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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Is the groundwater reservoir linear? A mathematical analysis of two limiting cases" *by* G. H. de Rooij

G.H. de Rooij

gerrit.derooij@ufz.de

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Referee 1 (R1) views the paper favourably, recognizing the contribution it makes to an on-going debate. R1 raises some constructive comments that I address below.

In the first comment, R1 points out that the set of aquifers considered does not cover the full range of possibilities. The situations (s)he describes (macropore flow, discontinuous groundwater tables, cascades of bedrock depressions) are all quite real, but also well out of range of the analytical treatment presented here. Particularly macropore flow (through fissures) and cascades of bedrock depressions mostly occur in erosive landscapes (e,g. hillslopes). Many aquifers of interest occur in sedimentary systems, and consist of extensive beds of gravels and sands. Such systems are more amenable

to analytical treatment, and have traditionally been analysed in that way with considerable succes. In such systems, macropores only occur in well-consolidated clay, caused by irreversible shrinkage after prolonged exposure to air. Such clay layers typically rest on top of an aquifer of more conductive material. Groundwater tables that are nearly discontinuous can be observed at the edges of rifts that move downward slow enough to allow sedimentary filling of the depression developing over the downward-moving rock. At the interface, distortion of the soil layers and smearing of clayey material locally reduces the hydraulic conductivity, leading to very local large hydraulic gradients (within a few meters) and a corresponding jump in the phreatic level. R1 is correct that this phenomenon is not easily captured. But it is also quite specific and well known locally to allow case-specific modelling. Overall, the comment is concerned mostly with features that appear in erosive landscapes. I agree with R1 that the approach presented here can run into trouble in such areas for the reasons outlined by R1.

Nevertheless, I did include recent analyses of hill slopes. As these invariably showed non-linear behaviour I did not consider them further. Still, the assumptions made in those papers (smooth bedrock surfaces without fissures that support an aquifer with a surface similar in shape to that of the bedrock) seem to require closer scrutiny, if I interpret R1's concerns correctly.

To address the concerns expressed in this comment I can point out in the revision that the analysis presented here is expected to have its largest validity for sedimentary systems in which the sedimentary regime and other geomorphological processes prevented sharp contrasts in the geohydrological properties. This should give a clear indication of the constraints of the analysis. It should be noticed that such constraints are already alluded to in the final paragraph of section 2 and the first sentence of the Conclusions.

R1's second comment about the nature of the non-linearity of the storage-discharge relation is interesting. (Note that R1 states that I claim the relationship is non-unique, but I have not done so in this paper, so I presume non-linearity is intended here.)

When re-reading the paper with this comment in mind I realized that I demonstrated the deviation from linearity but did not investigate its nature or examined the degree of non-linearity under realistic rainfall regimes. I agree with R1 this would be worthwhile to elaborate on. The inclusion of more flexible external forcings requires a more general analytical solution, the derivation and coding of which is not trivial. Nevertheless I am willing to see if I can pursue this to add more material to the revision. As this is a substantial extension I believe this would also address R1's final comment.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 83, 2014.