Dear editor,
We thank the two anonymous referees for their considerate and comprehensive comments. We are glad to see that both reviewers are broadly supportive of this manuscript and had no fundamental issues with the methodology or results. Please see below for detailed responses to each of the referee's comments, as well as the advice from the editor. Also please note that all the comments and edits in the marked up pdf provided by referee 1 have also been incorporated. This process has resulted in an improved manuscript, which we hope you will now find suitable for publication.
Regards,
Margaret Shanafield on behalf of all authors.

<u>Editor Comments:</u>
The manuscript should be now revised and a revision and detailed (point to point) reply to the reviewers' comments should be submitted. The main points are that a broader overview over modelling, including other models, should be given and the impact of the specific settings on the results should be made clearer (reviewer # 2). I think in particular the last point is important, as this raises the question of how general the results are.

Response: Thank you.
In response to the request for a broader overview of previous modelling, we have added a table that gives a broad review of key studies on non-perennial rivers using physically-based models.

Regarding the specific setting; of course, the conditions at any given setting will impact the results. However, this catchment was nice in that it included several distinct soil types, which are discussed in detail; therefore, the results will be relevant to catchments with any of these common soil types, from sand to loam to clay. Additional text has been added to the discussion to highlight the broader relevance, as detailed below.

<u>Reviewer 1:</u>

Comment: The introduction needs to be thoroughly revised (see technical corrections). It would be interesting to include a compact review of past modeling efforts of non-perennial rivers as in l40 the authors touch on that only briefly. Also, in the paragraph on using numerical models for this purpose (l39-l50), the dis(advantages) of such models can be more broadly addressed. Although the goal of the study is mentioned, a hypothesis or research question is missing here and should be specified.

Response: The introduction has been thoroughly revised; we thank the reviewer for the helpful comments and edits thorough-out the marked up pdf, as these detailed suggestions made this revision quite easy.

A table of previous works that use coupled numerical models to understand non-perennial river flow is now included; we trust that succinctly gives the reader a nice background on the modelling side of this subject.

Comment: In Sections 2.2.1 to 2.2.3 the authors state the most dominant streamflow generating processes for the different topographical areas they consider. It is unclear however where these hypothesis originate from as no references to existing literature are made.

Response: These hypotheses were generated from the existing data and our field understanding of the catchment. We have modified the beginning of 2.2 to better explain this, stating " Using available soil and topography information for the catchment (DWLBC, 2004; Hall et al., 2009; DEWNR, 2016), we first developed a conceptual model to outline the most likely processes leading to streamflow generation and the resulting dominant streamflow generation for Pedler Creek (Table 2)."

Comment: If I understand the modeling setup correctly, different hydraulic conductivity parameters were chosen for sand and loam. The authors should mention clearly how these parameters were obtained and what parts of the model were calibrated. Also, I'm questioning the suitability of using a numerical model to look at the impact of subsurface hydraulics to stream flow generation, as the authors indicate in the discussion (l422-424) a conceptual model might be a better fit.

Response: In a conceptual model, one loses a lot of the "levers" that are available for seeing what impacts particular aspects have on streamflow generation. Furthermore, there are numerous inbuilt assumptions and implicit representation of processes that might be either limiting or incorrect. Hence, the case for a numerical model is that it can more "naturally" provide the details of the hydrologic response as a function of the hydrological forcing and (mostly) physically meaningful parameters.

As the newly included table 1 now shows, the use of a numerical model to investigate these processes is not unprecedented, and this study adds to that growing physical understanding.

Reviewer 2:

Comment 1. I am interested in how does surface-water and groundwater exchange in this coupling code? What does the coupling length in table 2 mean?

Response: The coupling of the surface and subsurface flow equations uses a first-order exchange coefficient (Ebel et al. 2009; Liggett et al. 2012). There is not explicit physical meaning for the coupling length, but the smaller it is, the close the solution to a continuity of pressure approach which would be the ideal. However, this is computationally too burdensome for most problems.

Comment 2. There is a clay layer in this study area. It prevents the further infiltration of water into deep subsurface. Hence it is obvious that the top soil and the topography will be the main factors in the generation of flow. Thus, the results are somewhat case specific. What do you think about other areas where there is no such a clay layer?

Response: In any catchment, the conditions will govern the result; the conditions in this catchment would not be significantly different than the majority of catchments in Mediterranean climate South Australia, Western Australia, and likely most Mediterranen climate regions globally. There is not clay everywhere, as our conceptual model shows; there are three distinct areas that contribute differently to streamflow generation.

