October, 7 2017

Re: Response to the review of Anonymous Referee #1 of the manuscript "Groundwater withdrawal in randomly heterogeneous coastal aquifers" by Martina Siena and Monica Riva.

We are grateful to Anonymous Referee #1 for his/her careful and detailed review of our manuscript. Following, is an itemized list of his/her comments (in italic) and our responses.

1. *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.*

We agree with the Reviewer on the observation that in the literature there is a quite vast (and not always original) number of contributions dealing with SWI in homogeneous and heterogeneous systems. Roughly speaking, among about 2,000 ISI papers concerning SWI have been published from the '80. Almost 100 of these considered heterogeneous systems, while only 20 included the presence of pumping wells. A quite recent review on SWI phenomena has been offered by Werner et al. (2013).

We do remark that most of the aforementioned contributions (a) deal with deterministic approaches, where the attributes of the system (e.g., permeability) are known (or determined via an inverse modeling procedure), so that (b) the impact of uncertainty of system attributes on target environmental (or engineering) performance metrics is not truly considered. This is in stark contrast with the widely documented and recognized issue that a complete knowledge of aquifer properties is not possible. This is due to a number of reasons, including data availability, i.e., available data are most often too scarce or too sparse to yield an accurate depiction of the subsurface system in all of its relevant details (e.g., Rubin, 2003). In this context, the inherent uncertainty associated with aquifer systems must be considered, this objective being achieved framing our analyses within a stochastic approach. The latter enables us not only to provide predictions of an output quantity of interest, but also to quantify the uncertainty associated with such predictions, to be used (for example) in environmental risk assessment and probabilistic management and protection protocols for water resources. Our work is precisely set in this framework.

As we mentioned above, only a few contributions studying SWI within a stochastic framework have been published to date (less than 20 ISI-ranked papers). Amongst these, the studies most relevant to our work have been listed in the Introduction of the manuscript. It has to be noted that the vast majority of these works consider idealized synthetic showcases and/or simplified systems (typically in a two-dimensional context) and/or simple flow conditions (usually steady state mean uniform flow). To the best of our knowledge, there are only two contributions where a probabilistic approach is employed to analyze the transient behavior of a real (three-dimensional) costal aquifer: (*a*) Lecca and Cau (2009), and (*b*) Kerrou et al. (2013), respectively targeting the Oristano (Italy) and the Korba aquifer (Tunisia). In Lecca and Cau (2009), the aquifer system is modeled by considering a (homogeneous) shallow phreatic unit and a (homogeneous) deeper unit, confined by an aquitard characterized by stochastically-varying hydraulic conductivity and variable thickness. The production of freshwater is simulated at locations in the model corresponding to the position where exploitation wells operate in the field. Kerrou et al. (2013) analyze the effects of uncertainty in permeability and distribution of pumping rates on SWI. In both studies, SWI phenomena are analyzed in terms of iso-probability maps corresponding to a target concentration (equal to 0.1). Kerrou et al. (2013) evaluate the regions delimited by the 0.05 and 0.95 iso-probability lines.

In the context illustrated above, considering the very limited number of stochastic studies, we are convinced that our work is markedly relevant to show the way stochastic approaches can find their place in the assessment of real settings.

Key elements of novelty in our work include the introduction and the detailed analysis of an original set of metrics, aimed at characterizing quantitatively the effects of heterogeneity on the extent of seawater wedge penetration and of the seawater/freshwater mixing zone. These metrics yield a quantitative depiction of SWI in a global sense across a three-dimensional system (not only at the bottom of the aquifer and/or along the vertical direction, as is usually done in the literature). Additionally, the effects of pumping on SWI are investigated by comparing three diverse withdrawal scenarios. These are designed by varying the distance of the wellbore from the coastline and from the freshwater-saltwater mixing zone. While the effectiveness of simultaneous pumping of freshwater and seawater to reduce SWI has been already investigated in the literature (e.g., Aliewi et al., 2001, Pool and Carrera, 2009, and Saravanan et al., 2014; as we clearly state in Section 1 of our manuscript), it has to be noted that in this work we evaluate for the first time the effectiveness of the so-called "negative barriers" in limiting intrusion within a randomly heterogeneous aquifer.

We will further stress the aspects of novelty of our contribution in the revised manuscript.

