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Comment on hess-2022-369

Anonymous Referee #2

Referee comment on "Controls on flood managed aquifer recharge through a heterogeneous vadose zone: hydrologic modeling at a site characterized with surface geophysics" by Zach Perzan et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-369-RC2, 2023

Summary

Perzan et al. manuscript deals with modeling managed aquifer recharge (MAR) under an almond orchard using geophysical data and Monte-Carlo simulations to deal with a relatively deep heterogeneous vadose zone. The manuscript is well written, the figures are excellent, and the scope is well suited for HESS. The workflow of parametrizing the model using geophysical data is thoroughly done, and I like the authors' approach to quantifying MAR recharge quantity and dynamics (comparing simulations with and without MAR). Unfortunately, the authors did not provide any validation of the simulation results by measurement of groundwater level and water content.

To give a wide perspective of their work, I think the authors tried to present this case study as representing a typical flood-MAR operation, that can include both agricultural MAR (Ag-MAR) and dedicated MAR facilities. This is not the case, in my opinion, and I suggest that the authors change the focus of the paper (including the title) to "Ag-MAR" or if they prefer "on-farm flooding". This is not just semantics: dedicated MAR facilities usually have a limited spreading area that overlies high conductivity subsurface layers, while in Ag-MAR the tradeoff is an "unlimited" spreading area that overlies less conductive sub-surface. The submitted manuscript exactly deals with the second case.

I have some concerns with the modeling of the infiltration process, calculation of the root zone residence time (discretization and root zone sizes are similar), and some suggestions for improving the manuscript (see details below). Overall, I recommend minor revisions.

General comments

I strongly suggest revising the Methods section. It is too long - about half of the manuscript. Although very informative, some parts of it should be moved to the supplementary material, mainly parts that were published previously (e.g., Fig. 4 and related text which is a similar approach to Goebel and Knight, 2021).

Please elaborate more on the impact of model discretization on the infiltration process. The vertical discretization of your model is very rough (1 m) for numerical infiltration problems where usually discretization is on the order of 0.05-0.3 m (e.g., Botros et al. 2012). Moreover, this may have a profound impact on your root zone residence time analysis – because your vertical discretization (1 m or more) is the same size as the root zone (1 m). Hence, the root zone in your model is probably only one cell that has no spatial dynamics – at each time step it always has only one value of water content. Please explain if this can impact your analysis (e.g., overestimating root zone residence time).

Specific comments

L76-77: This is the motivation of the study, but it is not convincing, as other studies explore "The influence of meter-scale heterogeneity on recharge.." – I suggest to mention some of these studies and their findings.

L95: The authors chose the term flood-MAR to describe their framework, but it may be too general for their specific agricultural flood-MAR operation. Their choice of 2nd metric of root zone residence time demonstrates this point. I suggest using the term "Ag-MAR" (e.g., Ganot & Dahlke, 2021; Levintal et al. 2022) or "on-farm flooding" (Bachand et al. 2014) instead of flood-MAR for most places in the text.

L111-112: In SAT-MAR anoxic conditions are one of the ways to reduce nitrate loads by denitrification.

L135-137: It is unclear why the authors chose for this work an agricultural field with a SAGBI rating of "poor" (maybe because of the available geophysical data?). This is an agricultural field that is not suitable for Ag-MAR according to the approach of O'Geen et al. (2015) which is much less exhaustive than the approach presented in the current paper. Please explain.

Table 1: Check the residual saturation (# 11 and 12). Values are too high (above common porosity values) – I believe it should be relative residual saturation.

L319-321: Not sure how old is the almond orchard at your site, but during very long spin-up times (>15-25 years) other crops grown at the site with different Kc may have a different impact on water budget and subsurface storage. In other words, it seems like very long spin-up times (such as 131 years) have a little practical benefit.

L335-342: The falling head boundary is elegant, but probably far from real-life water application of a large plot with an area of 800 m x 400 m. This is mainly true for an impractical initial condition with a ponding head of 0.8 m (!). From my experience ponding during on-farm flooding rarely exceeds 10 cm. Unrealistic high ponding conditions may also overestimate infiltration rates. An alternative upper boundary will be to keep a constant head of 5 to 10 cm followed by a falling head (e.g., for a total application of 20 cm, with a 5 cm constant head, let 15 cm infiltrate under a constant head, and then start the falling head loop for the remaining 5 cm).

L375: a suggestion – consider writing eq. (3) directly as the difference between MAR and no-MAR simulations (it is like plugging eq. (3) into eq. (4) and then Q0 and S0 are eliminated). Methodology-wise, it seems a more intuitive way to present the net recharge by MAR (and then you don't need eq. 4).

L438-400: Please explain how you got such high infiltration rates for a field that is rated as "poor" by the SAGBI index. These infiltration rates (even the 0.06 m/hr) will redefine the site's SAGBI rate as "good" or "excellent". Maybe the range of Ks that you used (#1 in Table 1) was too high? You disregarded half of the failed simulations because permeability was too low for the specified upper boundary flux (L432-434). It could be that the specific site cannot accommodate the amount of applied water, and therefore you preferred the converged simulations with high Ks. In other words, it could be that the Monte Carlo approach produced an overestimation of Ks and infiltration rates?

L453: I suggest to rephrase "..accommodate more infiltration.." (maybe to "these cells have higher infiltration capacity..")

L462-464: Interesting result. I suggest emphasizing that your spring flooding is probably a riskier approach, as most growers will prefer to apply winter flooding when almonds (or other perennial crops) are dormant.

L469: Fig. 6 and S8 are very nice. If doable, I suggest incorporating them both as one figure in the main text.

L497-502: The dichotomic division to fine and coarse facies is arbitrary and subjective. Looking at the continuous parameters of the hydraulic functions, it is clear that all outcomes (obj. 1-3) are most sensitive to hydraulic conductivity, porosity, and residual saturation. So why divide the global sensitivity analysis into different facies?

L566-568: I would be cautious with declaring that the 3 outcomes are not sensitive to flooding frequency (especially root zone residence time). I think the source of this conclusion is site-specific – in your case infiltration rates are relatively high compared to the applied water volume. Inundation frequency of several wetting-drying cycles is common practice in MAR operations to maintain high infiltration rates (as you also stated in section 4.2.1). In SAT operations (and probably also in Ag-MAR) these cycles have also an important role in soil re-aeration.

Technical corrections

L59: change Harrington et al. (2014) to (Harrington et al., 2014)

L139: Consider changing the precipitation units to mm (60 mm and 450 mm)

L172: eq (1) – consider changing z to other letters, because you perform 1D vertical interpolation/extrapolation and in the following paragraph z denotes both resistivity [z(u)] and direction [z], which can be confusing.

L319: consider changing "domain" with simulations or realizations (as you have only one model domain).

L426: Fig. S5 – while found in the SM, the axis title in Fig. S5c should be corrected to z(m); and the x=200 in the caption should be corrected to y=200.

L458: Fig. S7 in SM, correct the legend to longer than 48 h (red/orange) and less than 48 h (blue). Also correct caption – "(green)" to "(red)"

Throughout the text – I suggest replacing "pressure" with "head" or "hydraulic head" as you also present it in units of length (e.g., Fig. S8).

L493-495: either use average or median for both facies (currently average use for fine, and median for coarse).

Fig. 10: isn't obj. 1 title "Preferential infil." should be changed to "infiltration rate"?