Review of

Developing Functional Recharge Systems to Repel Saltwater Intrusion via Integrating Physical, Numerical, and Decision-Making Models for Coastal Aquifer Sustainability

By

1. Introduction and overall assessment

The authors assess the improvement caused by adding artificial recharge to an impervious barrier as measures to control seawater intrusion (SWI). To this end, they perform sandbox experiments and numerical methods on a highly idealized case. They use this model to compute a number of ratios that are used to feed an "analytic hierarchy process". They conclude that the combination of physical barrier with artificial recharge is the best option to control SWI.

Overall, the topic is of interest to HESS readers and the conclusion is valid, although somewhat trivial. Since they are not considering construction or operation costs, the more barriers you put (physical and or hydraulic), the better you will control SWI. Furthermore, it is clear that the authors have put a lot of work into this paper. Unfortunately, I believe the paper cannot be published in the present form for several reasons:

1) It is unnecessarily complex and, worse, incomplete. A large number of indicators are defined without a clear reason (no formal dimensional analysis). Yet, the most important parameters (recharge rate) are not defined.
2) The paper contains numerous conceptual errors (not too severe, but unacceptable).
3) It is very poorly written. Worse, it is very poorly organized. I do not just mean the language needs to be improved, but also the logical sequence.

I discuss issues 1) and 2) below and I make a number of writing recommendations in the last section. But the overall recommendation would be to greatly simplify your paper and remove everything that is not related to the main objectives.

2. Conceptual errors:

Introduction

Line 58: It is not true that "Physical and numerical models … reduce the high cost of hydrogeological and environmental investigations". There is no alternative to scu investigations. If you design a SWI control system without a good understanding of you system, you will waste you money (what is the depth?, how do you know you are intercepting the whole SW flux?, how much you need to recharge?

Methods:

I am not sure what you mean by "dimension analysis", but none of the dimensional analyses" I know consist of computing model output ratios. Please, do not anticipate results before describing what you do ("The results of the category (a) model cases reveal the location of the minimal hydraulic heads, which are expected to be the locations of the indicated artificial recharge systems" (and we still do not know what model category (a) is).

Sand tank:
Note that the selected ratios are arbitrary and do not result from a proper "dimensional analysis". That is, appropriate rations would result from writing the problem in dimensionless form, so that they would represent the minimum set of variables for definition of the problem. The current definitions are (1) arbitrary (e.g., IR is defined with respect to a base case that has not been defined yet, perhaps it would be better to define it with respect to the case without any remediation), (2) redundant (NAR and NDR are complementary, except that, to make things worse, BDR is defined in terms of cross sectional area), (3) not really descriptive variables (e.g., the SLR is not a real number, but a function). This is severe, as it invalidates the final results. Worse, the reader is left with the impression that the ratios are improvised. After devoting some length to describing these ratios in section 2.2, the authors introduce new ratios in section 2.3. Worse, the numbering of the new ratios is inadequate.

Recharge through wells is done when you have an aquitard. Furthermore, wells generate a radial flow around the well. The setting (both the sandbox and the model) are essentially 2D, so that including wells is not appropriate.

Further comments:

Line 132: "The filling process is done in layers of 5cm each, with a falling height of 50 cm for each layer, to ensure a homogeneous hydrogeological property of the media sand". Actually this procedure may lead to stratification (it depends on the filling rate) with the coarsest material at depth. While this may be adequate (it is similar to what occurs in nature) it does not ensure "a homogeneous … sand".

Densities of 0.99 and 1.022 kg/L (not m3/s) are low and high for FW and SW, respectively. Adding 0.15 g/L concentration of green food dye will increase the density of SW.

Line 149: You are not verifying saturation, hydraulic heads will equilibrate even if air bubbles are trapped.

Figure 4 can be dropped. It is not necessary.

In Figure 5, name only variables that you are going to vary (b is fixed, isn't it?). Also you may add the location of recharge wells here, so that you can eliminate Figure 6.

Tables 1-3 can be blended into 1 (if at all). Table 3 is particularly unfriendly s it forces the reader to check that all cases are identical. It took me a while to realize that you were just varying the barrier depth. In fact, you may just say that you did several runs with varying barrier depth and make them a function of barrier depth.

Please, eliminate Table 5 (textbook and irrelevant for your work)

Numerical Model
The model is not properly described. Beyond formalities, this is severe because the results suggest that boundary conditions in the model /never described) are different to those in the sand tank.

AHP technique

The description is too sketchy (I had to read independently to understand it). While, in my understanding, the AHP technique is not appropriate here. It is generally used for multiple criteria that are hard too subjective for quantitative comparison, so that the weights assigned to each criteria are derived from pair-wise comparisons by "experts".

Results

Model calibration is unclear. For one thing, the authors report a RMSE without having described what errors are being minimized. The parameters results are unrealistic (Table 6 contains no units, I assume that k is m/s, but a porosity of 0.04, a specific storage of 0.0619 1/m?, and a S_Y of 0.04 are clearly unrealistic). I do not think that these errors affect results severely, but it conveys a poor image of the model. Probably more severe are the apparent instabilities or the fact that boundary conditions look different. IT is also unfair to the reader to find here, for the first time, that model construction is done under transient conditions, which leaves the reader wondering how are the indices described in Section 2 computed.