2. *The Abstract reads as though a single well has been used in 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.*

The hypotheses and limitations of our work will be further clarified in the revised manuscript. Considering our replies to item 1 of the Reviewer, we do think that the joint interaction of more than one pumping well and their effect of SWI, albeit of interest, is beyond the scope of the present work and could constitute by itself the topic of a future study.

3. *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.

We thank the Reviewer for pointing out the aforementioned typos and misspellings. We will fix them in the revised manuscript.

4. *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 andWater-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.*

We thank the Reviewer for the references suggested. We remark that these studies do not consider stochastic heterogeneity, which is the main driver of our work. We also note that the work of Werner et al. (2006), mentioned by the Reviewer, does not include density effects. Conversely, the work of Werner and Gallagher (2006; Characterisation of sea-water intrusion in the Pioneer Valley, Australia using hydrochemistry and three-dimensional numerical modelling, Hydrogeology Journal, 14: 1452-1469) will be referenced in our revised manuscript in the context of field-scale deterministic SWI studies.

5. *L18: Correct to "considering variable-density flow" L21: Correct to "spatial patterns of salt".*

We thank the Reviewer for pointing out these typos and misspellings. We will fix them in the revised manuscript.

6. *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.*

The ensemble average (or mean) concentration field is evaluated by averaging solute concentration across the total number of Monte Carlo realizations. It is a function of space and time. The uncertainty associated with the system behaviour stems from the random nature of the permeability field. We will make this point unambiguously clear in the revised manuscript.

7. *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"*

We apologize and we will correct those typos. Lines 12-14 describe the organization of the manuscript. Such a description is included in many papers, across a variety of Journals (including HESS). We will abide by the Editor's decision on this issue.

8. *Page 5 Section 2.1 generally: The area 2.5 km by 750 m is a small region. Why was this particular region chosen?*

As we mention in the manuscript, we started by considering the two-dimensional constant-density model developed by Rodriguez Fernandez (2015) over the whole river basin (see Fig. 1a of the manuscript). First, we developed a three-dimensional (variable-density) model on an area of 2.5 km (along the coast) by 2 km (inland). A series of preliminary numerical simulations were performed considering a homogenous domain as well as analyzing a limited number of random realizations. Results indicated that values of salt concentration at the end of the 8-year period were appreciable only in a narrow (less than 400m-wide) region close to the coast. On the basis of such preliminary runs, we designed the size of the study area analyzed in the manuscript. This choice has the additional advantage of enabling us to set up a refined computational grid towards the sea boundary (where density-driven effects are relevant) while keeping an affordable computational cost. Note that, the selected model requires about 3 hours of CPU time on a single i7-3930K Intel core provided with 32GB memory for each MC simulation. We will add some details about this issue in the revised manuscript.

9. *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.*

Yes, it is correct. We will make this point unambiguous in the revised manuscript.

10. *L8: "embedded" is the wrong word here. "using" or "based on" would be better. L10: "fluids" should be "fluid".*

We thank the Reviewer for pointing out the aforementioned typos and misspellings.

11. *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).*

According to Eq. (4), the units of D_m are the same as those of $\boldsymbol{D}$, introduced at line 23, page 5. As discussed in the manuscript (page 6 lines 12-13), the value of longitudinal dispersivity, α_L, has been chosen such as $\Lambda \leq 4\alpha_L$, Λ being the element length measured along the direction of flow (Voss, 1984), to ensure stability. The adoption of smaller α_L would led to unfeasible computational times. For the purpose of our simulations, transverse horizontal and vertical dispersivity values have been set one order of magnitude smaller than the longitudinal dispersivity, i.e., $\alpha_T = 0.5$ m in our work, as it is commonly assumed in the literature (e.g., Cobaner et al., 2012; Koussis et al., 2002; Narayan et al., 2007).

12. *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.*

We agree with the Reviewer in that topographic divides and groundwater divides may not coincide. We started by identifying lateral boundaries as groundwater divides consistent with the previous two-dimensional model that was set up for the area (Rodriguez Fernandez, 2015). A close inspection of Figs. 1b and 3 reveals that these no-flow boundaries are indeed (approximately) perpendicular to the coastline in the portion of the domain here considered. We will clarify this point in the revised manuscript. We will also mention that the offshore extension is neglected in the current investigation.

13. *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.*

The inland boundary condition set in our model is defined for the first time at the point noted by the Reviewer. We do think this to be the appropriate place to specify all inputs (including boundary conditions) of our model.

