was tested because AEMET commonly uses these coefficients to obtain near-real time rainfall estimations (http://www.aemet.es/es/eltiempo/observacion/radar/ayuda; last access: 15 May 2020) from the same radar data that we used in this research; (ii) the pair a=300 and b=1.4 was tested because the NWS in the USA uses it at operational level (Fulton et al., 1998) and it is argued that these coefficients perform better in a convective environment than the first ones (e.g. Seo et al., 2020)".

References used in this comment Fulton, R.A., Breidenbach, J.P., Seo, D.-J., Miller, D.A., O'Bannon, T.: The WSR-88D rainfall algorithm. Weather Forecast, 13, 377–395, 1998. Seo, B-C, Krajewski, W.F., Qi, Y.: Utility of Vertically Integrated Liquid Water Content for Radar-Rainfall Estimation: Quality Control and Precipitation Type Classification. Atmospheric Research, 236, 104800, doi:10.1016/j.atmosres.2019.104800, 2020.

L200: Please add in a figure (Fig. 2?) the radar location

The location of the radar has been added in Figure 2b, formerly Figure 1b.

L201: "due to these effects". What effects? Not clear. Please explain and justify with adequate reference(s)

We agree with the reviewer that the sentence was not enough clear. We have restructured the sentence as follows (see Lines 251-257 of the reviewed MS): "Mountains may partially or totally block the electromagnetic radar signal and affect radar reflectivity and precipitation estimations(Germann and Joss, 2004). The study area is mountainous, but with low maximum altitudes (∼400 m.a.s.l.). This low elevation combined with the regional orography, the distance of the Begura de Salma River from the radar (∼50 km), the 0.5° azimuth of the PPI used, and the altitude of the radar location (113 m.a.s.l.) avoided any topographic interference with the radar signal. Thus, no orographic blocking reflectivity correction technique was needed".

References used in this comment Germann, U. and Joss, J. (2004) Operational measurements of precipitation in mountainous terrain. In: Weather Radar: Principles and

Advanced Applications. Meischner, Peter. (Ed). Springer, Berlin, pp 52-77

LL206-213: this paragraph is not clear. What were the driving data for this analysis? Those provided by the rain gauges surrounding the catchments? If so, the authors should provide some proofs about the reliability of their analysis (e.g., a scatterplot, even in the Supplement). Furthermore, if rainfall data are gridded in 2x2 square cells, why in fig. 6 it looks like they are spatially interpolated? Maybe, because they are related to the GSM-SOCONT scheme (for which the authors refer to inverse distance weighting)?

We have rewrote the paragraph in order to clarify this issue. We have also generated a scatterplot in which the two estimations are compared against the observed rainfall (the new Figure S1).

The new paragraph in Lines 262-270 of the revised MS: "With the set of coefficients a=300 and b=1.4, the maximum amount of estimated rainfall using radar data clearly underestimated the observed rainfall, with a PBIAS of –50.6% and an estimation of ca. 149 mm as the maximum rainfall amount, compared with the 257 mm recorded at the Sant Llorenç des Cardassar rain gauging station (see Fig. 2c). Instead of using the recorded rainfall in gauging stations to calibrate the radar-based rainfall, a correction method of the rainfall estimation based on spatial resampling was posited here. Accordingly, the 2*2 km spatial resolution of radar data was resampled by assigning to each grid cell the value of the maximum amount of estimated rainfall at 1*1 km. By this method, the regression coefficient reached R2= 0.8, a PBIAS of only +2.6% and 258 mm as the maximum estimated rainfall amount, which fitted the rainfall observed at that point (Fig. S1)". The estimated rainfall from gridded radar data was directly used in the SOCONT scheme without inverse distance weighting. Only the temperature is interpolated. This has been clarified as follows (see lines 365-367 of the revised MS): "Resampled 1 km resolution radar data (see subsection 2.3) were used in the model to obtain precipitation for each elevation band by including all 1 km resolution points falling within each elevation band".

