EDITOR

> *Following up on the assessment of the revised version of your manuscript, there is a need for further modifications and clarifications. A very detailed list of recommendations has been provided by one referee.*

We thank the Editor and the Referee for the long time spent revising our Manuscript and the detailed comments supplied.

> *I invite you to consider them very carefully - also the idea of possibly having your contribution wired around the concept of a technical note. The latter would certainly lift some of the main concerns that relate to the extent to which your findings can be considered for drawing more general conclusions in a very specific environment, with a limited amount of data.*

Thank you for the suggestion. We took into consideration the opinion of the Editor and after having exchanged several e-mails with him, we decided to turn our Manuscript into a technical note.

REVIEWER

General comment

The second version of this manuscript is an improvement on the first, but severe limitations remain.

We thank the Referee for acknowledging the work done during the revision of the first version of our Manuscript.

First of all, the time series is simply far too short to draw the conclusions that are drawn here. The problem is not so much that there are only two months of data, but rather that during those two months there was only one significant hydrological event (meaning: one event that generated a significant change in streamflow), with the result that much of the paper really rests on n=1. Regardless of how many data points were collected at high frequency during that one event, it is still just one event. And there is no basis for assuming that this one event is representative of the behavior of this catchment (note that it is actually relatively small compared to those in the longer-term record in Figure C1 – either in terms of precipitation or the change in network length).

The limitations implied by the relatively limited duration of the field campaign are aknowledged in the text (lines 383-386). In this new version of the paper, we have tried throughout the Ms. to be clear about the fact that all the conclusions drawn pertain to what emerges from the specific dataset analyzed in the paper (e.g. lines 354-359).

The authors' response fails to come to grips with this problem; they simply declare that "... the main issues associated to the use of water presence sensors highlighted in the text would not be changed by the use of more data, and the nature of the relationship between the mean persistency of the nodes and statistical properties of the ER signal is unlikely to be significantly modified as well." The first of those statements probably correct, which is why my earlier review indicated that this was more properly a technical note about the process of making these measurements. The second statement, however, is simply a statement of what the authors believe, and there is no evidence showing whether that is true or not.

The limitations implied by the relatively limited duration of the field campaign are aknowledged in the text (lines 383-386). The paper has been transformed into a technical note as per the suggestion of the Referee.

The authors do provide a figure comparing the distributions of daily rainfall intensity during their two-month study and during a longer-term record. But this misses the point: how do we know whether the catchment's RESPONSE to the one significant event that occurred can be used to describe the catchment's behavior in general (which depends on far more than instantaneous rainfall intensity)? The problem is not really whether the inputs are representative, but whether the RESPONSE to this one event can plausibly form any basis for generalizations.

We have removed all the parts of the paper where an attempt was made to relate the specific hydroclimatic conditions experienced during the two months study period and the long-term climatic regime in the Valfredda catchment (lines 122-127 of the previous version our Manuscript).

Note, for example, that the entire range of discharge that is spanned by these measurements is only about a factor of 10, and in four of the five cases shown in Figure 7 it is less than a factor of two (and sometimes less than 10%!). By comparison, in the study of Jensen et al. 2019 (for example), discharge varied by more than five orders of magnitude.

There are serious issues with the water balance in the study of Jensen et al. (2019). The ratio between discharge and rainfall is larger than unity in most seasons. High discharges were very likely overestimated in that catchment. The limited variations of Q in the Valfredda, instead, are mostly related to the presence of permanent groundwater sources, which makes the regime persistent (i.e. low CV_q).

In their response, the authors say, "Also, the revised version of the paper emphasized the specific geological features of the study catchment, the duration of the study period, and the impact of these factors on the main results of the paper (see lines 116-127)." This passage, however, consists almost entirely of unsupported claims that these factors have NO impact on the results. It says that the 2-month duration of the study is due to the short snow-free period (which is 6 months, not 2). It says that "the dataset included a wide range of climatic conditions and network configurations", even though Figure C1 in the supplement clearly shows that during this 2-month period (the green shaded region), the network length varied by only about 10-20% of (for example) the range during the following summer. And it contains the very surprising claim that "the study period is reasonably representative of the type of network dynamics experienced by the Valfredda Creek", which again is directly contradicted by Figure C1 in the appendix.

We have removed the quoted statements from the Ms. Thank you.

The second major issue pointed out in the previous review is the geological setting. I said in the previous review that "roughly 80% of the basin seems to have no surficial drainage network at all, consisting instead of talus slopes an moraines". That statement is, despite what the authors say, factually correct: the drainage network, such as it is, extends only to a very small fraction of the basin. The authors' response – that 65% of the basin "directly drains through the hydrographic network", based on a DEM analysis – misses the point in two ways.
First, the problem here is not that the channel network isn't somewhere below most of the catchment, but rather that it doesn't EXTEND INTO most of the catchment, being confined to a narrow strip along the western edge, consistent with the rest of the network being buried by moraines and talus slopes, which are visually obvious in Figure 1 (if you enlarge it, you can see the individual talus blocks). This network has been mapped as extending to within a few meters of the divide in the northwest direction (see Figure 1b), but stopping more than a kilometer from the divide in the northeast direction. In other words, as stated in the previous review, most of the basin has no surficial drainage network at all.

