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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Sensitivity of Hydrologic and Geologic Parameters on Recharge Processes in a Highly-Heterogeneous, Semi-Confined Aquifer System" *by* Stephen R. Maples et al.

Stephen R. Maples et al.

srmap@ucdavis.edu

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GENERAL COMMENTS:

"This manuscript presents an assessment study of the hydrologic and geologic impact on managed aquifer recharge processes. At 100 randomly sampled sites across the model domain the correlation between 17 hydro(geo)logical site characteristics/parameters and simulated recharge "benefits" is evaluated. Overall, upscaled vertical K multiplied with "Water Table Depth" (WTD) produce a good correlation with recharge rates. This proxy parameter (GPP – Kgeom * WTD) are most correlated with recharge rates, validated by local and global sensitivity analysis. Moreover, the anal-

yses also indicate that permeability and unsaturated zone pore volume (porosity used as an indicator for Sy) were relatively more important than other hydraulic parameters.

The study presented is comprehensive, thorough, well-organized, and clear. The conclusions are informative and I do not see any over-statement in the conclusions drawn. I thus think the manuscript should be considered for publication in HESS, although I would suggest that a minor revision is needed to clarify some parts of the manuscript. Maybe the most critical point I see is how to transfer the obtain important information for MAR (interconnected coarse-texture facies paired with water table depth information are crucial for finding suitable recharge sites) to any other field where this information is difficult to acquire. I see your point that GIS-derived indices of recharge suitability rely solely on soil and surface geology to determine geologic suitability for recharge (e.g. Line 354) and the integrated values (up-scaled K + WTD) are more useful. But the question is how we could get the required information without knowing the subsurface in every detail in the whole model domain/study area. Certainly, in your (semi-)synthetic approach we know the parameterization (by the way it is just one field/realization and there remain uncertainties about the distribution, however, for this study and target it is ok I believe but should be note more clearly in the discussion) but how could we use your guidelines where it is not known. I suggest discussing that more to strengthen the manuscript and impact of this interesting study. Comprehensive field tests could be probably the best to better understand the problems in general. However, they are time-consuming and only a limited number of sites are available to accommodate the tests. The numerical analysis, on the other hand, allows us to explore and assess multiple sites relatively easily, yet the validity needs to be carefully checked. So, how can your useful guideline to be considered by practitioners? Another point is related to the WTD. Correct me if I am wrong but my impression based on your manuscript (for instance Fig. 8) is that Kgem * WTD is very useful where the WTD is deep (so large unsaturated zone and thus more storage). Can you split your analysis/results further to see if the depth to the water table matters or not?"

RESPONSE: Thank you for the insightful comment. We agree that the manuscript would be strengthened by including discussion of potential approaches for characterizing the subsurface heterogeneity in real-world situations. We have added an additional paragraph in the Discussion to highlight some emerging geophysical approaches that show promise for characterizing subsurface geologic architecture for MAR (lines 483-492).

To the second point regarding splitting the analysis/results to determine whether the water table is important for recharge, the authors contend that we have discussed these findings in the manuscript (lines 358-360; Fig. 6), where we show that water-table depth alone is a poor predictor of recharge rate.

FURTHER MINOR COMMENTS:

COMMENT: "Line 94: What kind of geological analysis" RESPONSE: We agree that this language was ambiguous. We've have removed the first phrase of the sentence "Through geologic analysis of the data, additional parameters we estimated . . ." to just "Additional parameters were estimated" because the preceding text makes it clear that these parameters are part of the greater geostatistical analysis.

COMMENT: "Section 2.3.3 Model Spin up and Calibration: All sentence related to boundary conditions should be moved to section 2.3.2 Boundary conditions. (line 134-141)" RESPONSE: We agree and have moved the five sentences describing boundary conditions (lines 134-141) to the preceding section (2.3.2) as a new paragraph

COMMENT: "Table 2: Are these parameters the calibrated values?" RESPONSE: Yes, these are the calibrated values. The title of the table has been changed to "Calibrated Hydrofacies Hydraulic Properties."

COMMENT: "Section 2.4.1 Why do you select the sites randomly? I would assume that MAR will be pretty much every time in more coarse sediments. I think, a useful comparison would be to choose the sites based on the surface information (as

you mentioned as the "classical" GIS-approach) and compare the results with results from some randomly chosen sites. You might find additional arguments to criticize the "classical" workflow. I think that would be "just" another post-processing step and no demanding model runs are required." RESPONSE: The intention of gathering 100 random sites from 910 potential sites was to represent the variability of geologic configuration throughout the domain in a computationally-efficient manner. It was not known a-priori which site characteristics would be best correlated with MAR, and our intention was not to presume the 'best' sites from within the domain. We contend that our approach clearly shows the limitations of the "classical workflow" of identifying sites with favorable surficial geology. Our results highlight the limitations of approaches that rely on surficial geology alone (lines473-474 and lines 479-481). The approach suggested in the comment is explored in a companion paper recently published by the authors in Hydrogeology Journal (1)

1. Maples, S. R., Fogg, G. E., and Maxwell, R. M. (2019) Modeling Managed Aquifer Recharge Processes in a Highly Heterogeneous, Semi-Confined Aquifer System, Hydrogeology Journal, doi:10.1007/s10040-019-02033-9.

COMMENT: "Line 201: for all 100 sites" RESPONSE: We agree that "all 100 recharge simulations" was confusing, and changed the text to "all 100 sites."

COMMENT: "Line 230: 6 hydraulic properties and not 8!" RESPONSE: Thank you for catching this mistake. "eight" has been changed to "six" accordingly.

COMMENT: "Line 258: Where are the four representative sites! You could add these sites to figure 3a." RESPONSE: We agree that highlighting these sites in Fig. 3a would be helpful and have modified the figure accordingly.

COMMENT: "Line 286: yes, they are important, but it is not demonstrated here. The results section just comes a few pages later. Please reformulate." RESPONSE: We agree that the results presented in the Methods section are out of place. We have re-phrased this paragraph to not include mention of results, and instead reframe the

introduction of Morris parameters without mention of results (lines 289-290).

COMMENT: "Line 357: Yes, but again how to get this information for a larger study site." RESPONSE: We agree with the Reviewer's point, which is also stated in the General Comments, about the need for applicable field methods to make use of these findings. We have added paragraph of emerging geophysical techniques that could be used to validate findings presented here for real-world sites in the Discussion (lines 483-492).

COMMENT: "Figure 7: Change the 95% confidence lines to dashed lines or change the figure caption." RESPONSE: We contend that the dashed lines are clearly indicated as the 95% contour intervals in figure 7a, but we have added some clarifying language to the figure caption to make this more clear.

COMMENT: "Figure 10: Why is the gravel n so important for V_fines?" RESPONSE: We agree that this is an interesting result, and it is only observed for site q95. We attribute this result to the fact that site q95 has a high proportion of gravel, so it is not unexpected that pore size distribution (n) of gravel would have some influence on the recharge response. We have made a change to the text to acknowledge this result (line 405), but do explore the implications in detail.

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