L218: When introduced, the acronym MEDhyCON (such as any other) should be explained

We have introduced the whole meaning of the acronym MEDhyCON within the Introduction section: MEDiterranean hydrological CONnectivity Research Group.

L251: “a second hydrograph was designed”: with what Q values?

A second hydrograph was designed, ranging also between 2 to 512 m 3s-1, with additional flow steps. The first hydrograph consisted of nine different flow values, whereas the second one consisted of nineteen different values or steps, including intermediate values, and some other significant discharge values, such as the maximum channel capacity, and the value at which the presence of the bridges start influencing the hydraulics of the system. All values are represented as points in Figure 5 SDRC (formerly Figure 4). Besides, we have modified the sentence explaining the second hydrograph design. In the former version of the MS (Lines 250-253) was: “Under these conditions and with this designed hydrograph, a first approach to the SDRC was obtained. In order to improve the accuracy of the SDRC, a second hydrograph was designed, also containing the Q values of the first hydrograph”. The new version of the sentence (see Lines 313-317 of the revised MS) is: “These conditions and this designed hydrograph gave a first approach to the SDRC. To improve the accuracy of the SDRC, a second hydrograph was designed with nineteen Q values, also containing the previous nine Q values of the first hydrograph, new intermediate values and some other significant Q values such as the maximum channel capacity and the Q value at which bridges influence the hydraulics”.

L262: I would like some more details about the percentages of obstruction detected and how they were calibrated. Do they rely (only) on the three ground control points? Why those points? When/how were the water levels measured on them? Does the simulation consider 0% obstruction up to below the bankfull and 85% immediately after? No transient state?

The percentages of obstruction detected were estimated from post-event photographs and calibrated using maximum water stages observed in situ 10 hours after the event within a period of time in which the high-water marks were still preserved. These marks –fully representatives of the overbank flows– were mapped by using a dGPS Leica 1200 through these three ground control points with high precision. Therefore, intermediate data is not available to compare results in the simulation of the transient state, and maximum water stage must to be compared with maximum obstruction results. Furthermore, the obstruction mechanism was very fast causing that we hydraulically simulated that bridges’ obstruction does not occur when water depths are even close to the low chord of the bridge’s deck. However, when the firsts floating elements start clogging the bridge opening, the obstruction rapidly increases. In addition, this obstruction was maintained throughout the flash-flood event duration, as it could be checked observing post-event pictures. Nevertheless, for providing more clarity about the influence on hydraulics of bridges’ obstruction, see the answer to the previous comment and its related modification within the MS. In addition, we have added the following sentence (see Lines 330-331 of the revised version of the MS) previous to the sentence “85% at Bridge 1, 40% at Bridge 2 and with no obstruction at the other ones”: “These post-event pictures and maximum water stages observed in situ 10 h after the event was useful to estimate the obstruction percentages of these two bridges”. In addition, this sentence “Accordingly, ground control points in three representative locations around the hydrometric station were selected (Fig. 3b), also considering the maximum WS reached in the hydrometric station (4.55 m)” (Lines 337-341 of the former version of the MS) was also modified to better explain the support of the three selected ground control points (see Lines of the new version of the MS): “Maximum WS observed in situ 10 hours after the event, within a period of time in which the high-water marks were still preserved, were mapped through ground control points. Three of them were selected as representative locations around the hydrometric station (Fig. 4b), and the maximum WS reached at the hydrometric station (4.55 m) was also included”.

L321: in my opinion, the best tool for this kind of assessment would have been a complete 2D hydraulic study. Please discuss briefly your choice and its advantages (e.g., it's time-saving, etc.)

