HESS-2021-461: Response to Reviewer 1

The manuscript entitled 'A hydrological framework for persistent river pools' by Sarah A. Bourke et al., propose a paper that describes a framework for characterizing the hydrology of semi-permanent river pools, as well as some examples of this kind of pools. Althoug I find interesting the overall idea of the manuscript, it is not adequate for publication in its present form.

Thank you for taking the time to review our manuscript, we appreciate your constructive comments and look forward to improving the manuscript in response to your review.

The description of the 'framework' (section 2) is rather overconfident, as this is more a revision of former descriptions than an original one.

While we agree with the reviewer that there are a small number of papers that mention some of the hydraulic mechanisms that sustain persistent river pools, these often have an ecological or management focus, and the treatment of hydrology is incomplete, flawed, or cites this manuscript under review. Therefore, there remains a need for this manuscript to be published as a rigorous hydrological synthesis of these different mechanisms so that future studies can be conducted in the context of a robust hydrological framework.

We thank the reviewer for introducing us to the Joque et al. (2010) paper freshwater on rock pools that we had not previously cited. As the title suggests, this paper describes the ecology of freshwater that persists over impermeable hard rock. There is a brief hydrological description (1 paragraph of hydrology, 1 paragraph of examples) within the section on “the rock pool habitat: definition and distribution”. In the first paragraph authors mention that these features can be filled by precipitation, rivers and groundwater, but that the paper focusses on rain-fed rock pools which are the more typical freshwater habitat (presumably perched pools over impermeable bedrock). Thus, while identifying a relatively broad range of hydrological features that can exist (some of which may be within river channels, others which are not – gnammas for example), it does not detail the hydrological mechanisms that can support persistence of water in pools along rivers (groundwater discharge vs perched rainwater), which is the main focus of our present manuscript.

The reviewer also refers to Bonada et al. (2020), which is a paper on conservation and management of isolated pools in temporary rivers that we are aware of (and had cited). While this paper does provide a brief summary of hydrologic mechanisms that can support pools that is more rigorous than Joque et al. (2010), it cites the earlier version of our paper in HESS-D when doing so. As such, it is a circular argument to say that we are duplicating the work of Bonada (2020) given that they have applied the framework presented here in their manuscript. In revisiting the Bonada paper in response to this review, we have realized that we have not cited Leibowitz and Brooks (2008) chapter on vernal pools and will correct this omission in the revised manuscript. The 2008 chapter provides a summary of the water balance of pools that are not perched, which is consistent with the framework presented herein, but does not describe subsurface permeability features that control groundwater discharge.

The reviewer also directs us to Fellman et al., (2011), which we have discussed in the manuscript. This paper aims to characterize the hydrology of a particular set of pools as controls on dissolved organic matter biogeochemistry. While this manuscript does describe perched and alluvial through-flow pools along river channels, it does not robustly describe the hydrology of these features. It draws conclusions about the hydraulic mechanisms supporting pools based solely on stable isotope values of water (beginning and late dry-season), which are subject to uncertainty that has not been described. In their paper, the water balance of the pools is assumed to consist of inputs from rainfall and groundwater inflow and losses to evaporation. The calculation of evaporative loss from stable isotopic enrichment was made on the basis of a steady state model of evaporation divided by input (E/I). Perched and alluvial through-flow pools are then identified using this ratio (high E/I ratio implies perching). As such, although a subset of the pools are identified as through-flow pools, the conceptual model that underpins the analysis does not account for outflow of water from the pool back into the alluvium (Liebowitz and Brooks, 2008).

The stable isotopic enrichment of a pool with an initial volume of 400 m^3 can be simulated using the water balance equations presented in the current manuscript under review (Figure 1). The evolution of stable isotopic values is simulated using the approach of Bourke et al., (2021). A perched pool will have no inflow during the dry season and losses to ET only; a through-flow pool will have losses to ET, inflow of alluvial groundwater and loss via outflow (infiltration) of pool water back into the alluvium (ET + GW inflow + Outflow). For a perched pool with a volume of 400 m^3 at the beginning of the dry season, water volume over 112 days will reduce to 178 m^3 with δ^{18}O enriching from -8 to 3.5 ‰. The addition of a groundwater inflow of 0.0002 m^3/min (0.3 m^3/d) results in similar end-point values (210 m^3 and δ^{18}O of 2.4 ‰). In this example, using the line of thought presented in their paper, Fellman et al. would have concluded that groundwater in this second pool is not an important component of the water balance. However, over 112 days this groundwater inflow equates to 8% of the initial volume of the pool and may be important for hydrochemical parameters in the alluvial water (or regional groundwater) that have different values than the pool water. Furthermore, alluvial through-flow pools will usually have water losses associated with infiltration to the streambed sediments, which Fellman did not account for. Thus, the inflow of groundwater may be larger than otherwise thought, if it is balanced by infiltration from the pool of a similar magnitude. For example, groundwater inflow of 0.0008 m^3/min balanced by outflow via infiltration of 0.0006 m^3/min will result in the same pool water level as a groundwater inflow of 0.0002 m^3/min, but the isotopic enrichment will be slightly smaller (δ^{18}O of 1.3 ‰). Over 112 days, the groundwater inflow in this third scenario adds up to 128 m^3, or 32% of the initial pool volume. A fourth scenario where the water balance is consistent with Fellman (ET and GW inflow terms are as per scenario 2), but the pool area is halved (initial volume remains the same) demonstrates that the water balance of the pool and stable isotopic enrichment are sensitive to the pool geometry (volume to area ratio), which Fellman et al. did not report on or consider explicitly in their analysis. Thus, the identification of hydraulic mechanism supporting pools was made on the basis of unsupported assumptions about pool water balances.

