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Interactive comment on “Simulating future salinity dynamics in a coastal marshland under different climate scenarios” *by* Julius Eberhard et al.

Julius Eberhard et al.

e.julius@web.de

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We thank Anonymous Referee #1 for reviewing our manuscript. We believe that the comments and suggestions identified important issues and clearly help to improve the paper, which we are very grateful about. In the following, we would like to respond point by point to the referee comments (RC), typeset in italic type, to the best of our abilities. Responses are marked as author comments (AC) and typeset in roman type.

RC1: *The content or scientific significant is not enough. As the authors mentioned in Section 4, many factors may change in the future, only considering salt from deeper aquifer seems not well considered. I suggest adding more simulations based on this*

model. The effect of short term scenarios is a good idea, such as the storm surges which authors mentioned in Section 1 or extreme long dry season. Also, sea level rise as a global concern should be considered.
AC1: Thank you for the suggestions. We agree that the future development of coastal marshland salinization can depend on multiple factors. Considering the geological setting, in this study area the slow upward seepage from a deep aquifer through a thick confining layer is expected to be the main mechanism of salinization. Direct lateral intrusion of salt water to the unconfined aquifer is not an issue here. Regarding storm surges, there are no known floodings in the past which might have affected the salinity in the Freepsumer Meer; the area is furthermore surrounded by dikes, making flooding due to a storm surge rather unlikely in the future. Through the use of different climate change scenarios, we believe to have included different long-term climatic conditions within the expected ranges and have analyzed their effects. The issue of sea level rise has been approached on page 7, lines 1–5. It has not been considered in the study due to two reasons. First, observations of the deep pressure head nearby the study site did not exhibit any significant trends in 1990–2014 despite an observed sea level rise in this period. Second, we regarded simulated deep pressure heads near the study site, produced by the hydrological model GSFLOW, driven by the climate scenarios I and II. The pressure head differences between simulations with assumed 0 cm, 80 cm, and 150 cm sea level rise within the 2000–2100 period were marginal (in the order of 10 cm at the end of the period) and the uncertainties involved in the model setup were considerable. We concluded that the effect of sea level rise is negligible for salinization in our case. We will include more explanations on the expected relevance of these different processes for the salinization in our study area and the subsequent choice of the model in our introduction when we revise the paper.

RC2: *Please stress the novelties of this study at the end of Section 1.*
AC2: We acknowledge that the study’s novelties are not sufficiently communicated in the introduction. In a revised version of the manuscript, we would therefore add

the following point: The study is novel in that we concentrate on long-term climatic effects on slow salinization from upward seepage of deep groundwater, which we expect to be the main mechanism in the study site. This contrasts with a number of studies describing direct sea-water intrusion or short-term effects through boils, paleochannels, etc. in coastal marshlands (e.g. Weerts, 1996; Kim et al., 2003; de Louw et al., 2010; Colombani et al., 2015; Kliesch et al., 2016).

RC3: *Page 4, Line 20-24, authors illustrate the boundary conditions. Please clarify the boundary conditions for salinity, how much salt comes from the bottom boundary?*
AC3: Indeed, a specification of the bottom boundary condition for salinity is missing and will be added. The salinity of the confined groundwater is given as a constant parameter in the model and was estimated in the model calibration. Table 2 in the manuscript provides the calibration range and the estimated value of 6.6 mg cm^{-3}.

RC4: *Page 4, Line 33-34, authors used grain size distribution, organic carbon content, and bulk density to estimate three parameters. Please clarify how this estimation works, any equations used in this estimation. Authors can provide some supplementary of this estimation if necessary.*
AC4: Thank you for the suggestion. We agree that the manuscript can be improved by adding the proposed details. As discussed in Section 2.4, we used pedotransfer functions of Wösten et al. (1999) in the calibrated model (with an adjustment of Θ_r), viz.

$$\lambda = 10\frac{\exp(\lambda^*)-1}{\exp(\lambda^*)+1},$$

$$\begin{aligned}\alpha = \exp\{&-14.96 + 0.03135 \times \text{clay} + 0.0351 \times \text{silt} + 0.646 \times \text{SOM} + 15.29 \times \text{BD}\\ &- 0.192 \times \text{topsoil} - 4.671 \times \text{BD}^2 - 0.000781 \times \text{clay}^2 - 0.00687 \times \text{SOM}^2\\ &+ 0.0449/\text{SOM} + 0.0663 \times \ln(\text{silt}) + 0.1482 \times \ln(\text{SOM}) - 0.04546 \times \text{BD} \times \text{silt}\\ &- 0.4852 \times \text{BD} \times \text{SOM} + 0.00673 \times \text{topsoil} \times \text{clay}\}\,\text{cm}^{-1},\end{aligned}$$

$$n = 1 + \exp\{-25.23 - 0.02195 \times \text{clay} + 0.0074 \times \text{silt} - 0.1940 \times \text{SOM} + 45.5 \times \text{BD}$$

$$\begin{aligned}&- 7.24 \times \text{BD}^2 + 0.0003658 \times \text{clay}^2 + 0.002885 \times \text{SOM}^2 - 12.81/\text{BD}\\ &- 0.1524/\text{silt} - 0.01958/\text{SOM} - 0.2876 \times \ln(\text{silt}) - 0.0709 \times \ln(\text{SOM})\\ &- 44.6 \times \ln(\text{BD}) - 0.02264 \times \text{BD} \times \text{clay} + 0.0896 \times \text{BD} \times \text{SOM}\\ &+ 0.00718 \times \text{topsoil} \times \text{clay}\},\end{aligned}$$

$$\Theta_r = 0.1,$$

where clay, silt, SOM denote percentages of clay, silt and soil organic matter, BD is the numerical value (without unit) of bulk density measured in g cm^{-3}, topsoil is 0 (below -30 cm) or 1 (above or at -30 cm), and

$$\begin{aligned}\lambda^* = {}& 0.0202 + 0.0006193 \times \text{clay}^2 - 0.001136 \times \text{SOM}^2 - 0.2316 \times \ln(\text{SOM})\\ &- 0.03544 \times \text{BD} \times \text{clay} + 0.00283 \times \text{BD} \times \text{silt} + 0.0488 \times \text{BD} \times \text{SOM}.\end{aligned}$$

We propose to add the equations in a supplementary.

