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Dr. Mario Parise
Scientific Editor, NHESS
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Dear Dr. Parise,

A revised version of the manuscript NHESS-2023-130 entitled: "*Hydrometeorological controls and social response for the 22 October 2019 catastrophic flash flood in Catalonia, north-eastern Spain*" by A. Amengual, R. Romero, M.C. Llasat, A. Hermoso and M. Llasat-Botija is attached. In the next pages, the authors include a point-by-point response to the comments and concerns risen by the reviewers.

The authors express their gratitude to the scientific editor and the reviewers for their valuable comments, which significantly enhanced the quality and presentation of the revised version of this study. Specifically, the abstract and introduction now more clearly emphasize the novelty of the present research. A new Figure 3 has been included to schematically present the methods used in this study and their interrelations. Additionally, the revised manuscript describes in detail the meteorological tools and outcomes, it describes sources of uncertainty and limitations. The discussion section is now divided in two subsections and a new conclusion section has been incorporated in which the main results are presented as bullets.

Sincerely,

Dr. Arnau Amengual

REVIEWER 1

General comments

In this revised version, the authors addressed the comments raised by the reviewers in a satisfactory manner and substantially revised their manuscript with addition of details regarding the hydrological modeling, in particular the evaluation of the performance by comparing peak discharge and time to peak to available field estimates; and by provided much more details on the social response analysis.

The authors would like to thank again the reviewer for the highly constructive comments made during the revision process. The new revised version of the manuscript addresses all the concerns pointed out by the reviewer, including a remodeled discussion section and a short conclusion section, summarizing the main findings of the study.

Specific comments

The paper is now suitable for publication providing the authors address the minor comments below:

Line 114: I guess the authors provide an estimate of actual evapotranspiration and if yes how was it estimated. In general, when presenting a catchment, it is common to provide estimates of potential evapotranspiration, which should be much larger than the provided value of 417 mm.

Yes, it does. In the revised version of the paper, it has been specified that this value refers to the annual mean actual evapotranspiration. The provided value is obtained from previous research by Marquès et al. (2013). These authors computed the mean annual actual evapotranspiration of the Francolí catchment for the 1971-2000 period by implementing the approximation of the Budyko curve (Budyko and Miller, 1974) developed by Zhang et al. (2001). This method estimates the evapotranspiration partition of the water balance by using the dimensionless Budyko Dryness Index (BDI) as well as land use and cover properties. The BDI is defined as the ratio of potential evapotranspiration to precipitation, but it is corrected by a dimensionless parameter that accounts for plant accessible water storage to expected precipitation during the year.

Budyko, M.I., and D.H. Miller, 1974: Climate and Life, Academic Press, New York.

Zhang, L., W.R. Dawes and G.R. Walker, 2001: Response of mean annual evapotranspiration to vegetation changes at catchment scale, Water Resources Research, 37, 701–708.

Marquès, M., Bangash, R. F., Kumar, V., Sharp, R., and Schuhmacher, M., 2013: The impact of climate change on water provision under a low flow regime: A case study of the ecosystems services in the Francoli river basin. Journal of Hazardous Materials, 263, 224-232.

Discussion/conclusions: the current content of the "conclusions and further remarks" should be moved to the discussion section which would then organized in two sub-sections: the first one with the discussion of the physical conditions of the events (the present discussion section) and the second about the discussion of the social response. Then a conclusion in about 10 lines should be written summarizing the main findings of the study.

The authors appreciate this comment by the reviewer. We agree that the current content of the "conclusions and further remarks" section should be moved to the discussion section. The new revised discussion section is now divided in two subsections entitled:" 6.1 Physical factors" and "6.2 Social protocols and potential areas for improving preparedness". In addition, a new conclusion section has been redacted, briefly summarizing the main observations from this study. These are presented as bullets in order to address the comments of reviewer 2.

REVIEWER 2

General comments

I appreciate the authors' efforts in addressing my comments but the manuscript still needs work. My comments have not been adequately addressed. When preparing a response document, please provide page/line numbers of the revisions. I had a hard time finding what has been addressed and where the revisions have been applied. I have recommended a few edits and comments in the PDF.

The authors would like to express again their gratitude for reviewing this study and providing exhaustive and constructive comments to enhance the content of the new revised manuscript. The edits and comments made by the reviewer in the PDF has been addressed as well. Corrections in the text are highlighted in red. In particular, the abstract has been rewritten in order to present in a more orderly manner, the novel contribution of this study, the methods used to achieve the research objectives and the results obtained. Figures 1 and 11 have been modified to improve readability. Note that the program for generating Fig 1b does not allow to change the format of the units of the distance scale.

Specific comments

1. Overall, the writing is OK but some improvements should be still addressed.

2. Abstract: Since the role of social response is presented as a unique aspect of this paper, results related to this factor should be briefly provided.

