Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1
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Comment on hess-2021-634

Anonymous Referee #1

Referee comment on "Effects of the dynamic effective porosity on watertable fluctuations and seawater intrusion in coastal unconfined aquifers" by Zhaoyang Luo et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-634-RC1, 2022

Firstly, I note that this is the third time I am reviewing this work which has been previously submited to and rejected from two other journal publications. My key concerns remain (see detailed comments below) and consequently I am unable to recommend publication of the submitted paper.

Abstract

li 27: "After validation with 1D experimental data and numerical simulations ..."

li 31-33: "accounting for vertical flow" - the equations used are the 2nd order approximation correction for vertical flows, there is an infinite order expression that has been excluded from the analysis (cf Nielsen et al, 1997). It is anticipated that the later (more accurate) correction vertical flow effects will not yield such favourable results.

li 35-38: "the phase lag can be ignored" The observations in Table 6 of Shoustari et al (2017) indicates that, for a 2DV propagating groundwater wave system, the phase lag in moisture content fluctuations is much greater than that for the watertable fluctuations. In addition, the dynamic effective porosity presented by the authors (eq 7) is derived based on a 1D sand column system so this is somewhat contradictory. I note that a reviewer in a previpous submission was also critical of this assumption.

li38-41: this has long been known.

Highlights

1. the "modified expression" is the same as Pozdniakov et al. (2019). Equation 7a is the same as Pozdniakov et al. (2019), it has just been written using different notation. If you insert the authors' equations 9 and 7b into 7a you get the same equation as when you insert Pozdniakov et al's eq 12 into eq 15. The author's fitting parameter a being equivalent to 2.pi.f(l,m)/tau0 in the notation of Pozdniakov et al. (2019). Therefore the correct description of what has been done is "Here we introduce the existing formulation of Pozdniakov et al. (2019) using different notation ... "

2. Only for the 2nd order approximation, the infinite-order correction for vertical flow effects has been omitted by the authors

Main Body

li 118-119: review language. It is known and agreed upon that the unsaturated zone does affect water table fluctuations. The dynamic efective porosity is a way of paramaterising this affect. Therefore it follows that the dynamic effective porosity will affect water table fluctuations. It is the extent to which, and our ability to correctly quantify the dynamic effective porosity that remains unclear.

li 120-121: I agree that using a complex effective porosity in a practical application (e.g. numerical model) is not possible but note that Cartwright et al (2006) overcame this by using the absolute value of the complex number which led to reasonable outcomes for practical use.

li 122-124: The influence of water table fluctuations on salt water intrusion is significant at long time scales (e.g. tidal - Robinson et al, storm surges - Cartwright et al). At these longer time scales, the influence of the unsaturated zone (and hence the dynamic effective porosity) on water table fluctuations becomes negligible (refer to all available dispersion relation theory and even the authors results showing that their nt/ne ~ 1 for small values of Tau_w).

li 153-1161: The authors acknowledge that this is a 2nd order correction for vertical flow effects, however Nielsen et al (1997) also provide an infinite-order solution which should be included in the analysis. I note that it is included in a supplementary figure S2 but it should be added to Figure 4 with the authors nt expression in place of ne. I anticipated that this will yield a much poorer comparison with the data and will highlight the somewhat fortuitous outcome that the 2nd order solution provides a reasonable comparison.

li 181-193: It is not clear to me how the equation is modified from the existing. Whilst the

authors' modified dynamic effective porosity has been derived differntly the result is the same as Pozdniakov et al. (2019) albeit with a differnt notation. If you insert the authors' equations 9 and 7b into 7a you get the same equation as when you insert Pozdniakov et al's eq 12 into eq 15. The authors' fitting parameter "a" being equivalent to 2.pi.f(l,m)/tau0 in the notation of Pozdniakov et al. (2019).

li 199: are they wetting or drying curves?

li 207-209: I would argue that the authors' approach is also approximate because, ultimately at the end their eq 7 is semi-emprical and requires fitting to data.

li 255-257: In figure 1 there is a clear departure between the curve fit and the data as nwHpsi/K increases (and nt/ne decreases) indicating poor performance where the influence of the unstaurated zone on water table fluctuations is large (ie small nt).

li 258-262: the limited ability of numerical solutions to Richards' equation to reproduce the lab data when neglecting hysterisis is discussed in depth in Cartwright et al (2005)

li 288: Nielsen and Perrochet (2000a,b) first proposed the complex effective porosity concept

li 290-292: It is important to clarify that, regardless of whether the system is 1D or 2DV, water table fluctuations are induced by external forcing at a boundary (ocean tides, wave, atmospheric pressure ...). Moisture content may, or may not, play a role in influencing the nature and extent of the response. Also note that the phase lag between moisture content fluctuations in the unsaturated and those in the water table, is also present in 2D systems (Shoushari et al, 2017).

li 298: cite the source of the experiments

li 342: as per my earlier comment, I anticpate that if the infinite-order solution was used rather than the 2nd order one the comparison will be much worse. Please add these curves to your Figure 4.

li 347-349: I disagree. As per my earlier comments, it is rather fortuitous that the 2nd order solution seems to do OK.

li 353-356, Fig 5 and 6: For a more rigorous comparison, rather than time series, present both the amplitude and phase profiles (ie A vs x and phase lag vs x)

sec 3.3: It seems to me that the approach adopted to examine the influence of the dynamic effective porsoity (ie the link between saturated and unsaturated zones) on saltwater intrusion is fundamentally flawed. As described, SUTRA solves the variably-saturated flow equations so therefore the moisture exchange between saturated and unsaturated zones is implicitly accounted for in the governing equations. To then replace the storage coefficient with a reduced dynamic effective porosity does not make sense phsyically as you are essentially accounting for the exchange twice.

References

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