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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.

P. Rasmussen et al.

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Received and published: 2 November 2012

We thank Referee #2 for the careful review and constructive comments. Please find our reply (author comments, AC) below each of the reviewer comment (RC) in the following (in the attachment the ACs are provided in Italics):

RC: 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. RC: 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. RC: 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 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.

AC: Constant head boundary in only specified in cells representing the sea and only in model layer 1. Recharge was allowed in all other cells except from cells representing the sea and the drainage canals. Thus the groundwater table was allowed to move freely in the upper layer in cells not representing the sea or the drainage canal. This will be clarified and described in more detail in the revised paper.

RC: 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.

AC: When using the Time-Variant Specified-Head (CDH) package for the density modeling the reference head value assigned to the boundary cell is updated prior to each transport timestep using the fluid density from the previous transport timestep (line 2-4 p 7980). By doing so the head value used for each transport timestep is referenced to the calculated density value at that cell (p. 13, Langevin et al., 2007). SEAWAT was tested on the Henry problem and could accurately reproduce the problem solutions (Guo, Weixing, and Langevin, C.D., 2002. User's guide to SEAWAT: A computer program for simulation of three-dimensional variable-density ground-water flow: U.S. Geological Survey Open-File Report 01-434, 79 p.).

RC: 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.

AC: We agree that the evaluation of the calibration result is to some degree subjective. However, an RMSE value of 1.53 m is found satisfactorily for the following reasons: (1) The defined geological units are assumed to be homogeneous which indeed is not the case in reality. The hydraulic parameters of each of the geological units are expected to exhibit heterogeneity to a certain degree (which cannot be quantified based on the available data). However, especially the fractured chalk aquifers are expected to exhibit strong spatial variability in hydraulic conductivity. Hence, even though the overall flow field is described acceptable by the model, the prediction of hydraulic head locally at the wells will not match the observed data precisely. (2) Data on groundwater abstraction is only available at the level of waterworks. Hence, no information on the abstraction from the individual abstraction wells is available and therefore the total abstraction has been divided evenly on the wells. This translates into a significant uncertainty on the local groundwater abstraction which in turn affects the ability of the model to accurately describe the local groundwater heads. (3) The observations of hydraulic head used for calibration are collected over a period of more than 10 years and may be affected by transient effects. Some data are measured during winter time where the hydraulic

head is relatively high while others are measured during summer season where heads are relatively low. In conclusion we would not expect, with the uncertainties described above in mind, that the match of the model to the data could be improved significantly by further calibration.

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

AC: We agree that the averaging of the pumping rates may affect the RMSE value of the model, see answer above. Unfortunately, the waterworks were not able to provide information on the pumping rates from the individual wells. As discussed above (RC: 2) the head of sea water was used including density effects. The used groundwater heads for model calibration were all measured in boreholes with low salinity and therefore small density effect. With respect to variability of the hydraulic conductivity of the chalk, please see discussion above.

RC: 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.

AC: Note that we do not write that the model compare well with SkyTEM in line 28 p7988 as reviewer#2 indicate. We write that the model results compare well with the well log (both methods estimate that the 250 mg/l drinking water standard is exceed below a depth of 25 m). In contrast we actually write in the next sentence on page 7889 that the interface is estimated to be located significantly deeper at approximately 40 meter below surface by the SkyTEM method. The same is written again on 7994 as also mentioned by the reviewer, where we just write that simulation and SkyTEM results show a reasonable general agreement (line 6-9) considering that the model was not calibrated based on SkyTEM measurements. We still find this statement is justified and reasonable. However, we have clarified the descriptions on page 7988 and 7994 further to avoid any misunderstandings. In addition we have included the section below describing and discussing the discrepancy between model and SKyTEM results in the revised manuscript. Reviewer #2 correctly note that there is a problem to the east of the drainage canal (approximately 1.5 – 2.5 km east of the deep investigation well), we have discussed that in the following new section. ..”At a distance of approximately 1.5 – 2.5 km east of the well 242.344 (see the two figures of the left panel in figure 7) a discrepancy occurs between the apparent salinity of groundwater as modelled by the groundwater model (upper figure) and the “salinity” measured by AEM/SkyTEM (lower figure). SkyTEM measurements indicate salt groundwater, while the model indicates fresh groundwater. We believe based on the data from the water supply wells about 1 km northwest of the cross section, that groundwater in the upper 25 m is fresh and hence that there is a problem with the SkyTEM interpretation of the groundwater salinity of the upper Chalk in this area. Marine Holocene sediments which may be only partly flushed, occur locally in the area and these may in some cases result in wrong interpretations for the upper chalk just below these. However, we cannot rule out that the Holocene marine sediments, are actually affecting the chloride concentrations in the upper part of the Chalk locally. Further investigations in this area are required to resolve this issue. The examples demonstrate that the combined use of geophysical (geoelectrical) data and model simulations will help to increase the understanding of a

specific setting (Carrera et al., 2010) and to identify where additional data are needed to improve the performance of the model and the inversion and interpretation of the SkyTEM measurements.” Since the submittal of the paper we’ve received chemical chloride analyses from the deep well with geophysical logs shown in Figure 8, these analyses are in perfect agreement with the log and model results with measured chloride concentrations of 237 and 1313 mg L-1 on samples from depths of 24.5 and 40 m below surface, respectively. This information will be added in the revised version of the manuscript to further support the description of the salt/freshwater distribution in the area.. We believe that the SkyTEM data provide extremely valuable data for model calibration, but they are not free of problems and they should be used jointly with flow and transport (model) inversion (Carrera et al., 2010). The joint application requires significant extra work and it was not possible to include it as part of this study as the interpreted SKyTEM data were available only at the very end of the project. The joint inversion and calibration of the flow model with SkyTEM data will be the subject of future work on simulations and related papers.

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AC: We will improve the figures as suggested in the final version of the manuscript.

RC: 1. Does the paper address relevant scientific questions within the scope of HESS? YES RC: 2. Does the paper present novel concepts, ideas, tools, or data? Partially - not completely novel, but important none the less RC: 3. Are substantial conclusions reached? Yes – based upon the model used in the study. RC: 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).

AC: The raised issues concerning boundary conditions and model calibration are discussed above, and clarifications will be made in the revised manuscript accordingly.

RC: 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.

AC: See discussion on model calibration above.

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Please also note the supplement to this comment:
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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 7969, 2012.