We acknowledged the uneven distribution of the stream network in our catchment (lines 94-96). In the revised version we have also pointed out that the analysis presented relies on a high resolution DTM (lines 96-98).

Second, the DEM analysis assumes that subsurface flow follows the topographic gradient, which is at best unproven at Valfredda and is known to be false in many similar karstic catchments (and particularly karstic catchments buried by talus and moraines!).

Thank you for the comment. In lines 94-107 we have provided a fairer description of the geologic characteristics of our study catchment removing all the unnecessary assumptions.

I appreciate that this geological setting may be typical of the Dolomite Alps. But it is still a very poor setting for trying to understand the processes that underlie network dynamics, because most of the network has been buried by talus, and thus most of the stream discharge is generated from areas where the network dynamics are unobservable.

We acknowledged the complexity of studying the hydrological processes of the Valfredda catchment (lines (lines 102-103) and clarified that the results obtained depend on the specific dataset analyzed and on the peculiar characteristics of the site (lines 354-359). Thank you.

The authors say that the 35% of the basin that doesn't even slope toward any channels is the source of several perennial springs, but there is no valid basis for this claim (which would require tracer data). There are perennial springs, and there is an internally draining part of the catchment. No evidence has been presented showing that one is the source of the other. (In this regard, the authors' letter is not even consistent with the manuscript; the letter claims that this "karst area" feeds one spring with "a quite constant discharge of about 40 l/s", whereas the manuscript says that this area feeds "a couple of localized springs with a seasonally variable discharge that ranges from about 60 l/s in the last spring to 35 l/s in the fall".)

We thank the Referee for the comment. We have removed the claim that linked the spring to the karst region of the catchment. Moreover, as most of the highlighted problems concern intrinsic characteristics of our study site and the length of the period analyzed (which can not be changed by definition), we decided (after an e-mail exchange with the Editor) to turn our Manuscript into a technical note. In this way we could focus the attention on our catchment and its specificity without drawing general conclusions.

The authors say that "the text has been polished and the language has been revised. Moreover the new version of the manuscript has been professionally proof read." The authors may wish to reconsider who they employ in this regard. To be clear, English is a tricky language, but there are many errors here that a professional should catch. Here is a sample (probably not complete) of some of the most obvious problems:
"important ecosystems services"
"Whol" (her name is spelled "Wohl")
"various degree of flow intermittency"
"discharge measurement were taken"
"a quite widespread phenomena" (should be phenomenon)
"the sensors' net were screwed on rock emergencies"
"a smaller mean inter-sensors distance"
"is greater or equal than"
"in the Appendix C"
"for each time of the surveyed period"
"the dashed line represent the threshold"
"these kinds of catchment"
"an higher persistency"
"an higher average intensity"
"it was responsible to increase"
"the trend observed during the event occurred"
"precious information"
"the ER sensors signal provides"
"Out data indicated the presence"

We thank the Referee for the careful research of the English typos and mistakes still present in our Manuscript. We apologize for the inconvenience, we have sent a formal complaint letter to the company who did the copyediting and we were more scrupulous and careful while revising the new version of the document.

More specific comments

L 39: "However, this method also proved to be highly time-consuming for relatively small catchments. Recent technological advances in the field of environmental sensing provide a good opportunity to support the observational reconstruction of stream network dynamics." But Figure C1 rather clearly shows that at least in this case, the direct measurements were more informative than the brief period of high-frequency automated measurements. It is inconsistent to claim that the automated measurements are preferred because they take less time, and then to claim that this 2-month record of automated measurements could not be repeated in another year because it would be too much work.

In the quoted text we did not mean to say that automated measurements should be preferred to field surveys because they take less time. On the contrary, the use of small sensors deployed on a mountain catchment and subjected to a huge diversity of climatic conditions requires even more attention and effort that those required by direct surveys: the field surveys that were necessary to collect all the data and check the sensors status were challenging and time consuming, not to mention the time spent analyzing and correcting the data. However, at the moment, remote sensors are one of the few alternatives that allow high frequency measurements of the active length to be taken. In fact data obtained from field surveys can not provide wet length values on an hourly or daily timescale. Figure C1 shows the active length estimated by the empirical model described in Durighetto et al. (2020) and the field surveys carried out on the study catchment to calibrate and validate that model. That plot can not be used to assume whether direct measurements are more or less informative than automated measurements, as a number of independent observations at a low temporal resolution cannot surrogate continuous observations of network dynamics during single rainfall events. At any rate, we decided to turn our Manuscript into a technical note because of the intrinsic characteristics of our study site, which make impossible the use of ER sensors for more than a couple of months per year.

