Responses to Referee #1's comments

Firstly, I note that this is the third time I am reviewing this work which has been previously submitted 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.

Thanks to the public review process of *HESS*, we have the opportunity to clarify the history of this work.

Versions of this work were rejected by two other journals (*WRR* and *JH*). However, contrary to the implication of reviewer's text, the manuscript has changed. Furthermore, for both journals, the other referees were positive concerning the manuscript.

This work solves a problem that was previously unexplained – this is the contribution of this work. The reviewer does not acknowledge this unfortunately. This is a fundamental point that should be considered and the reviewer's points do nothing to change this.

Abstract

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

We will change to "After comparing with both 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 favorable results.

There is no point to consider the infinite-order expression of Nielsen et al. (1997) given that its predictions do not agree with experimental data. For example, Shoushtari et al. (2016), who considered the infinite-order expression, state that "in the high frequency limit ($n\omega d/K \rightarrow 1$): (1) the theory predicts a zero phase lag ($k_i = 0$) corresponding to a standing wave scenario and (2) the amplitude decay rate has an asymptotic value of $k_r = \pi/2d$. The present data however will be shown to contradict this with both k_r and k_i observed to monotonically increase with increasing $n\omega d/K$". The modified effective porosity presented in the manuscript does nothing to change this finding. Furthermore, as shown by Shoushtari et al. (2016) in their Fig. 4, the **only** theory that agrees with experimental data is the so-called second-order theory. Thus, we considered only this latter theory in this manuscript.

li 35-38: "the phase lag can be ignored" The observations in Table 6 of Shoushtari 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 previous submission was also critical of this assumption.

There is no contradiction. We are **not** predicting soil moisture in the vadose zone, which the reviewer is referring to. Indeed, this manuscript does not concern this, rather watertable fluctuations and seawater intrusion. As stated in the manuscript: "Results show that the Boussinesq equation accounting for the vertical flow in the saturated zone and dynamic effective porosity can accurately predict experimental dispersion relations (that all existing theories fail to predict), highlighting the importance of the dynamic effective porosity in modeling watertable fluctuations in coastal unconfined aquifers. This in turn confirms the utility of the real-valued expression of the dynamic effective porosity. An outcome is that the phase lag between the total moisture (above the watertable) and watertable height measured in laboratory experiments using vertical soil columns (1D systems) can be ignored when predicting watertable fluctuations in coastal unconfined aquifers (2D systems)." We see no reason to change this text.

li38-41: this has long been known.

Please give citations that prove the dynamic effective porosity facilitates watertable waves propagating further landward.

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 ..."

We note that the reviewer recognizes that this expression was derived in a different way later.

This is a semantic point essentially. In Pozdniakov et al. (2019), $f(l,m)$ is an approximate expression (their equation 12) whereas in this manuscript it is an exact expression derived using the modified van Genuchten model (equation 8). So, it is incorrect to assert that it is a matter of "different notation". It is, on the other hand, accurate to describe our formula as a modification of their formula, as done in the text.

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

Please see our reply at the bottom of Pg. 1.

Main Body

li 118-119: review language. It is known and agreed upon that the unsaturated zone does affect water table fluctuations. The dynamic effective porosity is a way of parameterizing 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.

We will correct this sentence.

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.

We will mention this work in Introduction. Cartwright et al (2006) adopted an absolute value of the complex number and got reasonable outcomes. Their approach further confirms that the phase lag between the total moisture (above the watertable) and watertable height can be ignored when predicting watertable fluctuations in coastal unconfined aquifers.

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).

We disagree. Kong et al. (2015) showed that unsaturated flow effects play an important role in affecting watertable fluctuations with long time scales.

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.

Please see our reply at the bottom of Pg. 1.

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 differently the result is the same as Pozdniakov et al. (2019) albeit with a 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 authors' fitting parameter "a" being equivalent to 2.pi.f(l,m)/tau0 in the notation of Pozdniakov et al. (2019).

Please see our reply on Pg. 2 starting with "We note that the reviewer recognizes …"

li 199: are they wetting or drying curves?

We will clarify that they are 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-empirical and requires fitting to data.

The reviewer has apparently misread the text. The cited lines (again) point out the difference in derivations as described in our reply on Pg. 2 starting with "We note that the reviewer recognizes …".

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 unsaturated zone on water table fluctuations is large (ie small nt).

There is a deviation, yes, but we consider it to be acceptable for the effective porosity in terms of results. Indeed, the Boussinesq equation and numerical model with the dynamic effective porosity calculated from Eq. 7a accurately predict watertable fluctuations (Figures 4-6) and seawater intrusion (Figure 7), respectively.

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

We will mention this work.

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

We will correct this.

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

(Shoushtari et al, 2017).

It is obvious that watertable fluctuations are "induced by external forcing at a boundary", as already indicated in the text. We see no point to repeat this at these lines.

Concerning the phase lag, see our reply at the top of Pg. 2.

li 298: cite the source of the experiments

We will add the references (i.e., Parlange et al., 1984; Cartwright et al., 2003; Shoushtari et al., 2016).

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.

Please see our reply at the bottom of Pg. 1.

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

Please see our reply at the bottom of Pg. 1.

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)

Since we are predicting watertable fluctuations, it is more direct to present time series of watertable in different locations.

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.

The static effective porosity (equal to the saturated soil water content) is also used in Richards' equation. Therefore, the static effective porosity also can be corrected by replacing the dynamic effective porosity. A recent *WRR* paper (Zheng et al., 2022) confirms that this approach is reasonable. It should in particular be noted that our dynamic effective porosity was adopted in this paper and the authors concluded "Simulation using both Cartwright's 'wetting and drying' model and *Richards' model with dynamic effective porosity* are used to evaluate experimental results, with the latter model providing a better match for large capillary-fringe truncation”.

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