We agree with this observation, but flow direction here used were useful to firstly assess the role of hydraulics in damages. Furthermore, a detailed and complete 2D hydraulic modelling in which urban flooding dynamics are being investigated. All the buildings and urban elements of Sant Llorenç des Cardassar village will be introduced in the 2D model by using a high-resolution digital elevation model established from LiDAR technology two months after the catastrophe. As we pointed previously out, this complete 2D hydraulic study is out of scope of this first study; although the results of flow direction here developed will be compared to the 2D hydraulic study. The sentence of Line 321 of the former MS has been modified to reinforce this argument (currently in Lines 397-401 of the revised version of the MS): "Second, the flow direction in the urban network was calculated with Arc Hydro Tools (ESRI, 2019). This gave a preliminary assessment of the role of hydraulic processes in physical damage. Due to the flow direction, this is mainly related to the velocity vector component perpendicular to the building element surface (Amirebrahimi et al., 2016)".

Reference used in this comment: Amirebrahimi, S., Rajabifard, A., Mendis, P. and Ngo, T.: A framework for a microscale flood damage assessment and visualization for a building using BIM–GIS integration, Int. J. Digit. Earth, 9(4), 363–386, doi:10.1080/17538947.2015.1034201, 2016.

Table 1 needs more explanation. Terms like IPmax should be explained

This comment is rising that NHESS audience is beyond the catchment hydrology expertise. Accordingly, we have added a detailed description of each parameter at the beginning of subsection 3.2 Hydrological response of the flash flood, as follows (Lines 430-442 of the revised version of the MS): "The hydrological response of the flash flood was analysed through variables derived from the rainfall (Table 1a, 7 variables) and runoff (Table 1b, 9 variables) of the catchment: Event rainfall duration: duration from

the beginning of rainfall until stopped it; Time of maximum rainfall: time of the highest rainfall intensity; Centroid storm: central time of the rainfall event; Average radar rainfall: mean rainfall obtained by radar; IPmax average radar: average of the highest rainfall intensities obtained from radar rainfall points; IPmax radar: highest rainfall intensity obtained from radar data; IP average radar: average of rainfall intensities obtained from radar rainfall points; Runoff: discharge volume amount divided by the catchment area; Runoff ratio: ratio between runoff and rainfall, also known as runoff coefficient when is expressed in percentage; Event duration: duration of the flood event; Qmax: peak discharge; Time Qmax: time of the peak discharge; T centroid storm – T Qmax: duration between the time of the rainfall centroid and the time of the discharge peak; Qaverage: discharge average during the flood event; Unit peak discharge: peak discharge divided by catchment area, allowing the comparison of peak discharge independently from catchment size; Reduced Unit peak discharge: discharge peak divided by catchment area in square kilometres elevated by 0.6. The exponent was obtained from Gaume et al. (2009), who applied this parameter to compare reduced unit peak discharge from different flash-flood events".

Figure 7 is not very clear. Maybe it could be divided into more figures. However: in Fig. 7a, are the red dashed polygons all derived from the Copernicus EMS? Also zone 2 and 3? Do the latter perfectly correspond to the Government survey? Figs. 7b and 7c are not very readable/useful, in my opinion.

The Figure 7 is the Figure 8 in the revised MS. Once clarified this structure detail, we must recognize that the legend in Fig. 7a was not comprehensible due to "Affected zones" did not help to observe what are the source for determining the affected zones; i.e., Copernicus EMS or Government survey. As a result, we have modified the sub-figures "a", "b", and "c". With this modification, we believe that sub-figures "b" and "c" are totally useful due to are areas not detected by Copernicus EMS and we explain throughout the main text their damage level supported by these sub-figures.

LL510-511: I guess it is Fig. 8c.

Exactly, many thanks for the accurate detection of this error. It has been changed, in the reviewed version of the MS is Fig. 9c.

LL535-536: “Despite these antecedent. . .as reported by these authors.” Why?

