Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., author comment AC1
https://doi.org/10.5194/hess-2021-103-AC1, 2021

Reply on RC1

Francesca Zanetti et al.

Author comment on "Analysing river network dynamics and active length - discharge relationship using water presence sensors" by Francesca Zanetti et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-103-AC1, 2021

We thank the referee for providing useful comments which will help us to improve our Manuscript. In what follows, we provide a detailed set of responses to the reviewers' comments (italic text).

General comment

This is an interesting work based on a relatively new technology that has the potential to offer new insight into the dynamics of stream network in small catchments. Streams are highly dynamic systems and characterizing such "liveliness" is important to move towards a better understanding of catchments' functioning.

Overall, this manuscript is well written, logically structured, and clearly illustrated. The introduction is solid and the results are well supported by the data. I have only some specific and minor comments that I would like the Authors to address (see below). Overall, I recommend a moderate revision but since the MODERATE grade is not available to HESS reviewers, I indicated minor review in the review form.

We thank the referee for the positive assessment of our manuscript.

Specific comments

L78-83. I think that the three research questions could be more appealing than the current ones. I understand that, as this is a relatively new measurement approach, aiming at providing the reader with some methodological observation is useful for possible further application of the methods. So, I like question 1. However, I would move it as last question because, in my opinion, it's more important to focus on processes that the method is able to describe and understand, rather than on the method itself. The second question is a bit too narrow because it implies a "yes" or "nor" and does not lead to much insight into hydrological processes. Similarly, question 3 sounds a bit too "methodological" and not so oriented towards process understanding. So, I suggest moving question 1 as third, and to rephrase question 2 and 3 and bit.

Thank you for the suggestion. We will rephrase the second and third research questions,

and we will change their order as suggested. In particular, we plan to reformulate the research question n.2 in order to avoid it to be a closed-answered question. This could be rephrased as "how is the persistency of individual nodes of the network reflected by the statistical features of ER signals across a range of different substrates and various degree of flow intermittency?". Likewise the third question could be rephrased as follows: "how do wet length and catchment discharge co-evolve in response to a sequence of rain events, and how does their mutual relation depend on the temporal resolution of the available observations?"

Indeed, my overall impression is that this work is much technically- and methodologically-oriented and less prone to describe and understand hydrological processes, and I guess that reader of this journal are more interested in knowing how catchment works rather than know if some data can fit a certain model or no. So, I invite the Authors to consider revising the work to reflect this aspect.

Thank you for the comment. We trust our Ms is in line with the scope of HESS, as the aim and scope of the journal explicitly mentions "the study of the spatial and temporal characteristics of the global water resources (solid, liquid and vapour) and related budgets, in all compartments of the Earth system (atmosphere, oceans, estuaries, river, lakes and land masses), including water stocks, residence times, interfacial fluxes, and the pathways between various compartments". Moreover, in the aim and scope section of the journal it is explicitly claimed that "papers should contribute to the advancement of hydrological modeling, hydrological monitoring and data analysis, [...], experimental design and technology". For these reasons we believe that our work could be of interest for the readership of HESS. Furthermore, we would like to emphasize that we are aware of several highly cited, technically-oriented/methodological papers recently published by the journal (i.e. Kaplan et al., 2020 (DOI: 10.5194/hess-24-5453-2020); Kirchner 2016 part 1 (DOI: 10.5194/hess-20-279-2016) and part 2 (DOI: 10.5194/hess-20-299-2016)), an instance which makes us believe that being methodological does not represent per se a shortcoming for an HESS Manuscript. At the same time, we acknowledge the importance of deciphering the underlying physical mechanisms behind the empirical data and we have tried to provide some insight on the major hydrological processes underlying our data (lines 379-387). We are also willing to further emphasize the implications in terms of processes in the revised version of the paper (see also reply to the second referee), as per the suggestion of the referee.

L127. What are the criteria for the choice of the field deployment? Please, specify.

Thank you for the comment. Sensors were deployed along the most dynamical part of the tributaries that were observed during the field surveys, in a catchment with a size that makes the field surveys feasible with the human resources available (say, catchment area < 3 km^2). Technical difficulties and the time needed to reach some locations (due to the harsh environment) were thus taken into account in the selection process. The specific location of the sensors was also chosen considering the heterogeneous substrates of the catchment, making it possible to analyze the sensors' behaviour on different type of soils, and ensuring an heterogeneous distribution of the nodes' persistency. These points will be better specified in the revised text.

L145. This sounds a very short period to me. I understand that practical issues might be arisen but typically we need a longer time period to observe hydrologically processes that

often highly variable in time. Si the average rainfall and stream flow in this period comparable to long-term rainfall and stream flow (or compared to the other years where observations are available, since this is a relatively new experimental catchment). I think it's important for the Authors to discuss this issue, explain why (if) they reckon this is a suitable spell and why, and why (if) this is a representative period for derive information on the hydrological functioning of this catchment. Moreover, the should discuss how this short period of time can potentially impact on the results.