Their analysis approach, based on an incomplete water balance, led Fellman to conclude that many of the pools studied were isolated from the alluvium water table, but this conceptualization (see their Fig 1) is not hydrogeologically robust. All but one of the pools in their paper occur on permeable alluvial sediments with pools 1-12 shown overlying a similar thickness of alluvium. If the pools were not connected to the alluvial (and/or regional) water table, without the presence of a low-permeability layer beneath these pools, the pool water would infiltrate into the alluvium (Brunner et al., 2009) and the pool would not persist. Thus, the inset diagram of “pools isolated from alluvium water table” is hydraulically implausible (as already discussed in the manuscript).

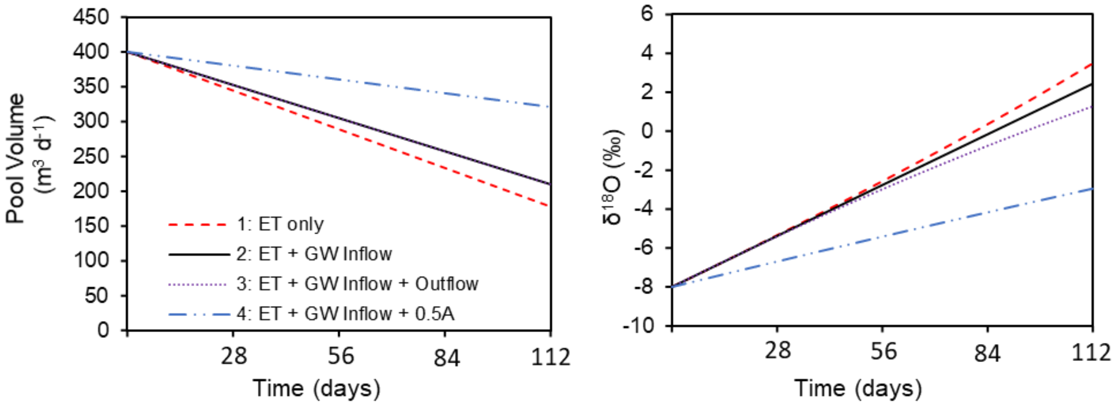

Figure 1 Evolution of pool volume and values of stable isotopes of water in pools with varying water balance components over approximately 4 months of dry season (ET = evapotranspiration, GW = groundwater, A = pool area). Model modified after (Bourke et al., 2021)

Sections 2, 3 and 4 are is too descriptive, too long and repetitive, the equations are obvious and the figures are of poor quality. Most of this part could be synthesized in the table 1 with approprite references and some auxiliary text like that in section 5.1.

We are glad that the reviewer finds Table 1 useful. While the water balance equations may appear obvious, existing literature does not adequately or robustly describe the water balance of river pools (see discussion above), and so we feel that it is important that these are explicitly presented and explained so that water balances can be accurately accounted for in future studies. Similarly, hydrologic concepts that we may take for granted are often used or interpreted differently by practitioners in related fields. The reviewers’ comment that the hyporheic zone as an ecotone or habitat relevant for aquatic life provides a great example of this. While this is true from an ecological perspective (Stubbington, 2012), there is also an extensive susbset of hydrology related to the hyporheic zone that focusses not on ecological properties, but on the scales and mechanisms of hydraulic fluxes, which are driven by *streamflow* and channel morphology (e.g. Stonedahl et al., 2010, Bourke et al., 2014). Thus, when the stream is not flowing, these in-and-out hyporheic exchange fluxes are not operating. In this manuscript (and others, e.g. Leibowitz and Brooks 2008), alluvial water is treated hydraulically as a groundwater storage, with fluxes from the capture zone into the pool considered groundwater inflow, and outflow via infiltration (to the release zone) back into the alluvial groundwater. These fluxes are driven by the hydraulic gradients between the pool and the alluvial groundwater and are not related to streamflow. Conceptually, this hydraulic exchange is most accurately described as analogous to the well-established concept of through-flow lakes found in literature on surface water – groundwater interaction (Winter et al., 1998). While this surface water – groundwater exchange process seems clearly distinct from the relatively short-time scale fluxes of hyporheic exchange associated with streambed contours, we can see that the distinction between this and longer timescale parafluvial flows may be unclear, particularly for non-perennial streams (Del Vecchia et al., under review), so we will revisit this text during revision to attempt to improve clarity for the reader.

