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Discussions

Interactive comment on "Minimum dissipation of potential energy by groundwater outflow results in a simple linear catchment reservoir" *by* Axel Kleidon and Hubert H J. Savenije

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Review of "Minimum dissipation of potential energy by groundwater outflow results in a simple linear catchment reservoir" - Kleidon and Savenije.

In this manuscript, the authors show that simple linear behaviour of a catchment results from the minimization of dissipation by the draining fluxes of two sub-catchments, in which a flux between the two catchments optimizes itself to reach this minimum. To my opinion, it is a very novel contribution, shedding a new light on why so many catchments behave as a linear reservoir. However, there are some minor issues which should be

improved before publication.

The first one is about the main message: The authors claim that the linear behaviour is caused by a connecting flux between two sub-catchments, which minimizes the dissipation of the draining fluxes. However, they start with the assumption that the sub-catchments already behave as a linear reservoir. So what this manuscript tells us that if two sub-catchments behave linearly, the sum of the two also reacts as a linear reservoir. And with the proposed method it is possible to also predict the timescale and mean height above the drain. It does NOT explain why a sub-catchment behaves like a linear reservoir.

A second point considers the used units: Especially the units of the fluxes are inconsistent throughout the manuscript. In Eq. 1-9 and 17-19 they are given in m/s, while in Eq. 10-14 and table 1 they are given as a mass flux per unit area. Also the storage S is not consistent: in Eq. 1 it is in m, while directly after Eq. 9 it is given in kg. The dissipation terms (D) in table 1 are given in W/m2, while in Eq. 10-13 they should be in Watts.

A third point is that in the example of the Ourthe, the authors show that tau can be reasonably well predicted. However, they do not report other state variables or fluxes. For example, when a precipitation of 1000 mm/year is assumed, z_a and z_b would be about 20 cm. The authors should make it plausible that this is a reasonable value.

As a last point I think the authors take to big steps when discussing why the dissipation by Q_{ab} should not be maximised (P9, L3-6): The main reason they give is that when doing this, z_a is not equal to z_b. But why do they have to be the same? The only reason I can think of is that with an equal z, it is possible to obtain an effective tau, which is close to (only) one observation. But one observation does not prove the hypothesis of this manuscript. The authors do recognize this 'lack' of sufficient observations to fully accept their hypothesis, but they are, in my opinion, too fast in rejecting the hypothesis of maximum dissipation by Q_{ab}.

Some very minor points:
P1, L4: maybe refer to the master recession curve as well? Or maybe even better: create a Master recession curve of the Ourthe data.
P3, L9: from the analysis in this paper, leakage is even required to obtain a linear reservoir.
P4, L10: In the derivations, it does not seem to matter if the reference level is set to zero or not. However, I suggest to leave arbitrary reference levels in the derivation, because this simplification causes Q_{ab} to be zero in the optimum state, while the point the authors want to make is that this flux is essential to obtain this optimum state
Table 1: A_a and A_b are in m^2
Eq. 11: Add that dissipation is determined under the assumption that the change in kinetic energy is negligible.
Eq. 18: Although this equation gives a weighted mean, it is not the geometric mean, which is given by $\tau_a^{A_a/A} * \tau_b^{A_b/A}$.

With kind regards,

Martijn Westhoff

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