Worse, the flat region of the salt interface (between 30 and 40 cm inland) in the experiment suggests that permeability is higher in this region (probably a consequence of the handling of the barrier). This flat region was reproduced in the numerical model, but nothing is said about heterogeneity.

The graphs do not appear to make much sense. It is not clear to me what hydraulic grade line is. But if I interpret it to mean head, the paths are inconsistent. You probably do not need the paths.

Results of each case are hard to read because of the abuse and redundancy of indicators and inconsistencies. For example, IR had been defined the ratio of observed intrusion length at a time (t) to maximum saltwater intrusion length (base case). Yet, for the base case IR is found to be 0.97.

I could not follow the last part (new items are introduced and I must confessed that I was exhausted of going back and forth to recall the meanings of all the abbreviations).

Editorial comments and writing

The paper is very poorly written. I am referring not only to the traditional "look for a native speaking person" (being a non-native myself, I hate when I am told it), but also to the logic. The paper is complex and long, and the writing does not help. Try to simplify it. I understand that you are under pressure to publish quickly, but, please, facilitate the lives of readers by providing an easy to read paper (in the end, it will favor citations). Numerous statements call for a more refined argumentation. I list a few below:

In the abstract: "Three countermeasure combinations, including vertical barrier, surface, and subsurface recharges, are numerically investigated using three model case categories. Category (a) model cases investigate the hydraulic head's variation along the aquifer to determine the best recharge location. Under categories (b) and (c), the effects of surface and subsurface recharges are studied separately or in conjunction with a vertical barrier". Perhaps it is sufficient to say "The numerical model is used to investigate the SWI control efficiency of vertical barrier, and optimally located surface and subsurface artificial recharge".

Also, in the abstract, try to minimize the use of acronyms (OK in the body of the paper) and too many numbers (they tend to hide, rather that highlight). For example, it is very hard to read: "An analytic hierarchy process (AHP) as DMM is built using the classification ratios of hydraulic head (HHR), salt line (SLR), intrusion (IR), repulsion (Rr), wedge area (WAR), and recharge (RER) as selection criteria to select the overall best model case. The optimal recharging location, according to the results, is in the length ratio (LR) range from 0.45 to 0.55. Furthermore, the DMM supports case3b (vertical barrier + surface recharge) as the best model case to use, with a support percentage of 47.93%, implying that this case has a good numerical model classification with a minimum IR of 67.9%, a maximum Rr of 29.4%, and an acceptable WAR of 1.25". Instead, you may just say: "An analytic hierarchy process is built to compare SWI control strategies on the basis of head, salt line, intrusion, repulsion, wedge area and recharge. We find that best results are obtained by combining a vertical barrier with surface recharge at a distance from the coast comparable to the thickness (here you have a problem, your LR is a model dependent variable, you must relate it to generic aquifer variables".

The introduction is very poorly structured… You start by saying that SWI is a relevant problem, continue with methods to control SWI (but do not mention artificial recharge, which many, including me consider the best control method, as its efficiency is greater than 1, see Abarca et al., WRR, 2006). Then you introduce artificial recharge to conclude that "Although many studies investigate saltwater intrusion in coastal aquifers, only a limited number study the control methods of saltwater intrusion"! This is inconsistent (and false!). I believe that the logical sequence of the introduction needs to be revised.

Line 46: "Artificial recharge techniques, such as surface and subsurface recharge systems, are critical for establishing hydraulic barriers and mitigating the effects of saltwater intrusion". They are not critical… Instead "Artificial recharge techniques can be used for establishing hydraulic barriers and mitigating saltwater intrusion, while recovering SGD".

"These techniques have several advantages compared to traditional methods, including low cost…" what traditional techniques do you refer to?

"Although many studies investigate saltwater intrusion in coastal aquifers, only a limited number study the control methods of saltwater intrusion". Indeed, you should cite some of them

"Although physical and numerical models are effective economic tools for selecting the best solutions for repelling saltwater intrusion, deficiencies in the acquisition of appropriate evidence to support the final decision are discovered". What do you mean by discovered… Perhaps you should indicate some of these deficiencies.

The objective statement should be short and to the point. Instead, you list seven goals, which are really methodological steps.

Look for appropriate references (this may also help you to simplify the writing by leading the reader to other papers for details). For example, "MODFLOW-2005, in conjunction with the SWI2 package, is used in this study for numerical modeling of saltwater intrusion. SWI2 is a software package used to analyze three-dimensional groundwater flow, model saltwater intrusion, and calculate hydraulic heads. The main advantage of using the SWI2 package is that it requires fewer

cells for the simulation process than variable-density groundwater flow packages like SEAWAT. The ability of SWI2 to represent each aquifer as a single layer of cells results in significant model run-time savings". I am not sure this choice is appropriate, but please provide references for all the codes.

Also, provide a reference for "HM 169 GUNT HAMBURG"

There are numerous terms that you must revise (you do not "repel seawater intrusion", you control it, or minimize it)