Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., referee comment RC3
https://doi.org/10.5194/hess-2021-622-RC3, 2022

Comment on hess-2021-622

Anonymous Referee #2

Referee comment on "Bedrock depth influences spatial patterns of summer baseflow, temperature and flow disconnection for mountainous headwater streams" by Martin A. Briggs et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-622-RC3, 2022

Review of HES-2021-622 https://doi.org/10.5194/hess-2021-622

I appreciated the opportunity to review this interesting paper by Briggs and coauthors entitled '**Bedrock depth influences spatial patterns of summer baseflow, temperature, and flow disconnection for mountainous headwater streams**'. The work addresses important questions regarding the description of connectivity and interaction between groundwater and surface water in mountainous catchments. The authors develop in their paper an interesting vision at the interfaces between geomorphology, hydrology and hydroecology (principally fish habitats). They performed systematic measurements of depth to bedrock along stream corridors in eight headwater streams in Shenandoah National Park (Virginia USA) using passive seismic technics along with identification of wet/dry segments and measurement of river temperature. They highlight 3 main important outcomes from these measurements:

- that measured bedrock depths strongly deviate from the ones available in global-scale geologic and soil dataset.
- permeable streambed thickness is highly discontinuous along the stream channels. On zones with important depth to bedrock, the authors identified localized disconnection of stream flow channels during extended period of droughts.
- mean stream temperature during summer is negatively correlated with depth to bedrock suggesting preferential connectivity with groundwater with implications for stream aquatic ecosystems and habitats.

This paper has been carefully prepared and is well written. The introduction presents the context, state of the art and main questions in a comprehensive manner. The results are interesting and their interpretation are well supported by a robust analysis. The discussion and conclusions will definitely trigger the attention of the readers of HESS. I have only raised few general points and made suggestions that could be helpful for the authors to

develop the discussion and conceptualization of their results.

I have some concerns regarding the comparison between measured depth to bedrock and the one compiled in global databases. I agree with the authors that such databases might not be suitable to capture local properties of soil types or depth to bedrock along the river corridor. Nonetheless, there is a major difference in representative scales between the geophysical measurements and the estimates that are compiled in those databases. The depth to bedrock database from Shangguan et al. (2017) provides data over a spatial resolution of 250m, while the data presented here integrate a few cubic meters around the instrument (is the measurement scale actually mentioned in the manuscript?). I believe that it is still interesting to mention but I would recommend the authors to minimize its importance in the manuscript and acknowledge the main differences and complementarities between both datasets.

It also remains unclear to me to what geomorphological processes/features of the landscape the measured depth to bedrock are assigned to: preferential erosion, fracturation/weathering, sediment accumulation, all of them without distinction? I believe that it would be important to link the measured stream corridor depth to bedrock and streamflow behaviors to some knowledge of local catchment-scale geomorphology/geology. This could help identifying generic information to be transferred to other catchments (or at least provide guidance). For example, in table 1, it seems that there is an inverse correlation between valley width and DTB. Also, one would expect that DTB impacts drainage density (dd~K*DTB) and intermittency (through aquifer volume available V~ 2*DTB*river length*hillslope length ~ DTB*river length/dd). Exploring such generic relationship would help to conceptualize the results and increase the impact of the paper in my opinion.

Some references :

Litwin et al 2021 https://doi.org/10.1029/2021JF006239

Luo et al. 2010 https://doi.org/10.1130/G30816.1

Warix et al., 2021 https://doi.org/10.1002/hyp.14185

Ilja van Meerveld et al. 2019 https://doi.org/10.5194/hess-23-4825-2019

I believe that this work brings very interesting insights and data for our understanding of the impact of depth to bedrock to flow continuity and groundwater-surface water exchanges in mountain regions. I recommend the paper to be published in HESS. Please

also consider few minor points listed in the following.

Specific comments:

l145-149: likely to be biased by the location of wells preferably implemented downhill and where more productive aquifer maybe be identified.

l167-169: do you mean in context where the water table is close to the surface? i.e. when K/R (R=recharge) is low?

l234: how to differentiate sediment accumulation from weathering/fracturing development that can also enhance K?

l320: I did not understand how atmospheric effects were filtered here.

l326: providing the equation of BFI would help the readers that are not familiar with this index

Figure 4: why showing depth in log here? I think it masks the actual variability of your dataset.

Figure 4: 1 m seems to be the minimum depth measurable, correct? Is it mentioned in the manuscript?

table 1: it seems that there is an inverse correlation between valley width and DTB. Do you see correlation between drainage density and DTB? Since dd ~ K*DTB. It would be interesting to assess the relationship between landscape topography and measured DTB to identify generic relationship that could be transferred to other catchments.

l398: how is this analyzed/filtered?

Figure 8: I did not fully understand how this graph is interpreted.

l423: I did not fully understand what this means? Did you remove an outlier to improve statistics?

Figure 9: it would be useful to add the confidence interval on this plot.

l450: I do not understand why "(low permeability)" is added between parenthesis here. Please clarify your meaning.

l479: they concern different spatial scales. Not sure how we can interpret this result. See general comments.

l495: I fully agree with this statement. However, the resolution of this database is way lower than the scale you are interested in. In consequence, it may appear obvious that differences exist.

l619: I find hazardous to compare two different years with different recharge records. The BFI is integrative of full baseflow period, but may not be representative of the punctual measurement performed. Could you clarify this point?