The authors appreciate this comment by the reviewer. We agree that the abstract was a bit messy and that the results related to address both the physical and human dimensions and their interrelations during this catastrophic flooding should be included in it. Therefore, the abstract has been rewritten to better organize the ideas and to include these outcomes related with the social response (lines 11-28).

3. Introduction: Please explicitly discuss the unique aspects and novelty of this paper.

A new paragraph in the revised introduction has been included to better contextualize and highlight the novel aspects of this work (lines 79-96).

4. Please add a schematic view of your methodology as a figure at the beginning of Section 3.

A new Figure 3 has been included in the revised version of the manuscript. This figure presents schematically the methods used in this study and their interrelations. In the methods section a new statement has been included, contextualizing this new figure (lines 184-186).

5. Sections 4 and 5 should be renamed and presented under results section.

The authors agree that sections 4 and 5 should be presented under a more general results section. However, sections 4 and 5 cannot be grouped under this general section presenting the results as this work has already reached the maximum number of subsections into which a section can be divided according to the journal's guidelines. Therefore, the authors must maintain both sections separately, even if presenting results. We believe that the titles of both sections are descriptive in the sense of the results that their present, as they are entitled as “Analyses of….”.

6. The configurations and assumptions of the control numerical simulation in Section 4.1 should be briefly discussed. This can be a supplement document too.

The authors agree that the configuration and assumptions of the control numerical meteorological simulation should be briefly discussed in the manuscript. To this end, Section 3.1 has been rewritten in order to clarify how the TRAM model is formulated, in which assumptions it relies and how it can be employed to advance in the understanding of the synoptic and mesoscale factors that were decisive for the genesis of the catastrophic HPE (lines 188-225).

The basic idea in the context of this study is to describe as accurately as possible the leading physical mechanisms responsible for the onset and evolution of the convective systems. To this end, TRAM model was forced with large-scale analysis coming from the European Centre for Medium-Range Weather Forecasts (ECMWF) and the National Centers for Environmental Prediction (NCEP) and tested for different domain sizes, time horizons and vertical/horizontal resolutions, in order to optimize its performance in terms of the simulated rainfall (i.e. a closer resemblance to the radar-estimated pattern of Fig. 4b). In the end, the selected TRAM simulations are forced with initial and lateral boundary conditions from the ERA5 grid reanalyses and are performed over the western Mediterranean domain shown in Figs. 5 and 6.

7. The models (TRAM, QPEs KLEM etc.) have inconsistent spatial resolutions. How did you handle this inconsistency? Did you perform any geospatial interpolations? Clarifications are needed.

The authors believe that it does not exist such inconsistency in the spatial resolutions among the models. The TRAM meteorological model is devoted to analyse the physical factors at synoptic- and meso-scales that contributed to this catastrophic flash flooding, with a spatial resolution is 3 km. That is, the meteorological simulation allows to identify and study in-depth the key mesoscale ingredients that concurred in the development and evolution of the heavy precipitation event (HPE): airflow organization and low-level convergence, interaction of the air parcels with the regional orography, moisture advection, air instability, etc.

The quantitative precipitation estimates (QPEs) derived from radar observations have a finer spatial resolution of 1 km and a temporal resolution of 10 minutes. These increased spatio-temporal scales enable a more thorough analysis of the key features of the HPE that led to the flash flood in the Francolí basin in terms of spatial and temporal rainfall organization at catchment-scale.

The KLEM model operates at a spatial resolution of 25 meters and allows to examine in detail the basin response to the HPE in terms of soil moisture, spatial and temporal variability in rainfall and interaction with the geomorphology at subbasin and basin scales. It allows to identify the rainfall, geomorphological and hydrological factors influencing basin response to heavy rainfall.

It is important to note that the meteorological and hydrological experiments are independent and each model serves to investigate the role of the different physical factors at distinct spatial scales that were involved in the onset of the catastrophic flash flood. The hydrological model is forced by the QPEs as boundary conditions. This model directly assigns to each cell the rainfall value corresponding to its intersection with the QPE grid points, without the application of any geospatial interpolation.

8. My understanding is that the automatic gauges record data at sub-daily timescale but the number of these stations are limited, particularly for streamflow. How did you use daily data for a rapid catastrophic flash flood event? What limitations and uncertainties exist here? The authors' response to this comment was not responsive to my questions.

Well, this work only uses automatic stream-gauge data at the Tarragona river section. The Tarragona station measures streamflow near the watershed outlet, covering a basin area of 809.1 km^2. There is available another automatic stream-gauge at Montblanc city, which encloses the upper Francolí catchment with a drainage area of 339.9 km^2. Unfortunately, the flood bore destroyed this stream-gauge, resulting in the unavailability of a complete time series of data for the 22 October 2019 episode.

