The manuscript under review is promising and caters to a very important issue, particularly, looking at the use of forecasted hourly rainfall data to simulate debris flows for early warning. This is also pertinent in areas where there is a dearth of data required to develop early warning systems, so a procedure that use synthetic data, but which is carefully validated is quite beneficial. Furthermore, this procedure can be useful in regions lacking historical rainfall data, which is rightly pointed out by the authors. The initial version of the manuscript indeed lacked relevant supporting literature, and the research objectives were not clearly articulated in the introduction; however, these shortcomings have been effectively addressed in the revised version. The Results and Discussion sections have been enhanced through an expanded presentation of the findings. Additionally, the manuscript commendably extends the application of rainfall intensity-duration thresholds, with a focus on Kedarnath, India, as observed in the revised version.

I note significant improvements in the figures, following recommendations from Referees 1 and 2. Nonetheless, I am confused by Figure 11. Despite captions indicating segments (a) through (i), only two sub-plots, labeled (a) and (b), are present, depicting varying cumulative rainfall intensities. This discrepancy suggests either an error in the figure, necessitating nine subsections, or a required revision of the caption. The previous version included nine subplots, which have been altered in the current version without updating the caption accordingly. Please update it accordingly.

Overall, the study exhibits robust scientific rigor, and the utilization of numerically synthesized data to model rainfall-threshold forecasts is commendable. However, I have some reservations regarding the numerical analysis and the resultant mapping of debris flow extents. Focusing on Figure 10 (a), while there is a notable correlation between the simulated debris flows of 2013 and actual events, numerous potential 'false positives' are apparent away from the river channel peripheries. It does prompt a question: could these be attributed to alternative debris flow mechanisms, such as those originating on hillslopes?

Despite these observations, the revisions undertaken, coupled with feedback from previous referees, indicate that the manuscript has successfully addressed key concerns. I would recommend to accept the manuscript, provided the suggested (small) revisions are made. I present a synopsis of the overall changes made by the reviewers:

Methods Section:- Detailed information about the analysis and calibration details for the debris flow model have been included, and this was important for reproducibility and validation of the methods used, addressing a significant gap in the original manuscript.

Validation and Calibration of Models:- The authors have now validated the Weather Research and Forecasting (WRF) model outputs with ground-based and satellite-derived precipitation data. They have also conducted a sensitivity analysis and used different empirical equations for debris flow volume estimation, which was a good step up from the previous single empirical equation based on the Taiwanese case.

Overall Structure and Presentation:- The authors have restructured the manuscript for better flow, clarity, and logical consistency. This includes adjusting the positioning of certain sections and adding necessary explanations and justifications for their modelling choices.

Minor comments:

Line 110: Although the referring to statistical thresholds holding physical explanations are true, it would be nice to see a reference citing/explaining this.

Line 230: In the examples of US, Italy, and Japan, please provide the relevant literature for reference.

Figure 12: In the legend, I do not see the year of publication for Lakhera et al. Are they referring to the same 2020 study that is mentioned in the caption?