Responses to Referee #2's comments

The study is keyed to proposing an empirical expression to evaluate a dynamic effective porosity and assess its impact on the quantification of watertable fluctuations and seawater intrusion in coastal aquifers. After studying the work, I am afraid I am not in a position to recommend publication at this stage. In addition to having some doubts about the possibility that this study constitutes more than an incremental advancement, at least the way it is framed and the way the Authors present it, I do have two major concerns. When combined, these seriously question the validity of the approach and of the key results of this work.

We use the concept of a dynamic effective porosity to resolve a problem highlighted in the recent paper of Shoushtari et al. (2016). As further explained below, these authors presented experimental data on coastal aquifer watertable fluctuations that could not be modelled by existing analytical results or by Richards' equation. The dynamic effective porosity is shown to produce results that agree reasonably with a range of data from careful experiments.

The Authors observe that considering vertical flow effects making use of (i) an approximated (at second-order) formulation and (ii) a dynamic effective porosity leads to an accurate prediction of experimental dispersion relations of watertable waves. This result is in contrast with a previous analysis according to which it is shown that an infinite-order expression (that includes the second-order approximation presented in this study) cannot predict these results in an accurate way. In order to resolve this issue the authors should compare their results as well as the infinite-order expression against outcomes of the Richards' equation (which accounts for vertical flow under variably saturated flow settings). It can also be noted that, in addition to the theoretical elements described above, the physical basis according to which an approximate solution should provide improved results as opposed to its exact counterpart is not clear.

As we explain below, the reviewer asks that we repeat work that is already well documented in the literature (Shoushtari et al., 2016), and is described in the manuscript (lines 104-111, 314-319).

The reviewer mentions approximations presented by Nielsen et al. (1997). These authors considered fluctuations in an aquifer with a vertical boundary at which periodic (hydrostatic) fluctuations are imposed. Effects of the vadose zone above the watertable were not considered. The dispersion relations referred to by the reviewer were designated by Nielsen et al. (1997) as the "second-order small amplitude" and "infinite-order small amplitude" equations (Nielsen et al., 1997, their Eqs. 16 and 17, respectively). Nielsen et al.'s approximations, along with other similar approximations (i.e., shallow, capillarity free aquifer, shallow with capillarity effects, non-shallow, capillarity free, non-shallow with capillarity effects), were already compared with experimental data (Shoushtari et al., 2016, their Fig. 4), and with numerical solutions of Richards' equation (Shoushtari et al., 2016, their Fig. 8), viz.,

- Experimental data. Referring to their Fig. 4, Shoushtari et al. (2016) state "The data indicates a monotonic increase in wave number with increasing $n\omega d/K$ which is in direct contrast with most of the dispersion relations which predict: (1) zero phase lag ($k_i = 0$)." Indeed, the only theory that predicts the non-zero phase lag of the experimental data is the second-order theory. Shoushtari et al. (2016) described the second-order theory as "The observed monotonic increase in wave numbers with increasing $n\omega d/K$ appears to be captured by the 2nd order (in $n\omega d/K$), capillarity free dispersion relation." The main difference is that, in contrast to the second-order approximation, the other theories (including the infinite-order theory of Nielsen et al., 1997) predict standing wave behavior for large $n\omega d/K$. We emphasize that the second-order theory is the only theory that gives predictions that agree with the experimental data, i.e., the experimental data of Shoushtari et al. (2016) serve to validate the second-order theory.
- Numerical solutions to Richards' equation. This was also done by Shoushtari et al. (2016, their Fig. 8), who presented results with and without hysteresis included. Like the infinite-order approximation, the Richards' equation results did not agree with the experimental data: "Whilst the inclusion of hysteresis effects have led to some small quantitative differences when compared to the non-hysteretic results, both sets of results show the same qualitative discrepancy when compared to the data" (Shoushtari et al., 2016). This means that the standard Richards' equation model should not be used to simulate propagation of high-frequency fluctuations.

The reviewer's final sentence refers to the infinite-order approximation as the "exact counterpart" of the second-order approximation. However, the infinite-order approximation is not exact if one considers that it is based on the Boussinesq approximation, assumes small amplitude fluctuations, ignores the vadose zone, etc. As indicated in the present study, the vertical water exchange between the saturated zone and unsaturated zone varies with the fluctuation frequency. Repeated analytical corrections without accounting for unsaturated zone effects may lead to an inappropriate estimate to the hydrostatic pressure and hence deviations between predicted and experimental results.

I found the approach adopted in modeling the saltwater intrusion not convincingly supported. To the extent of my knowledge, the code adopted (SUTRA) already solves variable saturated (saturated-unsaturated) flow settings. Therefore, while a model parameter such as a dynamic effective porosity could be considered and included to account for the effects of the unsaturated zone on water table dynamics when these effects have not yet been considered (e.g., when using saturated models such as Eq.1-2 of the manuscript), I strongly doubt about its use and physical implications when solving the Richards' equation. The latter already accounts for the unsaturated zone and its impact on subsurface flow dynamics. As such, I find the approach to be inconsistent and not substantiated by robust physical bases.

