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Comment on hess-2021-103

Anonymous Referee #2

Referee comment on "Technical note: Analyzing river network dynamics and the 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-RC2, 2021

This paper reports on what appears to be a pilot project using electrical conductivity sensors to detect presence/absence of flow in a small Alpine basin. I appreciate the difficulty in making these kinds of field measurements, and the paper does a good job of highlighting some of the challenges involved.

However, the abstract, discussion, and conclusions make some rather confident claims, without acknowledging the severe limitations of the data and the unusual characteristics of the Valfredda study site. These issues substantially undermine the strong claims presented here. The major issues are enumerated below.

1 – I am sure that these field data were hard-won, but they span only two months (or maybe only one month – Figures 6 and B2 both refer to "the study period" but one is only about half as long as the other…?), and include only a small handful of precipitation events. It is hard to draw robust conclusions from such limited evidence. The study by Jensen et al. (2019, cited in the references) provides an illustrative contrast, with a much more extensive set of observations, and thus more robust inferences, drawn from a similar number of sensors along a similarly sized channel network (but a longer study period with more precipitation events). I will leave it to the editors to decide whether HESS wants to publish such a limited data set – speaking for myself I would have waited for a more comprehensive picture to emerge.

2 – The resulting uncertainties are very large (see figure 9), but this is not adequately accounted for in the presentation. The text (line 327) says that b varies by about 1% as the temporal resolution changes, but given that the uncertainty in b can be over 10%, it is actually unknown how stable b really is (or isn't). The text (line 331) even argues for a systematic increase in R^2 from 0.485 to 0.522, even though the uncertainty in R^2 can be over 20%, making this "systematic" increase statistically meaningless. Even these very large uncertainties may be underestimates, because the underlying data are serially correlated, meaning that (for example) few of the points in Figure 8 are statistically independent of one another. From the methods it is unclear whether this has been taken into account, as it should be.

The last main conclusion of the paper is that (lines 411ff): "The mean value of the exponent of the power law relationship between catchment discharge and total active length was found to be almost independent on the frequency of the observational data, which instead had a larger impact on the goodness of fit of the power-law model. When the frequency of the data is lower, the observed values of R2 are, on average, larger…" In view of the vast uncertainties in Fig. 9, these conclusions are reckless. Within the uncertainties, either of these trends could be strongly increasing, strongly decreasing, or zero. There is simply no robust conclusion that can be drawn from the data.

3 – The limitations of the study site are severe, particularly for analyses of network dynamics. The basic problem is that roughly 80% of the basin seems to have no surficial drainage network at all, consisting instead of talus slopes and moraines. The critical issue here – which is not acknowledged anywhere in the paper – is that this ~80% of the basin is still generating discharge (at least some of which is presumably measured at the outlet), but the accompanying network dynamics are invisible because they are occurring beneath piles of rock debris. Outside of the mapped network there appears to be roughly two square kilometers of drainage area with no surficial drainage at all.

At best, that means that any observations here cannot be compared with the rest of the network dynamics literature, in which the discharge from the whole basin is compared with the flowing stream network across the entire basin. Thus, for example, there is no way to compare Figure 8 with similar diagrams from other studies, because in this case most of the discharge appears to be generated by subsurface flow that is presumably strongly damped and lagged, suppressing the variability in Q (this may account for the sharp vertical lines in Figure 8, for example).

The manuscript doesn't confront (or even disclose) this problem anywhere, which is surprising given the abstract's mention of "the diversity of the hydrological behaviour of the study catchment" – by which the paper seems to mean only the two small drainage networks that were studied, not the other roughly three-fourths of the catchment.

It is virtually a truism in catchment studies that each site has its own idiosyncrasies, but here this particular "uniqueness of place" makes a network dynamics study particularly difficult. Why study network dynamics in a catchment where the great majority of the drainage area has no network at all? Such a site makes it particularly difficult to draw any mechanistic inferences from the observed network behavior.

The manuscript says (lines 367ff): "Network length was found to be more sensitive than discharge to small precipitation inputs: while most rain events induced visible changes in the active channel length, the catchment stream flow was sensitive only to the rain events lasting for several consecutive days (6-9/09, 13-18/10, 20-24/10) and to intense storms (more than 20-30 mm in 9-12 hours)." This is exactly the behavior that one would expect from a field site like this one, with most of the discharge being generated by relatively slow subsurface flowpaths over ~80 percent of the catchment, but with network lengths being measured on the very few surface drainages in the remaining small fraction of the catchment.

The manuscript continues (lines 375ff): "In our case study, the standard deviation of the wet length as derived from the sensors' data is 360 m, while the standard deviation of L predicted by the power-law model based on the observed variability of the discharges is only 224 m (about 40% lower). This underestimation is induced by the poor ability of the power law model to capture the observed network dynamics produced by small precipitation inputs." It would rather seem that the problem is that *no* model could possibly capture the relationship between the network dynamics in a small part of the catchment, and the discharge generated by completely different mechanisms in the great majority of the catchment.

All of the conclusions concerning the relationship between stream length and discharge (essentially everything after line 10 in the abstract and after line 406 in the conclusions) are based on very thin data from a catchment in which discharge mostly comes from subsurface flow through rock debris (with the result that changes network length in the small fraction of the catchment with surface drainage are unsurprisingly not clearly related to the discharge, which mostly comes from the rest of the catchment). Thus all of those conclusions are based on very thin data that does not allow straightforward interpretation even in this study catchment, and cannot be extrapolated to the great majority of catchments that lack this particularly exotic geometry.

If those conclusions are excluded – as they really should be, given their weak empirical support and their inherently problematic interpretation (network lengths are not measured in the part of the catchment that generates most of the discharge) – then we have essentially a technical note outlining a new way to deploy conductivity sensors, and conveying some lessons learned from a first deployment of these sensors. That would seem to be a more appropriate way to go, rather than trying to draw strong conclusions about length/discharge relationships from such limited data and such a problematic study site.

As noted by at least another reviewer, the language would also need work (e.g., "customized" rather than "personalized"), but any revised manuscript is likely to be substantially different so I have not marked those issues in this go-around.