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Hydrology and Earth System Sciences Discussions

Interactive comment on "Assessing impacts of climate change, sea level rise, and drainage canals on saltwater intrusion to coastal aquifer" *by* P. Rasmussen et al.

Anonymous Referee #2

Received and published: 27 August 2012

Scientific Significance: The paper does a good job at presenting the reason and validity for undergoing the study. How seawater intrusion will affect aquifers under future climate change scenarios is of course important.

Scientific Quality: The authors use data observations and measurements and apply them in a groundwater model. The results and conclusions are presented and discussed appropriately. However, I have a couple of concerns with regard to the model.

1. The boundary conditions, given on line 19-20 p. 7983, state that "a constant head in the uppermost model layer representing the sea." I find this a little vague as to what

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the boundary conditions used were. Was sea level used as the boundary condition over the entire model, even the land area several meters above sea level? Or was this the starting conditions, where recharge into the model allows the top layer aquifer to increase to above sea-level, and just the model area underneath the sea was kept at sea level? This should be clarified.

2. Also with respect to the boundary conditions, did the constant head with respect to the sea take into account the density difference between seawater and freshwater? This is in reference to Henry's problem, which is important for flow regimes in coastal areas. Because of the higher density of the saltwater, a higher effective head is observed with respect to freshwater in confined aquifers. As the fractured chalk aquifer is a confined aquifer, this head difference will have an impact on the seawater intrusion in the aquifer.

3. With respect to the calibrated model (lines 13-21, p. 7985), the authors state that the model was manually calibrated until the results were considered "good enough". However, is a model calibration with a RMSE of 1.53m when the heads have only a total range of a little over 7m really good enough? In fact, when you look at the calibration results in Fig. 9c, it seems that a couple of the wells are as much as 4m from their predicted values. In addition, the model underestimates the head on all observations over approximately 0.2m. With such a high head difference in the calibration of the model, with no validation used, one could question whether or not the model scenarios run are really valid. I believe the authors should provide a better model calibration, or a better argument for why they consider the model calibrated.

I am wondering if the high RMSE value is in part due to an averaging of the pumping rates across all of the production wells, when in reality, the abstraction probably varied significantly from well to well? Does the water works not have a record of the amounts pumped from each well? The calibration may also be made better if a "seawater" head was used in the model (if it hasn't already been used). Of course the problems calibrating the model could also be due to variability in the hydraulic conductivity of

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particularly the fractured chalk – which is often seen to high spatial variation in K values. This could be discussed more in the paper.

4. I am not sure I agree with the authors' conclusion that the SkyTEM compares well with the model (Line 28 on page 7988; Fig. 7). To me there is a significant difference, with the location and depth of the fresh water. The authors attempt to explain this difference in the discussion (p. 7994 Line 21-23), but in my opinion do not full succeed. They did not mention at all that the TEM shows seawater at the surface east of the drainage, where the model shows freshwater. This could, of course be a problem with the TEM survey, but the authors should acknowledge this difference and explain the possible reasons for the discrepancy.

Presentation Quality: In general, the presentation quality of the article was very good. There were a few typological errors, and the axis scales on the figures, specifically Figure 9, could be done in a more logical manner.

1. Does the paper address relevant scientific questions within the scope of HESS? YES

2. Does the paper present novel concepts, ideas, tools, or data? Paritally - not completely novel, but important none the less

3. Are substantial conclusions reached? Yes – based upon the model used in the study.

4. Are the scientific methods and assumptions valid and clearly outlined? Not entirely – I found questions with regard to the boundary conditions and model calibration (as stated before).

5. Are the results sufficient to support the interpretations and conclusions? Yes, if the model calibration is valid. But without a valid calibration, the results can be questioned.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? No – I think a

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better description of the boundary conditions is needed.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? YES

8. Does the title clearly reflect the contents of the paper? YES

9. Does the abstract provide a concise and complete summary? YES

10. Is the overall presentation well structured and clear? YES

11. Is the language fluent and precise? YES

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? YES

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? No, the length of the paper is good.

14. Are the number and quality of references appropriate? YES

15. Is the amount and quality of supplementary material appropriate? N/A

Additional comments:

The figures should be numbered according to the order that they appear in the text. Figure 9 should actually come before Figures 6, 7 and 8.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 7969, 2012.