Richards' equation models unsaturated flow, however, it fails to describe unsaturated flow in some situations. For example, as indicated by Cartwright. (2014), when predicting the water retention curve above watertable fluctuations, Richards' equation matches well with experimental results for low-frequency watertable fluctuations, but it deviates markedly from the measurements for high-frequency watertable fluctuations (Fig. R1). This means that Richards' equation cannot accurately predict the water exchange between the saturated zone and unsaturated zone for high-frequency watertable fluctuations, as already reported by Cartwright. (2014) and Shoushtari et al. (2016, 2017). Specifically, as the fluctuation period decreases from 12.25 h to 75 s, the maximum soil water content measured at z = 1.2 m decreases from the saturated soil water content (0.355) to a smaller value (about 0.22), but the predicted values nearly keep the same (i.e., equal to the saturated soil water content). In this manuscript, we rectified this shortcoming of Richards's equation by introducing the dynamic effective porosity. The comparison between measured and predicted results shows that this method works well (Fig. 7). In addition, Zheng et al. (2022) confirmed the validity of this approach. In their paper, Zheng et al. (2022) used our dynamic effective porosity expression and found that "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". In summary, the "standard" Richards' equation has already been shown not to simulate observed vadose zone pressure head-saturation data. On the other hand, inclusion of the dynamic effective porosity in Richards' equation produces results that do agree with experimental data.

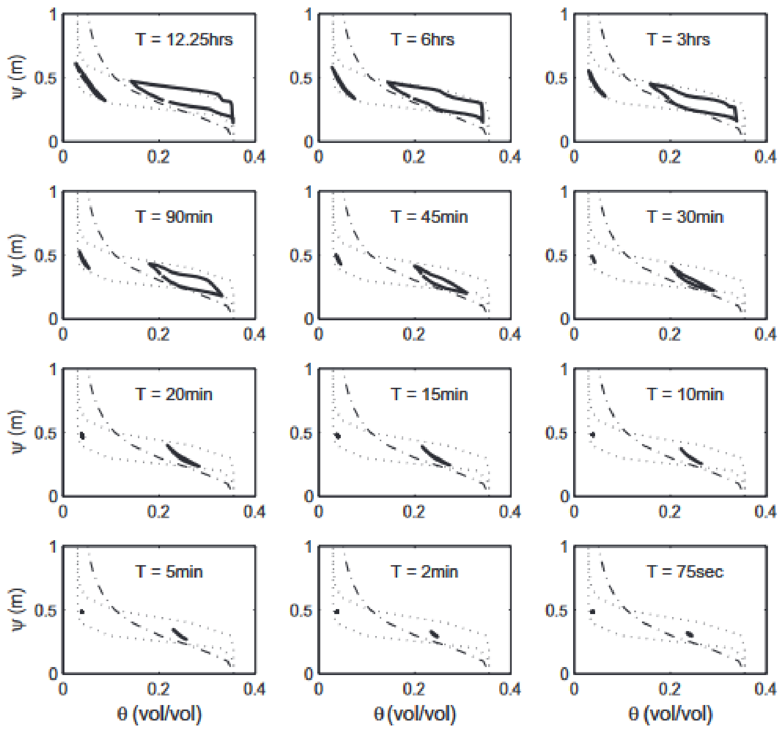

Fig. 2. Observed moisture-pressure relationships (bold solid lines) for a full oscillation period at $z = 1.2$ m and 1.4 m. Each panel corresponds to a different oscillation period as indicated. The dashed lines denote the equilibrium van Genuchten (1980) curves (drying and wetting) based on parameters given in Table 1. The dash–dot curve is an indicative van Genuchten (1980) curve with $\beta = 3$.

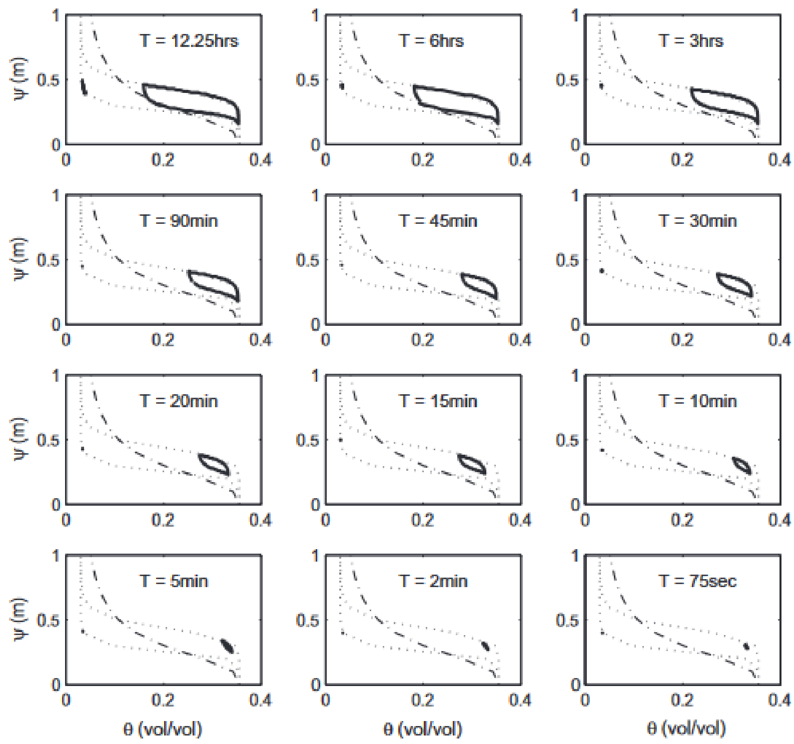

Fig. 3. Simulated moisture-pressure relationships (bold solid lines) at $z = 1.2$ m and 1.4 m. Each panel corresponds to a different oscillation period as indicated. The dashed lines denote the equilibrium van Genuchten (1980) curves (drying and wetting) based on parameters given in Table 1. The dash–dot curve is an indicative van Genuchten (1980) curve with $\beta = 3$. Simulations used the hydraulic parameters given in Table 1 and bottom forcing parameters of $A_0 = 0.15$ m and $d = 0.9$ m.

Fig. R1. Comparison of measured and predicted water retention curves (Cartwright, 2014, Figs. 2, 3). Richards' equation matches well with experimental results for low-frequency watertable fluctuations, but it deviates markedly from experimental results for high-frequency watertable fluctuations.

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