RC5: *Page 6 and Page 7, authors illustrate the calibration and provide Fig 3 to prove a good fit between simulation results and observations. Please provide more details about this calibration. The calibration results should be quantified to make this calibration more persuasive to readers, using correlation coefficient or Nash–Sutcliffe coefficient or other methods to quantify the comparison between simulation and observation. In addition, I suggest briefly introduce the basic theory of PEST to make readers understand this process more clearly.*
AC5: Thank you for the valuable suggestion. We agree that the calibration needs some clarification. We propose to add the following description to our paragraph on the model calibration:

"The parameter calibration was performed in three steps: (1) First, the parameters saturated hydraulic conductivity (k_s), dispersion length (L_{dis}), deep groundwater salinity, and vertical resistance were estimated with the aim to minimize the sum of squared deviations between the simulated and measured groundwater levels. For this purpose we used the PEST software package (Doherty, 2010) which uses a steepest decent searching optimization algorithm based on Gauss–Marquardt–Levenberg.

(2) Second, the value of the deep groundwater salinity was varied, leaving all other parameters as estimated in the previous step. Here a visual comparison of measured and simulated salinity was used for the optimization. (3) After the previous steps were performed for each of the three PTFs, the sub-annual dynamics of each were compared (locations of local minima and maxima, ranges of sub-monthly fluctuations) and the PTF of Wösten et al. (1999) was chosen. For the final calibrated model, the sum of squared deviations between modeled and observed groundwater levels is 694.8 cm^2. The mean deviation per observation is 2.0 cm. The correlation coefficient of modeled and observed groundwater levels is 0.76."

RC6: *Page 6, Line 10-20, please provide more details about these six scenarios, such as what's the difference between SRES and RCP. These climate conditions are cited from other literature, but still need to illustrate in this study to make readers understand the predictions clearly.*

AC6: We will try to make the descriptions on the six scenarios clearer by rephrasing the paragraph about the climate scenarios:

"Scenarios I and II involve greenhouse gas emission scenarios from the SRES-A2 and SRES-B1 families. In direct comparison, A2 scenarios are characterized by a regionally oriented economical development and a higher global population growth, while B1 scenarios follow a storyline of a more global economic growth and a lower population growth (Nakićenović and Swart, 2000). In contrast to the SRES scenarios, in the RCP scenarios the greenhouse gas emissions are decoupled from socioeconomic models. Instead a range of possible future CO_2 emissions and radiative forcing trends till 2100 are used. Thus the RCP4.5 and RCP8.5 scenarios correspond to an increase in radiative forcing of 4.5 and 8.5 $\mathrm{W\,m}^{-2}$, respectively, in 2100 compared to pre-industrial values. The differences in the trends in temperature and precipitation for the resulting six scenarios are described in the following paragraphs."

RC7: *Page 5, line 21, is this equation 2? Please mark clearly.*

AC7: We agree that the typesetting of the second equation may be irritating. Since this equation is not further discussed or cited and was just meant to clarify the meaning of the different resistance values, we suggest including it in the line, e.g.: "Every resistance value can be understood as the ratio d/k_s, where d is the distance . . . "

RC8: *Table 1 is too simple to understand, the figure and table should be complete and informative itself. Please add more illustration in the title, and clarify what is Θ_r, Θ_s, and other symbols. Please amend it for other tables and figures.*

AC8: Thank you for this remark. We realize that we should improve the titles of various tables and figures in the manuscript, we will work on this.

RC9: *Please improve or redraw Fig 1 a and b, the study site seems not clear. In Fig 1a, please use some color instead of the hatched area; mark that the small map on the left corner is German[y]. In Fig 1b, please adding elevation data in the study site. I am not sure what's the blue lines in the lake in Fig 1b, do these blue lines represent drains? Why there are so many drains in this former lake?*

AC9: We see that Figures 1a and b need improvements in order to characterize the study site better. The map of Germany will be designated clearly and the hatched areas will receive a new filling or color in future versions of the manuscript. In fact, the Freepsumer Meer, which is the drained area between the two villages, forms a quite distinctive depression compared to the surrounding areas. We intend to add three indications to the figure caption: First, a more specific description of the extent of the former lake; second, that the whole of the Freepsumer Meer lies on average 1.5 m below the surrounding area; third, a clarification that all blue lines are drains. In our opinion, the second point would avoid the need for detailed elevation data, which might clutter the figure. The relative depression of the Freepsumer Meer also explains why there are so many drains – historically, the drainage of the lake required a fast and efficient removal of water.

RC10: *Figure 2, the plot of Totalized Rainfall to year, the winter rainfall sums should be Oct–Mar.*
AC10: Thank you for this correction. It should indeed be “Oct–Mar”, we will change it.

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