We have added text to the discussion to highlight the broader relevance (Lines 341-347): ". We hypothesized that these distinct topographical conditions and soil types have a definite influence on streamflow generation mechanisms. However, although each catchment has its own set of conditions, most Mediterranean climate catchments would have similar topography to what was modelled here, with hills graduating to a coastal plain. Moreover, because the modelled catchment included a range of soil types, we were able to explore the variation in streamflow generation processes across several soil types. Moreover, it is likely that many other seasonally-flowing catchments would have similar variation in soil, as the periodic and often flashy nature of streamflows carries fine material from the steeper headwaters and deposits it on the plains (Jaeger et al., 2017)."

Comment 3. Is the clay layer in this area continuous? Are there any windows in the clay layer that will cause the water to infiltrate deeper?

Response: No, there are some gaps in the upper parts of the catchment near the fault whereby water can infiltrate more readily to the deeper aquifers. The deeper aquifers do respond to seasonal rainfall.

Comment 4. How do you conceptualize the fault indicated in figure 3 in the model? What do you think its role? What do you think the fault would cause the infiltration of perched groundwater from steep hills into deep subsurface? Then the water will enter the flat valley which means the contribution from GW would be a main component in flat valley?

Response: The fault is conceptualised through the impact it has on the unconfined aquifer, which is that above the fault, the unconfined aquifer is quite thin, and below it, it is much deeper. See figure 4. This would be the main impact on the shallow, unconfined aquifer.

Comment 5. The difficulties of running ISSHMs were discussed at the end of discussion. This study used the HGS, so what do you think other codes which has been well parallelized?

Response: Given the time investment in building such a complex model for HGS or any of other ISSHM codes, it is hard to speculate on how well the same (or near same) model setup would function in another code.

Comment 6. The land use is also variable in space especially the steep hills where the land cover is obviously different from other area. Does the land cover/manning coefficient affect the generation of flow in different subdomains?

Response: It is possible, but we did not carry out a sensitivity analysis on the surface conductance parameters to assess this. It would make for an interesting test in the future, but was beyond the scope of the current study.

Comment 7. The preprocess of river channel including the location and connection based on DEM is not that easy. How do you perform it in this study?

Response: As stated in the manuscript, a Python script was used to depress the stream nodes to account for DEM smoothing and resolution. This is described in detail within the Supplemental Information.

Comment 8. I am also curious why the sequence is hypothesis first and then modeling. Why it is not the modeling first and then analyzing the results to get some conclusions?

Response: We would think that in science we should always first develop the hypothesis and then test it. Here we have not used a "null-hypothesis" approach, but we are still testing it with the model. Moreover, as is commonly advocated within hydrology, we have started simple (e.g. a simple conceptual model), and added more complexity as needed to fit the situation.

Comment: Some words are hard to see in Figure 2.

Response: Figure 2 has been edited to increase text size.

Comment: Lines 7-9 might mislead readers that this is a paper about code development.

Response: We have edited these first lines as suggested by reviewer 1; the challenges associated with numerical modelling of wet/dry river systems are indeed a topic within the manuscript, so they are relevant.

Comment: Line 292, details of 6 CPUs? How many cores of each CPU?

Response: Using the Flinders University HPC Deep Thought, 6 cores were used from a AMD EPYC 7551 @2.55Ghz with 32 Cores / 64 Threads compute node. This information has been added to the manuscript.

Comment: Section 2.4.1, why so many layers? It looks the process in subsurface under the clay layer is not that important in this study. Can the deep layers be reduced to decrease the computational burden?

Response: This is possible; however, where there is unsaturated flow, as many layers as subjectively deemed feasible were implemented to more accurately simulate these flows.

References:

Ebel, B. A., B. B. Mirus, C. S. Heppner, J. E. VanderKwaak & K. Loague, 2009. First-order exchange coefficient coupling for simulating surface water–groundwater interactions: parameter sensitivity and consistency with a physics-based approach. Hydrol Process 23(13):1949-1959 doi:10.1002/hyp.7279.

Liggett, J. E., A. D. Werner & C. T. Simmons, 2012. Influence of the first-order exchange coefficient on simulation of coupled surface–subsurface flow. Journal of Hydrology 414-415:503-515 doi:https://doi.org/10.1016/j.jhydrol.2011.11.028.