Comment on **"Minimum dissipation of potential energy by groundwater outflow results in a simple linear catchment reservoir"** by A. Kleidon and H.H.G. Savenije

Summary of the Paper

Kleidon and Savenije give an explanation why the simple linear reservoir can often be used as a model to describe groundwater outflow to a stream, despite the very complex nature of real catchments. The authors show that the linear reservoir equation can be derived based on the assumption that the potential energy of groundwater of a whole catchment dissipates at minimum rate. They derive the model analytically for idealised conditions. They then discuss why this concept was chosen and how it fits into broader (thermodynamic systems) theory.

Comments

Having read the review of Martijn Westhoff, I realise that I repeat some issues that were already mentioned. However, I didn't remove them since this repetition might suggest that they are actually important.

Generally, the paper is well written and an interesting and innovative contribution to catchment hydrology. It is an ongoing research question, whether and why we so often observe a linear behaviour at the catchment scale. On the contrary, non-linear behaviour is frequently observed, too, and different concepts exist to explain that (e.g. Brutsaert and Nieber, 1977; Wittenberg, 1999; Harman et al., 2009). Although literature on thermodynamic optimality in hydrology exists, the employed approach is rather unusual and based on many assumptions (see comments below).

Sub-catchments The authors use the linear reservoir equation also for the sub-catchments they start with (see Eq. 4: $Q_a = \frac{\bar{z}_a}{\tau_a}$ and Eq. 5). As pointed out earlier and also stated in the paper, we often observe non-linear behaviour (the authors mention that the linear reservoir is "often contested [...] particularly by authors who look at processes at smaller scales"). It would be interesting to read a discussion whether this assumption limits the validity of the result. What would happen if one started with non-linear sub-catchments?

Generally, the authors ask the question why catchments behave as a simple linear reservoir despite the complex reality. It is perfectly reasonable to start with a simple example. However, a result using two very simplified sub-catchments is perhaps not an answer to "why does such a simple equation pertain, while [...] the subsurface is extremely heterogeneous". What if we had more than two sub-catchments? What would heterogeneity result in (e.g. in effective rainfall, soil properties, ...)?

Flux between sub-catchments The authors assume that the redistribution flux Q_{ab} is happening on a faster time scale than the groundwater outflow. It would be interesting to read whether there is more evidence for that assumption (perhaps observations)?

(Sub-)Catchment geometry If I understand it correctly, the example shows two sub-catchments that drain to a specific sub-stream, which then discharges to the large stream. This setup is different to the one shown on Figure 2, where it seems as if both sub-catchments drain to the same stream. The authors mention on page 4, line 8, that the channel has a dendritic structure. After reading the whole paper, I suppose this points to cases like in the example, where there are two (or perhaps more) channels eventually draining to one channel and not just one central channel throughout the whole catchment. Maybe it is just me not understanding it correctly. However, it might be helpful if the setting would be explained more clearly, and also to know if the example is comparable in terms of its general geometry.

Ourthe river example The authors clearly state that their quick calculation is no rigorous validation and this might not be the emphasis of this work. Nonetheless, could it be made more rigorous by adding some more calculations? This could imply other data from the same catchments to see if they are consistent, and data from other similarly arranged (sub-)catchments. The current time-scales are all quite similar, and probably the associated soil and hydro(geo)logical properties. This means that the resulting value of an overall time constant lying between the two sub-catchment time constants could perhaps be explained otherwise, e.g. by simple averaging (which I assume is implied by page 7, line 24). Such further calculations could probably be done quite quickly and would certainly give more insight.

Justification of minimum dissipation approach The minimisation of dissipation by groundwater outflow is the main hypothesis and it is underpinned by looking at a broader system, including the unsaturated zone. The authors hypothesise that dissipation is minimised in the saturated zone, while being maximised in the unsaturated zone. This theory is strengthened by referring to similar approaches dealing with river systems. Is there any other evidence for the minimum dissipation approach (in preference to the maximum dissipation approach)? Furthermore, can the separation between saturated/unsaturated zone be assumed to be that distinct in nature, considering e.g. a varying groundwater table or a large capillary fringe. Although it is admitted, that many of the hypotheses need to be tested and looked at in more detail, I think further arguments (or empirical evidence, as mentioned above) would help to strengthen the main hypothesis.

Units There are some inconsistencies regarding the units. In Table 1, the unit of area is given as m^{-2}, but should rather be m^2. Also, according to Table 1, Q_i, P_i, etc., are mass fluxes. The transformation to volume fluxes is straightforward if we assume that density and porosity are constant (this transformation is indicated on page 6, line 4). However, many equations miss the density ρ and porosity n if the Q_i are assumed to be mass fluxes (e.g. $Q_i = \frac{\bar{z}_i}{\tau_i}\rho n_i$). It seems to cancel out in the following equations, so that it should not influence the resulting equations.

Summary

The paper tries to explain an interesting and still unresolved question in an innovative way and is hence clearly of value for the hydrological sciences. It contains many assumptions, most of them simplifying the setup. While this is reasonable in order to find a simple analytical solution, their impact could be discussed more extensively (what if they are not satisfied?). The main assumption is the minimisation of dissipation. Since it forms the basis of the whole approach, a more detailed reasoning and discussion of that would be desirable. The paper focuses on the presentation of an idea and not on its validation, which is generally fine. However, more testing (calculations) of the proposed theory should be relatively straightforward and could justify the theory in an empirical way.

Kind regards,

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References

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