Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
https://doi.org/10.5194/hess-2021-634-RC2, 2022

Comment on hess-2021-634

Anonymous Referee #2

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-RC2, 2022

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.

- 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.

- 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.