The remaining information about peak discharges and times-to peak come from the post-event field campaign carried out by Martín-Vide et al. (2023). These are field estimates and the plausible ranges of the estimations are shown in Table 4. When estimating peak discharges from post-event field campaign measurements, errors can be typically as high as 50% of the estimated value. So, caution is always advised when using these estimations. However, Martín-Vide et al. (2023) used hydraulic modelling to estimate peak discharges along the different river sections. These model outcomes were compared with field measurements of high water level marks. Therefore, the authors are quite confident that the estimated ranges are robust to subsequently carrying out an analysis of basin response. In the manuscript, the reader is referred to Martín-Vide et al. (2023) for further technical details.

Martín-Vide, J. P., Bateman, A., Berenguer, M. Ferrer-Boix, C., Amengual, A., Campillo, M., Corral, C., Llasat, M. C., Llasat-Botija, M., Gómez, S., Marín-Esteve, B., Prats-Puntí, A., Ruiz-Carulla, R., Sosa-Pérez, R: A flash flood with large woody debris clogged bridges. The 2019 event of Francolí River (NE Iberian Peninsula), J. Hydrol.: Regional Studies, 47, 101348, 2023.

9. Section 4.3.3: Add a table and show the sensitivity scenarios and the assumptions of each along.

Table 5 has been modified in order to show the sensitivity experiments, which factor analyses each one and the accompanying assumptions.

10. The initial soil moisture is determined based on the antecedent precipitation, as a standard proxy. Why not using global data like ERA5 and CCI that directly present the soil moisture? This, at least, should be discussed as a limitation of this paper.

This is a very interesting point. Numerous hydrological models are updated with initial soil moisture estimates derived from ERA5 or the soil moisture project from the ESA Climate Change Initiative. Frequently used for real-time hydrological forecasting, these models build on more complex infiltration equations, often resolving the water balance equation.

In contrast, the soil conservation service curve number relies on antecedent precipitation to evaluate initial moisture conditions, and this approach has been adopted in this study. However, it remains as a future task to incorporate more complex infiltration schemes into KLEM, as well as to start the model by assimilating soil moisture fields coming from these analyses. The authors agree with the reviewer that this concern should be discussed as a limitation in the paper. The revised manuscript contains a statement with this respect (lines 472-475).

11. Please present values of the fit metrics (e.g., NSE and relative errors) during the model calibration.

The NSE score and relative errors in peak discharge and volume are presented in Table 3. These statistical scores have been explicitly included and commented in the text (lines 439-446).

12. Please discuss the reasons on high error values in the manuscript.

The factors associated with the high error values in the hydrological sensitivity tests have been discussed in the revised version of the manuscript, lines 491-505. These experiments aim to assess the influence of three specific factors on the development of the flash flood event. These are the roles of the: (i) initial soil moisture content; (ii) early rainfall period preceding to the torrential precipitation rates and amounts, and; (iii) variability of the heaviest rainfall period.

The results of the different sensitivity tests highlight the relative importance of each factor in modulating the overall basin response, quantified by the errors in reproducing the control simulation. The highest deviation in simulated runoff volume occurs when considering normal antecedent conditions in sensitivity test 1, even with a smaller total rainfall. In terms of peak discharge, sensitivity test 2 has the most significant impact by neglecting the effect of the early rainfall period on the overall hydrological response. The variability in rainfall during the heaviest precipitation period plays a crucial role in exacerbating peak discharge.

13. Sources of uncertainty and how they can affect your results should be discussed.

The authors appreciate the reviewer's comment. In the revised version of the manuscript, the impact of the different sources of uncertainty and limitations and how they affect the results have been discussed (lines 452-475).

14. The term "hydrometric section" is still used in the paper unlike the authors' response.

This term has been changed to "river section" throughout the manuscript

15. Section 4.1: Mesoscale processes and role of orography are still vaguely presented. Please clarify how the simulation works and how Figures 4-5 were produced.

The authors have made a substantial effort in the revised Sections 3.1 and 4.1 to present more concisely the meteorological model, the key mesoscale processes and to clarify the role of the orography (lines 284-291; lines 297-300; lines 333-334). Simulations involving the annulment of terrain height in the initial configuration of the domain have proven to be very useful in studying the effects of this boundary factor when it is suspected to be relevant.

16. Figures 4-5 can be merged and presented as one figure consisting of all the maps.

The authors kindly disagree about merging figures 4-5 in just one panel. The authors believe that it would negatively affect the structure and readability of the study.

17. Figure 11 still needs clarifications. Please clarify the difference between "simulated" and "estimated".

Done. The caption of Figure 12 (former Fig. 11) has been rewritten to clarify the differences among "observed", "simulated and "estimated"

18. Please spell out all abbreviations in the figures, tables and headings; these need to stand alone.

Done.

19. In the figures and tables, citations to other illustrations should be also avoided.

In the figures and tables, citations to other illustrations have been avoided, except for these captions that were necessary for the correct interpretation of the information displayed.