14. *Page 7. 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.*

We set $h = 0$ as initial condition. Adopting $h = 2.4$ m (equal to the mean value of h set along the inland boundary) did not lead to significantly diverse results at the end of the 8-year time period in the homogeneous system. As it is commonly done in the literature, (e.g., Bear et al., 2001; Koussis et al., 2002; Jakovovic et al., 2016) we set initially $C = C_F = 0$. A uniform time step $\Delta t = 1$ day has been set during the 8-year run. A higher time resolution was required for the subsequent 8-month period, due to the activation of pumping. We then set $\Delta t = 30$ min for the first month and progressively increased the time step, up to $\Delta t = 120$ min, as the system showed smoother variations while approaching steady-state. Pumping is implemented by setting a flux-type condition in all cells included in the well-screens. The total rate Q extracted is uniformly distributed across the numerical blocks associated with the screen.

15. *Page 8 The variability that has been obtained across the various realisations is entirely dependent on the assumptions about the heterogeneous K field. If different geostatistical properties 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?*

The adoption of diverse geostatistical models (e.g., non Gaussian distribution of k, conceptualization of the system as a composite medium, or others) would probably lead to different results. Albeit of interests, the analysis of the effects of diverse geostatistical models on SWI metrics is outside the scope of the current contribution. With reference to the type of random heterogeneity analyzed, we note that spherical variograms have been employed to describe a variety of field settings.

The variogram sill we consider represents a domain with moderate variability. As we state in the manuscript, the value of the correlation scale has been selected consistent with documented analyses according to which the integral scale of log conductivity and transmissivity values inferred worldwide using traditional (such as exponential and spherical) variograms tends to increase with the length scale of the sampling window at a rate of about 1/10 (Gelhar, 1993; Neuman et al., 2008). We remark that ours is one of the first attempts at including the effect of random heterogeneity within a three-dimensional, transient density-variable system (see also our answer to item 1). We concur that a systematic analysis of the influence of variogram shape and variogram parameter values would be of interest and will be the subject of a future study.

16. *Page 8 L18-28: This is methodology and belongs in the Material and Methods section, not in the results.*

We will move this part to the Material and Methods section in the revised manuscript.

17. *P8-9: I am unable to find any new outcomes, beyond those obtained from previous research, from Section 3.1.*

Please see our answer to item 1.

18. *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.*

We will move this part to the Material and Methods section in the revised manuscript. We investigate the effects of pumping on SWI by comparing three diverse withdrawal scenarios, designed by varying the screen location along the vertical direction and the distance of the wellbore from the coastline and from the freshwater-saltwater mixing zone. In this work we evaluate the effectiveness of each pumping scenario within a randomly heterogeneous system in terms of local and global metrics describing the extent of seawater intrusion and salt mass fraction at the freshwater well (see also our answer to item 1). We are aware that our study does not cover the totality of feasible combinations of pumping scenarios and heterogeneity. As already remarked, this is the first attempt to include the effect of (*i*) random heterogeneity and (*ii*) simultaneous extraction of freshwater and seawater within a three-dimensional (variable-density) realistic system.

19. *P12-13, Conclusions: (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.*

We partially agree with the Reviewer's comment. Heterogeneity effects have been already observed in deterministic models, or in stochastic analyses invoking ergodicity assumption. We will revise the conclusions highlighting the novel elements of our study, as we detail in our reply to item 1.

20. (*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.

As we mentioned in Section 1 of the manuscript (page 3, line 23), in several works (e.g., Rivest et al., 2012; Lu et al., 2016) concentration values at unsampled locations are obtained by applying a kriging procedure on the basis of available concentration data. Since Kriging is an estimation method, it provides an estimate of the concentration. Therefore, the *kriged* value of concentration should be regarded as the mean (or average) value of an otherwise random concentration. For this reason, we state that the authors mentioned above used "average concentration fields" and such fields should be compared against our "Ensemble-averaged concentration" field (see also our answer to item 6 for the definition of ensemble-averaged concentration). Possible limitations of our study, in terms of the geostatistical model and associated parameter values adopted, will be further stressed in the revised manuscript.

21. (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.

Some of these conclusions were already associated with previous deterministic (homogeneous and heterogeneous) models. Here, we are pointing out that these are indeed valid within a stochastic framework. We will revise this conclusion to stress this important issue.

22. The references need attention so that consistent formatting is achieved.

Many thanks. We will update the format of the references in the revised manuscript.

<u>References (note that only the references not already included in the manuscript are listed)</u>

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