With regard to Figures 1-4 in Section 5.1, these are presented as generalized conceptual diagrams of the hydraulic mechanisms that can support persistent river pools. Although we want to be geologically and geomorphologically plausible, they are not intended to represent particular settings or landscapes and are not to scale (this will be specified in the revised manuscript). These figures were always intended to be non-site-specific conceptual diagrams (even in the first submission of this manuscript they did not represent the settings of specific pools), and are broadly consistent with other published diagrams of incised river valleys and floodplains (e.g. Hayes et al. 2018). As this manuscript has a hydrological focus, we have drawn these figures so as to allow us to demonstrate the hydraulic processes that we are discussing, consistent with our experience of, primarily, Australia, but also North America. Some of these processes may not be obvious or common in geologically younger landscapes, or more humid climates, and we will endeavour to clarify and refine these figures in response to the reviewers' comments where possible.

In determining the geometries and labels used in these figures we consulted with colleagues who specialize in geology and geomorphology. We received a range of responses, from which we chose those that we thought were simplest, and most effective at conveying the hydraulic processes we were describing to a broad audience (for which this paper is intended). We thank the reviewer for highlighting the inconsistency in the implied permeability of the bedrock, we will be sure to avoid this in the revised manuscript. Similarly, the lack of a defined surface drainage line between the hillslope and river in Fig 1 will be rectified. The reviewer has suggested "Alluvium" as a replacement for Alluvial channel – we are not sure of the basis for this suggestion, but are happy to make the replacement. We have used the term "valley-fill" to refer to any sediments within the geological river channel (as distinct from the flowing channel that a hydrologist may consider) and do not intend this to make any reference to a particular age of sedimentary deposition – hydraulically, the time of sediment deposition is not of primary importance. Unfortunately, a more suitable alternative has not been suggested by the reviewer and no supporting citations for this comment were provided so we are unable to determine a suitable replacement term. The reviewer also suggests that the water table in Fig 2b is too far from the surface of the floodplain. This figure represents the case of a perched water table beneath a river that resides in an arid or semi-arid climate where the regional water table can be tens of metres below the surface (Villeneuve et al., 2015). Perhaps the reviewer is suggesting that the regional water table should be within the flood plain rather than the bedrock? If so, this point is well taken and we will revisit this diagram during revision to ensure it is consistent with our understanding of the hydrology of these systems. The reviewer has also made a comment about the lower boundary of the aquifer in Fig 3b. This figure depicts the generalized case where valley fill sediments are relatively thin and the lower boundary of the regional aquifer is determined by the lower boundary of weathering in the bedrock, which is hydraulically connected to the valley fill. It is unfortunately not clear what issue the reviewer has with this depiction, which is consistent with our experience.

In my opinion, section 5.2 is of value and deserve publication if some aspects are improved. Mostly, the paper should be readable for everybody not used with Australian geologic units, map coordinates and elevation datum.

This section was added in response to reviewer comments on the previous submission of this work and we are pleased that this reviewer finds value in it. We can very easily ensure all coordinates are standard map grids. Presumably the reviewer would be more comfortable with elevations in meters above sea level (m asl), which is equivalent to m AHD (Australian Height Datum) that we had used and we will update the revised manuscript accordingly.

The map in Figure 6 should represent more information than just the location of unknown pools and the figures should be of better quality.
Thank you for the useful suggestions. We will add towns to this map (e.g. Tom Price) during revision. The grid coordinates shown are standard UTM values for Zone 50K, which are used by Google Earth, a statement of this and a north arrow (up) will be added to the revised figure. The pools used as case studies will also be identified.

The assumptions and interpretations should be better separated from observations.
Each of these case studies is currently structured as beginning with a description of the hydro(geo)logical setting, followed by the data collected and the resulting interpretation of mechanisms supporting pool persistence, and finally the implications for management. Perhaps sub-headings would make this clearer? The reviewer has not provided any specific guidance on how to improve the structure.

Section 6 is rather a discussion than a conclusion, but some discussion is necessary not for showing the interest of 'framework' but for identifying research gaps and further research goals, not necesarily using heavy instrumentation.
We are happy to work on this section during revision and can easily present a separate conclusion rather than the combined section currently presented. We are intrigued by the reviewers' assertion that it is only in intricate places that extensive instrumentation and multiple data sets are required to determine the hydraulic mechanisms supporting persistent river pools. This has not been our experience; perhaps persistent pools on Australian rivers are exceptionally complex? We would have been grateful for any specific papers the reviewer could suggest that could give us insight into how we can robustly understand the hydrology of river pools using a simplified (and therefore cheaper and less time-consuming) approach; however, none were provided

Many detailed comments are annotated in the manuscript. Please also note the supplement to this comment: https://hess.copernicus.org/preprints/hess-2021-461/hess-2021-461-RC1-supplement.pdf

Further response to individual comments in the supplement provided will be made when submitting the revised manuscript.

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