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Discussions

Interactive comment on "Combining analytical solutions of Boussinesq equation with the modified Kozeny–Carman equation for estimation of catchment-scale hydrogeological parameters" *by* Man Gao et al.

Anonymous Referee #1

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Title: Combining analytical solutions of Boussinesq equation with the modified Kozeny–Carman equation for estimation of catchment-scale hydrogeological parameters

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GENERAL COMMENTS

This paper presents a means to constrain values of hydraulic conductivity, effective porosity, and aquifer depth estimated from recession analysis by using an empirical relationship between hydraulic conductivity and drainable porosity that is a function of the pore size distribution, the latter being estimated from soil texture properties.

I think the concept has merit, is worth pursuing, and could lead to improved estimations of aquifer parameters. The theoretical part of the paper could be published but requires a better explanation of the assumptions that underlie the analytical solutions and a discussion of the implications of these assumptions when applying the parameter estimation technique to real world situations.

Where the paper falls short, and why I recommend this paper not be published, is in the application to real data. To claim that an early-time with b = 3 and late time regime with b = 1 exist in the aggregate of the data from any of the four watersheds is a very large stretch. An examination of individual recessions in the dQ/dt vs Q would likely reveal that such behavior (b = 3 to b = 1) is simply not present in the receding limbs of the hydrograph. Studies examining individual recessions have begun to show that the pattern in aggregated data, including apparent lower envelopes, does not represent constant aquifer properties (e.g., Biswal and Marani, 2010; Shaw and Riha, 2012; Mutzner et al., 2013; McMillan et al., 2014; Basso et al. 2015; Karlsen et al,. 2019; Santos et al., 2019). Jachens et al. (2019) in particular demonstrate the fallacy that the apparent pattern of the aggregated data in dQ/dt vs Q space (e.g., envelopes of b = 3 to 1 or other values of b estimated directly from aggregate) represent aquifer properties but rather arise from properties of the climate (i.e., magnitude and interarrival times of recharge events). Even to the extent the patterns do reflect aquifer properties, the authors do not show that the single Boussinesq aquifer (much less the simplifying assumptions required to achieve the analytical solutions) is a “good enough” representation of complex watershed made of multiple hillslopes and landscape scale heterogeneity in hydraulic properties to allow them to estimate aquifer properties using the proposed technique.

A more appropriate first test of the technique would be in a laboratory setting where the aquifer properties are consistent with a “Boussinesq” aquifer and the aquifer boundary conditions and initial conditions conform to those for which the analytical solutions were derived. Another appropriate test could be for a single and well-instrumented hillslope where the boundary conditions and initial conditions can at least be measured, if not controlled. Datasets exists for both situations.

SPECIFIC COMMENTS

53: Assumptions of the analytical solutions of Brutsaert and Nieber (1977) do not precisely include a relatively humid setting. Precisely, the solutions assume an initial saturated thickness of equal depth along the length of the hillslope. Mathematically, this could result from a spatially uniform pulse recharge to an initially dry aquifer.

58-59: This statement is incorrect. Mendoza et al (2003) did not improve the solution of Parlange et al. (2001). They simply tweaked with the lower envelope fitting technique because they didn't observe a b = 3 regime.

87-89: How appropriate are these pedostransfer functions for fractured bedrock and less-weathered saprolite?

119: "x" is not the distance from the river to the hillslope ridge. This would be "B".

125-134: The authors leave out mentioning that linearization of Eq (2) is required to obtain the solutions presented here, and do not say what that linearization implies physically.

131: Eq. (3) is true as time goes to zero, or if advection is excluded, as Brutsaert (1994) states. The authors don't mention this.

136: I don't see Eq. (4) in Brutsaert (1994). Can the authors say what equation in Brutsaert (1994) this is meant to be?

153: How do the authors reconcile that fact that the b = 1 here is an artifact of the linearization of the sloping Boussinesq equation and does not occur if the equation is not linearized (e.g., Bogaart et al. 2013)?

162-169: The authors may want to see Roques et al. (2019), who present an improvement to Rupp and Selker (2006a).

213-214: What does these slopes, along with the aquifer depth and length, imply for the validity of the sloping and non-sloping Boussinesq and their various solutions? The “Hi” term is useful dimensionless number in this regard. See, for example, Brutsaert (2005) and Rupp and Selker (2006b).

226: Vertical vs horizontal K can be very different in bedrock. A falling head permeameter and the assumption about the shape of the wetting front made in estimating K make it not a suitable instrument for determining this. The authors should comment on possibly large errors in estimating K.

248-249 and Table 2: Are these values of drainable porosity high for bedrock and some saprolite? Can the authors compare with directly measured “f” for bedrock from other locations, to see if these values derived from the pedotransfer functions are unusually high?

252-253 and Figure 3: The recessions in Figure 3 do not support convergence to a value of 1 at late time. How do the authors reconcile the lack of data to support a lower envelope with a slope of 1with their subsequent estimation of the aquifer parameters? Clark et al. (2009) and Wang (2011) propose different conceptual models for PMRW, with at least two water sources contributing to the streamflow. Both are able to mimic the observed dQ/dt vs dQ pattern much better than can the single homogeneous Boussinesq aquifer assumed by the authors.

267-268: I would say that 35.6 days at PMRW is not relatively fast compared to 45 +/- 15 days, but well within that range. PMRW has a similar D to HMQ and WS10, so D does not explain why PMRW is “slower” than HMQ and WS10. Yet the authors use D to try to explain why SPG is

356-357: Can the authors comment on how these values for f compare to those in Brutsaert and Nieber (1977) and Brutseart and Lopez (1998)? They also calculated

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values for f.

385-499: It is good that the authors consider to what degree discrepancies are due to riparian area impacts. Can the authors estimate the volume of water that must be stored to explain such discrepancies. Does it exceed riparian storage?

TECHNICAL/EDITORIAL CORRECTIONS

84: . . .of a soil pore. Or maybe better it is more correct to say the distribution of hydraulic radii of soil pores.

118: . . ., eta is the water table height above an impermeable layer,. . .

171-172: ". . .only the latest data are involved in the calculations. . ." It is not clear what this means.

200: The gamma term appears as an "r" in Eq. (19) in my pdf file.

219: Referring to any catchment as "famous" in this paper is unnecessary.

233: textures should be texture.

254-255: Should be either Brutsaert and Nieber (1977) or Brutsaert and Lopez (1998)

256 and 257: envelop should be envelope in both instances.

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