This was the second time I was involved as a reviewer for this manuscript. The majority of my comments from the previous review round have been addressed. However, there are some open issues and minor comments, which should be replied to/commented on in the manuscript before a publication can be recommended.

Major comment:

The authors provided in the last review round no uniform reply to the reviewers comments. Instead, the replies were scattered among the official reply and author comments in the public discussion of the manuscript. Indeed, stimulating the public discussion is important and I encourage the authors to do so in the future as well. However, for the non-public review the authors should provide a point-by-point reply to the reviewers in one document to avoid that the reviewers waste time 'searching' for the correct replies. I contacted the handling editor after the first submission of the scattered reply asking for a 'complete' reply from the authors. However, there are still replies linking to other documents. I did not take them into account the replies due to a very practical reason: I'm in a train with the printed version and cannot access other documents. Hence all issues which are not addressed directly in this reply remain unsolved for me. I strongly recommend to always provide a 'complete' reply, also for upcoming articles of the first author.

Detailed comments:

L333 The authors state the r-r model is calibrated using "the mean of the 20 stochastic rainfall realizations" (see also L535). If the "mean" is used, all rainfall peak intensities between the stations are smoothed out. The authors should comment why the think this smoothed rainfall time series is an appropriate input time series. Why were not simply all 20 realisations been taken?

Fig. 13. Why not using a simple barplot here? The information would be much easier to catch. Except the stochastic rainfall model I do not see any clear network setup choice here. It is not as clear as authors state in l544-555.

Comments from the previous review round:

Title: „Even event-scale hydrological response benefits from high density rain gauge observations" – Well, the rephrased title can be questioned, since the high network density is of course espially for events important. Why not „Identification of required rain gauge density for hydrological response analysis in small mountainous catchment " (or similar)?

Former comment 1b - Indeed, the value is "only" (please don't get me wrong here) based on prediction of RC and deltaP/Q. While a realistic estimate of these characteristics is valuable, the uncertainties resulting from the final network with 3 rain gauges for these two criteria is not shown and should be added in a later version of the manuscript.

We initially proposed in the public discussion to add two figures showing i) the RC (Figure A) and ii) the lag time ΔP/Q (Figure B) by comparing the values obtained from the best 1-station or 3-station raingauge network vs. the reference value calculated from the full raingauge network. These two scatter plots are shown below, but we finally found that these results were more visible as a polar plots. These two plots are gathered in the Figure 12 of the manuscript in "4.4.3 Optimum network evaluation", and they show the RC and lag time ΔP/Q calculated from the best 1-station and 3-station network compared to the full raingauge network (L534-543). As the stochastic method for generating rainfall fields cannot be used with a number of points as low as 1 or 3 stations, we performed the computations using the Thiessen polygons methods and consequently no error bars are associated to these plots.

-> For me Fig. A and B are much better to interpret and it is easier to „catch“ the relevant information in comparison to Fig. 12 and 13 (although it would be useful to have the full network in both figures on the same axis, not once on y (Fig. A) and once on x (Fig. B). Especially when lines cross each other it takes minutes to intepret which line shows the better fit. I also have in mind it it not recommended to use these polar plots for more than three datasets, because they are hard to read then (rule-of-thumb, of course). I'm wondering why ‚worst-3-stations' are not implemented in Fig. A and B?

Nevertheless, the Figure C compares the two methods (stochastic vs Thiessen polygons) when the RC and the lag time ΔP/Q are computed from the full raingauge network. We observe for both the RC (Figure A) and ΔP/Q (Figure B) a lower dispersion of values while increasing the density of the raingauge network.

->The „lower dispersion“ is hard to see/interpret. From Fig. A and B it seems both station sets lead to similar good results. For Fig. A and B, should there not always be one point for 1-station and 3-station network realted to one value from the x-axis? So the x-axis value of ~250 min has only one point fo the 3-station network, but none for the 1-station network. IF there are points missing, it is hard to judge on the dispersion.

Former comment 1c - In general, I'm missing the runoff peak as important characteristic in the manuscript. Maybe the authors can involve it/comment on it why it was not considered.

This point has been clarified in the public discussion. (https://editor.copernicus.org/index.php?_mdl=msover_md&_jrl=13&_lcm=oc108lcm109w&_acm=get_comm_sup_file&_ms=87052&c=189590&salt=10720610621776386148) and we added the figure showing the peak flows to the Supplementary Material (Figure S5).

-> Issue remains unsolved.

2 - Based on the comment before, the impact of the rain gauge network densities (and rain gauge locations) on the runoff is not analysed. In the additionally uploaded comment the main author states a rainfall-runoff modelling would go beyond the scope of the study. I do not agree with that and 5 recommend this modelling approach to analyse the impact on the resulting runoff itself instead on single runoff statistics. To attribute the spatial rainfall variability, a distributed rainfall-runoff model would be the best solution.

Accordingly to our answer in the public discussion, we added a modelling component to this paper; the model is discussed in the public discussion (https://editor.copernicus.org/index.php?_mdl=msover_md&_jrl=13&_lcm=oc108lcm109w&_acm=get_comm_sup_file&_ms=87052&c=189590&salt=10720610621776386148). Corresponding modifications of the paper are i) at the end of the introduction (L92-94), ii) presenting the model used in the method part "3.6 Rainfall-runoff model" (L335-350), iii) in the results section in "4.4.3 Optimum network evaluation" (L544-555), iv) with the Figure 15 summarizing the results of the different simulations and v) in the Supplementary Material part 1 with the Figure S9 (map of subcatchments), Figure S10 (the results of all simulations per event) and Figure S11 (the results of simulations per event, cumulated over time).

I'm struggling where to find the new supplementary. It was not uploaded with the revised version, I'm afraid I cannot review that part.

Technical corrections / minor comments:

General: Please check all brackets with numerous references, spaces are missing everywhere)

L24 Please add at the end of the sentence: "…for the studied catchment (0.22 rain gauges/km²)."

L207 "precipitated" -> "fallen"

L226 "line" -> "straight line"

L320 "(Section 4.4 and 0)" <- What does "0" refer to?

Eq. 7 Please correct the formula: "0" instead of =:=", root over whole term, providing proper limits for the sum operator.

L329 year for reference is missing

L343-344 repetition (see L226)

L549 space missing

L552 What kind of reference is Beria 2020b? Is it a technical report, a doctoral thesis or an institute publication?