We have modified this sentence and the following one in order to improve the explanation. The former version (Lines 535-538) was: “Despite these antecedent wetness conditions, the runoff coefficient of the event (i.e., 36%) was analogous to the median runoff coefficient under average wetness conditions (37%) than dry ones (20%), as reported by these authors. This response illustrated the key role of rainfall intensity in the generation of a high Qpeak (i.e., 442 m3 s-1) with a high potential to generate geomorphological changes”. The new version (Lines 672-677 of the revised MS) is: “Despite these dry prior conditions, the runoff coefficient of the event (i.e., 35%) was analogous to the median runoff coefficient under average wetness conditions (37%) reported by Marchi et al. (2010), rather than dry ones (20%). This runoff response resulted from the combination of rainfall intensity and its spatial distribution, complex geology and land cover disturbances in generating a high Qpeak (i.e., 442 m3 s-1) with high potential for generating geomorphological changes.”.

LL547-548: it’s not clear why the authors need to adjust the initial conditions manually. Does the model not perform well if used for long periods?

We thank the reviewer for his/her critical reading. Actually, the model is not able to reproduce the event when it is running for a longer period. Therefore, the model was run starting in 2015 until the flash-flood event with meteorological data. When using the initial conditions from the long-term run and the radar data, the model is not able to reproduce the event. The extraordinariness of the flash-flood event and the few flood events recorded since 2015 by the hydrometric station did not allow to calibrate the model for the event. In other words, by using specifically calibrated initial conditions, the model is more an event-based model rather than a classical hydrological model. This issue is thoroughly mentioned in the Discussion section, and we have added a

sentence reinforcing it (Lines 711-714 of the revised MS): “In this context, the initial condition H(t0) was manually adjusted, as numerical models applied to simulate catchment runoff response are often unsuccessfully implemented for Mediterranean-climate catchments due to their very heterogeneous responses over time and space (Merheb et al., 2016)”.

References used in this comment Merheb, M., Moussa, R., Abdallah, C., Colin, F., Perrin, C. and Baghdadi, N.: Hydrological response characteristics of Mediterranean catchments at different time scales: a meta-analysis, Hydrol. Sci. J., 61(14), 2520–2539, doi:10.1080/02626667.2016.1140174, 2016.

LL556-585: the discussion introduces many arguments in a general and qualitative way. I suggest to skip/reduce much of the discussion (especially that about the weather predictability, which is not addressed in this study) and/or try to quantify the different effects (please refer to my main concern).

We are very grateful for this key comment, because it is a great opportunity for going beyond the simple description and join the different issues addressed within this manuscript. Accordingly, we have further and deeply assessed the physical parameters conditioning the hydrological response of the Begura de Salma River catchment; i.e., historical and current land uses / land cover, soil conservation structures, the affection of wildfires and perviousness of lithology throughout the catchment.

English language and grammar review: L25: at the catchment scale

“the” has been added in the reviewed MS.

L26: peak discharge of 442. . .

“of” has been added in the reviewed MS.

L28: “i.e.” not needed

We have deleted “i.e.” before the catchment surface area.

L38: For that reason, they usually affect/impact basins. . .

The beginning of this sentence has been modified following your advice. In the reviewed version of the MS: “For that reason, they usually impact basins. . .”

L46: very close to the coastline

We have changed “closeness” by “close”.

L48: “scarce soils”. What do the authors exactly mean?

We have deleted the sentence “Another cause is the reduced vegetation cover and scarce soils” to avoiding confusion. Besides, the following sentence of the same paragraph was explaining this process in a better way: “Likewise, flash-floods are closely related to land use recognizing that the devastation of plant cover in the Mediterranean may increase the risk of flooding because bare soils produce larger runoff coefficients (Wainwright and Thornes, 2004)”.

L51: elucidates

The “s” has been added to “elucidate”.

L51: “the hydrological processes from an extreme flood”? Do you mean the hydrological processes activating during an extreme flood or so?

This was the former version of the sentence: “Characterising the response of a catchment during flash flood events is important because elucidate the hydrological processes from an extreme flood and their dependency on catchment properties and flood severity (Borga et al., 2007).”

