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Interactive Comment

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

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Referee 2 (R2) is more critical then R1: the contribution of this paper is lost on this referee, in part because R2 expects it to be in new equations instead of an analysis of the ones developed by me and others before me in light of the more widespread linearity of aquifers hypothesized by Fenicia et al. (2006) in their analysis of the effect of recharge. The choice of wording ('apparently motivated') does not do justice to my very clear expression about the relationship between Fenicia et al.'s paper and this work.

In an aside and later in the comments R2 seems to be confused by my definition of the upscaled hydraulic conductivity. This term occurs twice in the paper and is used as a shorter reference to the aquifer-scale hydraulic conductivity, a term that appears throughout the paper. I will eliminate these occurrences and opt for aquifer-scale conductivity throughout. Its background is explained in detail in de Rooij (2012) (not de Rooij, 2013, as R2 suggests). R2 proposes the term conductance instead. This term is closely related (but not exactly equal to) the transmissivity. The reasoning behind this is understandable if one strictly thinks within the Darcian framework: at the aquifer-scale, the Dupuit assumptions impose strictly horizontal flow, and the total discharge becomes proportional to the aquifer thickness, making the transmissivity (KD) a logical parameter to describe an aquifer. But part of my analysis focuses on the scale of the entire aquifer, as the title of section 2 indicates. At that scale, I relate the flux between the aquifer and the surface water to the difference in the average hydraulic potential between these bodies. I found it more elegant to incorporate all hydrogeological parameters (aquifer length, depth, and porosity) into the expression for the aquifer-scale hydraulic conductivity rather than arbitrarily leave one out, as R2 proposes. This is more a matter of taste than of fundamental groundwater hydrology though.

I have more fundamental objections against the use of the term conductance. This keeps the analysis firmly in the domain of the Darcian scale, where flows are driven by gradients and therefore described by 2nd order PDEs as explained in the paper. In earlier works (mentioned in the reference list) I elaborated on the transfer from the Darcian scale to the aquifer scale and demonstrated the analogies and differences in the governing equations at both scales, and used those to justify the choice of the terminology. I prefer to stay in line with this earlier framework to maintain the connection with this work.

R2 provides detailed comment to each of the sections of the paper. I will address these in the order in which R2 presented them.

Abstract.

HESSD

11, C643–C648, 2014

The repetitiveness in the phrases in lines 8-11 and 13-16 is correctly pointed out by R2, and I will rephrase this.

In a paper that asks in the title if groundwater reservoirs are linear, the abstract points out that this is the case only under rather limiting conditions. The answer will therefore tend towards: 'No'. R2 appears to agree.

In the comment about the characteristic time the author uses different wording to state what the abstract states. There does not appear to be a suggestion or a need for change, since R2's interpretation is correct.

The final statement underestimates the strong position of the linear reservoir approximation in groundwater hydrology. I have even been asked why I did this analysis because 'we already know groundwater reservoirs are linear'. A warning against an overconfident reliance on the linearity of such reservoirs seems to be in order, particularly if compensation elsewhere in a model leads to incorrect estimates of solute loads. I am not sure why R2 objects to this.

Section 1.

The suggestion to expand the title makes sense. Further justifications may be necessary if the analysis is expanded according to my reply to R1.

Section 2.

I chose the term potential it because I wanted to highlight the common property of all potential flows (porous media flow obeying Darcy's Law, diffusion obeying Fick's Law and electrical currents obeying Ohm's Law). The dimension provided [L] makes clear that the potential is defined on the basis of weight (not mass or volume). The three equivalent definitions of potential and their dimensions are textbook material, and the term potential is widely used. The choice of terminology of the aquifer-scale hydraulic conductivity was addressed above.

R2 goes on to remark that Figure 1 does not show an aquifer with a stream on one

HESSD
11, C643–C648, 2014

hand and a no-flow boundary on the other. The figure does not do so because near vertical rock faces providing a no flow boundary at some distance from a stream are not very common in nature. Much more frequent are aquifers intersected by several streams, canals, ditches, etc. Midway between these, infiltrating water has to turn right or left to be drained by one of the adjacent streams. This is the boundary between the watersheds of the two streams, and is mathematically equivalent to a no-flow boundary. This elementary principle is explained in many hydrological textbooks and does not require a full explanation in a paper.

Section 3.

Equation 5 is much older that R2 believes: it is the well-known linearized version of the Boussinesq equation that forms the foundation of many drainage theories. Several of the solutions developed there are special cases of the solutions that form the basis of my analysis by the way. There is no harm in, nor is any convention overruled by introducing a well-known governing differential equation upon which an analysis presented in a paper is founded, and that is what I do here. This comment therefore surprises me.

The remainder of this comment is incorrect. This form of the equation requires H(x) to vary so little compared to the aquifer thickness D that they can be considered equal for the derivation of the solution. Indeed, this is what linearizes it. Therefore, only the original, non-linear form of the Boussinesq equation was used to derive a solution for zero aquifer thickness at the stream. This is all accurately explained in this section (and in various text books).

The comment about the seepage face is remarkable, as it does not criticize my solution, but that of Boussinesq himself. To criticize in 2014 a much-used solution developed in 1904 for relying on conditions that are labelled ‘obviously not true’ seems to be a bit late, and appears at odds with the (correct) qualification as ‘famous’ of the paper by Brutsaert and Nieber (1977) that heavily leans on that very solution.

HESSD

11, C643–C648, 2014

Interactive Comment

Section 4.

This comment mainly recaptures correctly the main points of this section. R2 wonders what is characteristic about the characteristic time, even though this time is unequivocally defined in Eqs. (12) and (13), and quantified in Eqs. (15) and (17). The use of the term ‘characteristic time’ in conjunction with decaying exponential functions as done here is well-established.

Section 5.

The two sentences immediately following the one that made R2s wonder what I meant give the answer by explaining the why the non-linearity observed by Fenicia et al. (2006) proved to be apparent only upon closer inspection. Fenicia et al. (2006) explain this in more detail.

Section 6.

De Rooij (2013) only concluded that aquifers behave non-linear part of the time in a qualitative sense only. The different lingering times of non-linear behaviour caused by changes in recharge and by changes in surface water heads are quantified in terms of characteristic times for the first time in this paper, which allowed me to estimate how long aquifers of various sizes would behave in a non-linear fashion.

Conclusions

The first statement is incorrect: I analysed the storage-discharge relationship of Brutsaert and Nieber (1977) for strongly curved phreatic levels in considerable detail so I could conclude they never display linear behaviour – R2 referred to that conclusion in an earlier comment yet here denies that I carried out such an analysis.

The rest of the comment recaptures the conclusions. The statement that stream lines are not parallel is made rather haphazardly, disregarding the lack of curvature in lines of equal hydraulic potential that can be seen in many groundwater maps, the successful application of drainage theory based on very similar assumptions and the not infre-

HESSD

11, C643–C648, 2014

Interactive Comment

quent practice of ‘straightening out’ rivers in practical hydrological modelling: these are all indications that flow lines are sufficiently close to parallel for the assumption to give useful results. Nevertheless, it is a point that one needs to take into account when applying such analytical approaches. That being said I consider it unlikely that non-parallel stream tubes in a more heterogeneous medium are going to make the aquifer respond more like a linear reservoir, which is what the paper set out to explore.

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

HESSD

11, C643–C648, 2014

Interactive Comment