L 83-89: This is unnecessary. (Readers will already know, for example, that a set of conclusions closes a scientific paper...)

Agree, we have removed that paragraph.

L 117: you have a six-month snow-free period, so that's not a valid reason for only having two months of data. As stated in the previous review, I understand that getting good field data in such alpine settings is difficult. But that's why good data often require multiple field seasons.

See our previous answer on the same point, thank you.

L 125: as stated above, there is no evidence showing that the network dynamics, per se, during this period were representative of the catchment's behavior over any other time interval.

We have removed that statement, thank you.

L 341: "the dynamics of L were mainly driven by precipitation, the temporal pattern of which differs from the corresponding streamflow dynamics because of the non linearity of rainfall-runoff mechanisms". This is a non-sequitur. Precipitation and streamflow dynamics will be different even in linear rainfall-runoff mechanisms!

We modified the quoted sentence as follows "the dynamics of L were mainly driven by precipitation, the temporal pattern of which always differs from the corresponding streamflow dynamics".

L 369: this p-value would only make sense if the data points were all independent and the residuals were normally distributed. Neither is likely to be true here.

Thank you for pointing that out, we have removed the p-value.

L 380: what is called "range of variability" here is not range, but standard deviation. And the increase in variability is exactly what one would expect from the loss of degrees of freedom with the sparser sampling.

Thank you for the comment. We have changed the quoted paragraph and moved it to an Appendix.

*L 434: "It is worth noting that the presence of the localized springs did not affect the performance of the power-low model, indicating that the presence of karst areas might not be the cause of the observed hysteresis in the L vs. Q relation." I needs to be recognized that no evidence is presented to show that the water that infiltrates in the "karst areas" (whatever exactly these are... as far as I can tell the entire basin is karstic) emerges from the "localized springs" (and not elsewhere, or not *also* elsewhere). Likewise no evidence is presented to show that the flow from the localized springs originates from (let alone originates *entirely* from) the "karst areas".*

Thank you for the comment. We removed the quoted sentence.

L 454: "Nevertheless, when the mean interarrival between the observations increases, the variability across the samples is more pronounced and the chances that the experimental (L;Q) pairs don't exhibit a clear power-law trend also increases, as shown by the growth of the standard deviation of R2 with T. This suggests that the goodness of fit of the power-law model can be strongly dependent on the specific timing *of the field surveys in which active length and discharge are evaluated, making the observed pattern of R2 poorly informative." This is correct, but it is also Basic Statistics 101, and thus should not be presented as if something notable has been discovered here. Or if there is something happening here that is not obvious from a basic understanding of statistics, that distinction should be pointed out.*

We thank the Referee for the comment. We have modified the quoted sentence as follows: "This shows, as statistics suggests, that the goodness of fit of the power-law model can be strongly dependent on the specific timing of the field surveys in which active length and discharge are evaluated, making the observed pattern of R^2 poorly informative." Moreover we moved the description of the resampling and of the results obtained from the main text to the Appendix.

L 463: "The mean intensity of the ER signal, and the exceedance of a suitably selected intensity threshold were found to be highly correlated with the persistency of the network nodes. This suggests that ER sensors signal provides statistically meaningful information on the hydrologic status of different nodes of the river network." From statements like these (also in the abstract), readers would reasonably infer that the "persistency of the network nodes" is something that was actually measured. That is not the case. It is a modeled quantity, not real data per se. There are no measurements of network persistency (at least, none are presented here).

We thank the Referee for the comment. In the Manuscript there is an entire subsection dedicated to this topic (Section 2.3.1) where we explain our choice to use modeled persistency values instead of directly measured values. During the study period we frequently performed surveys in the field, however we did not map the entire stream network at every survey because collecting the data and checking on the sensors status on this alpine catchment proved to be very time-consuming. The experimental data alone could not be used to calculate the persistency of the nodes so we decided to estimate it on a modeling basis.

L 478: "an in-depth analysis..." Sorry, but one cannot develop an "in-depth analysis" from just one substantial hydrological event!

Thank you, we have removed the word "in-depth".

L 490: "we believe that more extensive field campaigns would not significantly modify the main conclusions of this study..." This may be accurate as a statement of the authors' beliefs, but it is not supported by evidence.

We have removed the quoted statement from the Ms.

*L 492: "... study, as the features outlined in the paper emerged systematically from the data collected during a sequence of events, which were few in number but characterized by heterogeneous hydroclimatic features." Figure C1 shows that, in contrast to this statement, the brief period studied here was characterized by relatively *homogeneous* behavior, compared to the other snow-free periods in that longer record.*

We have removed the quoted statement from the Ms.