Thanks for the comment. The length of the study period is affected by the characteristics of the study catchment, which is a high relief catchment located in the southern side of the Alps. The basin is snow-covered usually from late November until the end of spring (June). This constraints the time window that can be used to take field measurements and analyze the underlying network dynamics, as winter renews the underlying network dynamics every year. Moreover, the sensors' deployment is highly time-consuming and the set up of all the sensors require at least some months. During the 2019 the underlying climatic conditions were particularly unlucky, as the winter season came earlier than expected (early November instead of early December). At any rate, it is important to stress that though relatively short, the study period covers a wide range of climatic conditions and network configurations, as discussed below. A detailed statistical analysis was performed to analyse the representativeness of the climatic conditions observed during the period of record. Figure 1 of the supplement material compares the distribution of the daily rainfall depths (h) observed during the reference time window (Sep and Oct of 2019) and that observed in the long term (2010-2020) during the whole period within which the network is dynamical (from July 1 to November 30). The comparison shows that the frequency distribution of the rain depth for our study period matches quite well the corresponding long-term distribution, an instance which suggests that the rain regime during the analyzed period is in line with that driving the observed longer term network dynamics. The significant variations of the underlying hydro climatic conditions during the reference time window is also manifested in the statistic of the cumulated rainfall over 5 and 35 days, which have been shown to be the main drivers of the total active length in the Valfredda (Durighetto et al. 2020). Our analysis indicates that the 5-days cumulative rainfall depth (h_5) during the study period ranges from 0 up to the upper quartile of the long-term distribution of h_5 within the years 2010 – 2020, indicating that the event variations in rain amounts are well reproduced by the study period. Likewise, for h_{35} the min/max observed range during the period of record (Sep - Oct 2019) is comparable to $2\sigma_{h35}$ (σ_{h35} being the long-term standard deviation of h_{35}). Therefore, while the wet summer could not be included because of the time needed to deploy the sensors and the study period spans only two months, we think it is representative of the longer term network dynamics observed in the Valfredda. All these arguments will be added to the revised Ms. Please see also our response to the same issue in the section dedicated to the second referee.

L184. Where was the sensor placed? In the grass in a convergent zone, I guess, where water was not flowing? Please, give more information on the aspect of sensors deployment in the field.

Thanks for the comment. The sensor was placed on the grass where water flow was not observed permanently, as the channel activated only during and after precipitation events. In the revised text, we will describe more comprehensively the deployment in the field of the sensors.

Minor comments

L5. I suggest considering the term "customized" instead than "personalized".

Agreed, thanks. We will modify the text as suggested.

L6. I suggest removing "analysed,".

Agreed, thanks.

L6. The expression "nodes' persistency" is not clear without reading the manuscript. Please, clarify.

Thank you for the comment. We will clarify this point by saying "nodes' persistency (i.e. a proxy for the probability to observe water flowing over a given node, see e.g. Durighetto et al., 2020)".

L37. Perhaps here the citation to Godsay and Kirchner (2014) fits well. In any case, a more recent references would be a nice complement.

Thank you for the suggestion. We will add more recent references including the following: Floriancic et al., 2018 (DOI: 10.1002/hyp.13302); Godsey and Kirchner 2014 (DOI: 10.1002/hyp.10310); Jaeger et al., 2007 (DOI: 10.1007/s00267-005-0311-2); Lovill et al., 2018 (DOI: 10.1029/2017WR021903).

L129. "heterogeneous persistencies so as to avoid redundancy in the data." This is not clear to me, please specify.

Thank you for the comment. What we mean here is that the sensors were deployed close to nodes that had a different degree of persistency in order to assess the consistency of the sensors in various field conditions (from nodes almost always dry to nodes that dries down only sporadically). We will better specify this point in the revised text.

L160. Which observations? Please specify. What does "some modelling" mean? Please, explain.

Thank you for the comment. The models referred to here are described in the paper cited in L161, Durighetto and Botter 2021 (DOI: 10.1002/hyp.14053). More information will be added in the revised text, including a brief summary about the model formulation and the main assumptions.

L176. Building a reliable flow rating curve, especially in mountain catchments, is a challenge. How many points were collected to build the FRC, and what was the range of stream flow values and the resulting goodness of fit measures? In other terms, is the FRC reliable to infer robust stream flow values? Please, explain.

Thank you for pointing that out, we agree on the comment. The rating curve was built using 7 points with discharge ranging between 9 and 300 l/s and a coefficient of determination R^2 = 0.99. We will include these data in the revised text.

L178-179. I understand that the Authors want to give light to the ERC project but this is not the right place. Please, remove.

Agreed, thanks.

L199. Did the Authors create a definition of "reliable" for their purposes? The distinction between reliable and not reliable data can be vague. Please, specify what you mean by "reliable".

Thank you for pointing that out. Non reliable data are meant to be missing data, no-data and zeros induced by malfunctioning of the sensors. We acknowledge that the term "reliable" could be considered as misleading and subjective and we will remove it from the text. In the revised version we will specify the reasons for which some data were not included in the analysis, as indicated above.

L356. I suggest considering replacing "got" with "became".

Agreed, thanks.

Please also note the supplement to this comment:
https://hess.copernicus.org/preprints/hess-2021-103/hess-2021-103-AC1-supplement.pdf