And this is the new version of the sentence (Lines 54-56 of the reviewed MS): “Characterising the response of a catchment during an extreme flash-flood is important because elucidates the hydrological activating processes and their dependency on catchment properties and flood severity (Borga et al., 2007”.

L54: small spatial scale usually means low-resolution (e.g., scale 1:50000 is a smaller scale than 1:500). Please rephrase to make the sentence clearer

In order to avoid confusion, we have changed “small” by “limited”: see Line 61 of the revised MS.

LL67-68: in order to reduce the uncertainty of the Q estimate, I suppose

We have added “the uncertainty of”.

L69: “. . .also adding that. . .” please rephrase

We have deleted “. . .also adding that predictability is lowered. . .”, being changed by “. . .conditioned by. . .”. The new version of the sentence is as follows (Lines 83-85 of the revised version of MS): “Flash-flood events are also conditioned by high non-linearity in the hydrological response relating to threshold effects (Braud et al., 2014). Therefore, the predictability of such events remains low”.

L87: regarding vulnerability

We have deleted the beginning of the sentence “Particularly regarding to vulnerability”, because it was also redundant.

L88: “understanding. . .are developed”. Understanding is not developed. Please rephrase

We have changed “understanding” by “assessment of”.

L92: evaluates

It has been changed.

L97: “with that Copernicus EMS one”. Not clear

We have changed the sentence, being now “In addition, a comparison of ‘ground-based’ assessment and ‘remote-based’ Copernicus EMS may shed light on the accuracy of this rapid and helpful tool for assessing most catastrophic flash floods”. Lines

119-121 revised MS.

L112: why “precipitation as well as the Q”? I would write either “Precipitation and discharge” or “P and Q”

We have changed by “Precipitation and discharge”.

Captions: please check also all figure captions (e.g., “left” and “right” pictures in Fig. 3, in Figure 7 there are two references to (f))

In the former Figure 3 (Figure 4 in the revised MS), “left” and “right” was wrongly indicating the dates of the pictures. We have changed by “d” and “e” letters for more internal coherence of the figure and avoiding confusion. In the former Figure 7 (Figure 8 in the revised MS), we have deleted “(f) Damage level of the buildings and plots by zones”.

Supplement: please check grammar also here (e.g., L’independent; “Schema”). The caption of Figure S2 should declare the meaning of the variables.

The grammar has been reviewed: “L’independant” and “Scheme”. We have also added the meaning of the variables in the caption of the Figure S2.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2019-304, 2019.

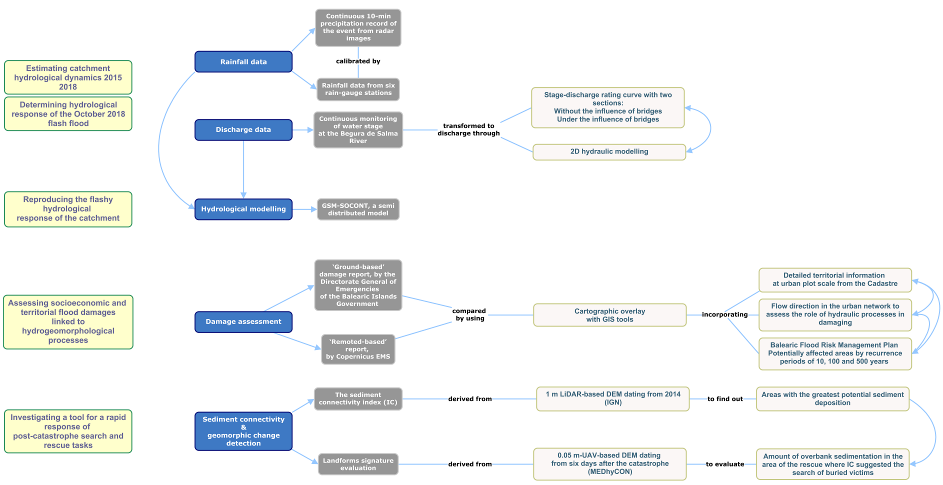

Fig. 1. Figure 1. Workflow of the experimental design.

