

Interactive comment on “Modelling climate change effects on a Dutch coastal groundwater system using airborne Electro Magnetic measurements” by M. Faneca Sánchez et al.

Anonymous Referee #1

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General comments

In this paper is presented a really complete hydrogeological dataset of a region in the Netherlands, a zone where understand and predict the saltwater-freshwater interactions results essential for the society. In a very clear way is explained the methodology that the authors have developed for creating an improved model of the studied area. The procedure that they follow is consistent and the model results are coherent and reliable. Therefore, one of the main objectives that they pursue (a practical application), could be probably reached. The results are very relevant and presented with very high

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quality figures. It is well written, with a good structure and rigorous in the analysis. I think this paper should be published after attending some minor reviews that I detail in the specific comments.

Specific comments

Page 6140. Geology. The definition of the aquifers as first and second can be confusing; I would recommend a less arbitrary terminology as shallower/deeper or upper/lower. Also in this part it would be interesting a brief description of the hydrodynamics (maybe with some modifications of the figure 2) to explain how the water is moving from the different aquifer layers and the role of the Holocene layer. This would make easier to understand the later explanations about seepage-infiltration and the final discussions in the model results.

Page 6142. It is stated that “We collected groundwater samples and we measured both (EC) total and EC(groundwater). . .” I guess that if you collected only groundwater samples you can’t measured the EC(total). This should be better explained.

Page 6147. Lines 15-19. It is stated that “the EC data derived from geophysical measurements (borehole-logging, AEM, ECPTs) was interpolated. . .” so the authors are giving the idea that they mixed all the different data for obtaining the final 3-D model. I guess that they used the AEM and the other two dataset were useful for calculating the FFi or the correlation with the CI and EC. This could be explained more clearly. As the methodology that the authors proposed included many different calculations and methods I think that a new figure with a scheme of all the procedures, databases and applications of each data would be necessary. This would synthesize all the work presented in this paper.

Page 6149. Lines 15-16. It is commented that the interpolation of the AEM measurements is proved to be better than older measurements techniques but I don’t see where this is proved or discussed.

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Page 6150. Lines 4-5. The authors decided to use the initial 3-D chloride concentration after 15 years of simulation time and I think that is a good idea. But I don't know if this is affecting to the 2005 simulation results. In the year 2005 we would be under these non-equilibrium conditions that the authors detected. This should be clarified.

Page 6151. Model calibration. After reading the model calibration section it is not clear for me which properties were modified and if the results were fitting well with field measurements (of the hydraulic properties). Did you use any automatic calibration software? Did you observe a big change compared with the initial hydraulic properties? In general this part would need some extra information.

Page 6153. Present situation 2005 AD. Did you check if the results presented in the model are fitting well with "true observations" related to the seepage and infiltration fluxes zones, rainwater lenses thickness, EC of the groundwater. . . ?

Page 6155. Line 4-5. This is a very interesting interpretation; the authors can give some approximated values of the differences in the bathymetry to use as a reference for other research areas.

Page 6156. Line 10. The sentence about previous studies was not commented before in the text and also it should include a reference.

Page 6156. Lines 11-16. The explanation about the time for reaching the equilibrium is good but, in this case, as you are reaching it in only 15 years (but then you are modeling 100) can produce some misunderstandings.

Technical corrections

I found some sentences not very relevant that could be removed or better explained for justify their presence in the text. They are marked as unnecessary.

Page 6138, lines 15-16. Unnecessary

Page 6139, lines 4-5. Unnecessary

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Page 6142, lines 3-9. This is explained later in more suitable context.

Page 6142. Lines 26-27. Unnecessary

Page 6143. Line 7. The order of the references should be checked.

Page 6144. Lines 2-3. The explanation about constant thickness that is variable results confusing.

Page 6145. Lines 22-23. The information about the DINO database was already commented, is unnecessary.

Page 6147. Line 16. “ as mentioned. . .” Unnecessary

Page 6150. Line 26. Is the first time that the KNMI is mentioned, it should be explained instead of later in the page 6152.

Page 6155. Line 2. What is PZH?

Page 6155. Line 13. Unnecessary

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