

Interactive comment on “Coalescence of bacterial groups originating from urban runoffs and artificial infiltration systems among aquifer microbiomes” by Yannick Colin et al.

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General comments The present work describes the influence of urban runoffs and artificial infiltration systems on aquifer microbiomes. The manuscript is well written, easily readable and well structured. I have few general comments (open questions) and some minor suggestions. In what extend the comparison of samples taken at different time is meaningful? How did you integrate the time-scale in your analyses and discussion? In the discussion section, the authors explain the shifts observed in microbial communities structure by emitting hypotheses linked to nutrient availability and environmental factors. However, physical-chemical data describing the different

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compartment of the studied site are lacking. What would be the effect of the species coming from surface environments on the functioning of the microbial communities in the aquifer? **Specific comments** L91-94. I agree with this sentence. However, one should also take into account the fact that the transition between free-living organisms and biofilm is taxa-dependant. Meaning that the biofilm is not 100% reflecting the diversity of free-living microorganisms living in the aquifer. This is confirmed by H' values ($AQ_{bio} < AQ_{wat}$) in Figure 2B. L204-205. How could you explain the higher diversity in IB compared to WS and DB? Figure 3. It would be useful to add the eigenvalues for both axes. L213-215 and Figure S2. Dissimilarities instead of similarities? **Technical corrections** L226. Remove “(Table 2)”. Table 1 should be Table 2, and inversely. Please make the correction in the text, accordingly. L305. Add “species” after “Some”.

Please also note the supplement to this comment:

<https://www.hydrol-earth-syst-sci-discuss.net/hess-2020-39/hess-2020-39-RC1-supplement.pdf>

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2020-39, 2020>.

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