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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Groundwater withdrawal in randomly heterogeneous coastal aquifers" *by* Martina Siena and Monica Riva

Anonymous Referee #1

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In general, the study of heterogeneity effects on SWI is a worthwhile endeavour. Unfortunately, the investigation results are not generalizable, and where conclusions are made, they are all conclusions found in other, previous articles.

Specific comments: Page 1 Abstract: L8: "Mediterranean" isn't needed because "worldwide" includes the Mediterranean. Abstract generally: There are not new findings presented in the Abstract. The title reflects a generic investigation, whereas the Abstract describes a more site-specific investigation, but regardless, there is nothing that is new in the Abstract, because seawater pumping to reduce SWI has already been studied, as has the effect of heterogeneities on SWI. There needs to be clear guidance in the Abstract as to what is an advance on the existing body of scientific knowledge regarding this topic. Also, the Abstract reads as though a single well has been used in

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Discussion paper

studying SWI. This would be an extremely rare situation – i.e. a single well pumping. It is more likely that there are many wells being used within a coastal aquifer. The limitations of using only a single well to study SWI need to be considered. Introduction: L22: “worldwide” can be removed without losing any meaning. L22: Grammar problem – suggest “threatened by seawater intrusion (SWI), which can” L24-25: The phrase “civil purpose” is not clear. Please use a phrase that is clearer. L25: “Highly critical scenarios are associated” is awkward. Suggest something like “Critical SWI thresholds are reached when seawater reaches extraction wells. . .” L27: “Mas Pla” is not spelt in the same way in the references list. L29: “subordinated to” is an odd phrase to use here. “dependent on” is more accessible to the readership and clearer. Page 2 L6-7: Commas used inconsistently in the formatting of citations. Also at L16 and elsewhere in the ms. L25: There is a disjoint in the flow of this paragraph. The sentence describing Abarca et al.’s (2007) work does not follow logically from the previous sentences. L27: “rely” should be “relied” to be consistent in the use of past tense in previous sentences. Page 3 L4-13: The list of examples of field-scale SWI studies does include pivotal cases. For example, Dougeris and Zissis (2014) is a synthetic case that considers steady-state schemes, so it is hardly worth mentioning. Narayan et al. (2007) is a 2D model of a very idealised version of the field scale problem. On the other hand, Dausman and Langevin (2005; Movement of the Saltwater Interface in the Surficial Aquifer System in Response to Hydrologic Stresses and Water-Management Practices, Broward County, Florida: U.S. Geological Survey Scientific Investigations Report 2004-5256, 73 p.) and Werner and Gallagher (2006; Regional-scale, fully coupled modelling of stream-aquifer interaction in a tropical catchment, Journal of Hydrology 328: 497-510) provided early examples of comprehensive field-scale, three-dimensional SWI modelling. L18: Correct to “considering variable-density flow” L21: Correct to “spatial patterns of salt” L24: The statement about “. . .uncertainty in the displacement. . .” needs more information. What sort of uncertainty is this exactly – related to the lack of knowledge of heterogeneities or other aquifer properties? It isn’t clear. L26: I don’t understand what is meant by “average concentration fields”, to the degree that I can’t

offer possible interpretations or alternative wording. Page 4 L2: “and” needed before “(iii) reducing. . .” L12-14: Recommending deleting this last paragraph –it is not needed for journal articles. L18: “river” should be “River”. Same at L22, L27 and elsewhere. L21: Correct to: “is mainly composed of a” Page 5 Section 2.1 generally: The area 2.5 km by 750 m is a small region. Why was this particular region chosen? L7: Where is states that the underlying clay acts as an impermeable barrier, is this saying that a clay sequence is presumed to represent the base of the model domain? It should be clearer. L8: “embedded” is the wrong word here. “using” or “based on” would be better. L10: “fluids” should be “fluid” L10 onwards: I won’t correct any English issues from this point, but it should be pointed out that these are numerous in the remainder of the manuscript. Page 6 L1: Please provide the units for Dm L6: There is no need to redefine variables that are already defined. L8: Use a comma in “101,632” L13: The choice of longitudinal dispersivity (aL) is critical. Because the model is heterogeneous, then aL should be smaller – it otherwise seems a little on the high side. Also, the vertical aL should be smaller than the horizontal aL, otherwise, solutes move between layers too easily (i.e. given that deltaic sediments are usually layered, thereby providing more resistance to flow and transport in the vertical than in the horizontal direction). L16: The use of no-flow boundaries is concerning. Topographical divides are unlikely to be no flow boundaries at this small scale. Perhaps the no flow boundaries running perpendicular to the coast are presumed to follow flow lines, rather than topographical divides. L17: The lack of offshore extension of the coastal aquifer should be mentioned as an area of possible error. L18: I though that the inland boundary was no flow, on the basis of the previous sentences, but now it reads as though the inland boundary is a specified flux boundary. The earlier text should be clearer about which boundaries are specified as no flow boundaries. Page 7 Generally: The initial conditions are not given or explained. The time-stepping is not explained. The approach to transience is not explained. The approach to setting pumping is not explained. Page 8 Generally: The variability that has been obtained across the various realisations is entirely dependent on the assumptions about the heterogeneous K field. If different geostatistical proper-

ties were adopted, then the outcomes would be different. How can the reader connect the variability should here (i.e., in the extent of seawater) to reality? Generally – I must have missed where it states at what time the results are given – at the end of 8 years? L18-28: This is methodology and belongs in the Material and Methods section, not in the results. P8-9: I am unable to find any new outcomes, beyond those obtained from previous research, from Section 3.1. P9-10: The scenario here for pumping should have been given in the Methods section. Also, the scenario is very site specific, so it is not clear how generalizable findings can be drawn from it. P12-13, Conclusions: The conclusions don't need to restate the methodology. This is more so done in the Abstract. Regarding the conclusion points: (1) This was already known and should not be a conclusion from this research. Of course heterogeneity influences seawater extent. Also, the rotation effect was expected on the basis of previous studies. (3) I don't understand the advice given about average concentration fields. I don't know anyone who is doing this. Also, the advice given here is stated as though it can be considered generic, but it is entirely dependent on the geostatistical parameters and the field-scale case study that form the basis of the analysis. (4) All of this advice on pumping is known from previous studies, but is stated here as though it is being advised for the first time. A proper recognition of the knowledge contained in previous studies is needed to avoid giving the wrong impression that the current study was the first to make such conclusions. The references need attention – so that consistent formatting is achieved.

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