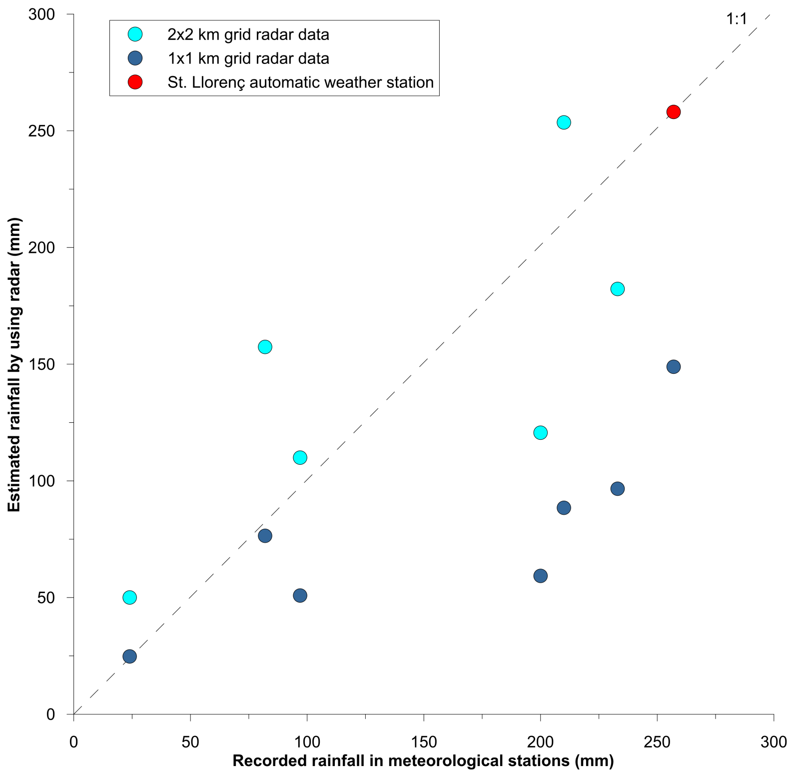

Fig. 2. Figure S2. Scatterplot of recorded rainfall in meteorological stations and estimated rainfall by using different grid radar.

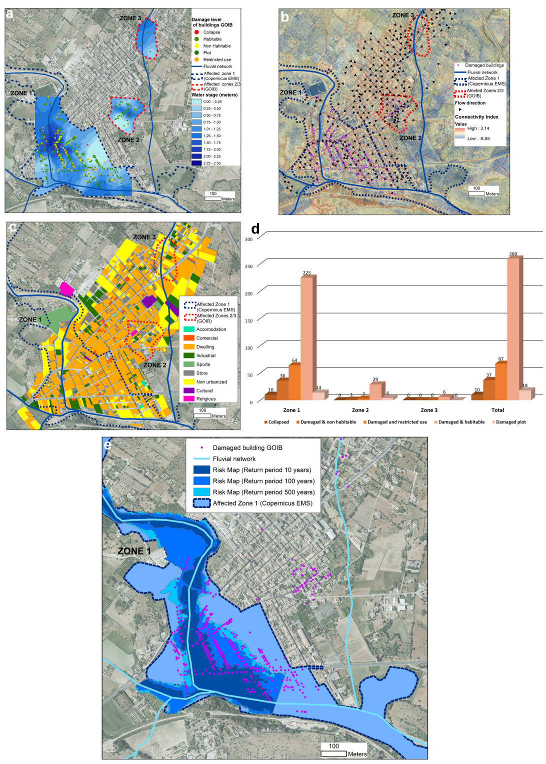

Fig. 3. Figure 8 (a) Map of the damage level classification of buildings and water stage reached in the different affected zones at Sant Llorenç des Cardassar according to the Balearic Islands Autonomous Gove