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Hydrology and Earth System Sciences Discussions

Interactive comment on "A combined field and modeling study of groundwater flow in a tidal marsh" *by* Yuqiang Xia and Hailong Li

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Dear Prof. Bauer-Gottwein,

We are very grateful to you for your recognition to our work. We have included observed salinity data in our revised manuscript and fully addressed your comments in detail below. Thanks.

Best wishes, Hailong

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1> Original comment 2-1:

The authors postulate that the difference in vegetation cover between the mangrove transect and the bald transect is due to the absence of lateral freshwater recharge under the bald transect. This hypothesis is plausible; however, the manuscript does not provide hard evidence for this hypothesis beyond the good fit between simulated and observed hydraulic heads. Salinity data from the high permeability zone and/or seepage fluxes along the groundwater/surface water interface would provide much stronger support and are easy to collect in this shallow groundwater system as far as I can see. Once concentration data is available, it can be used for the calibration of the variable-density groundwater model, which will result in a much more robust and reliable model.

Response 2-1:

Actually, we had measured salinity data along the two transects, and there are sufficient field observations (e.g., sediment cores, water table and salinity data) to support the hypothesis, i.e. the absence of freshwater recharge from the landward side of the bald beach transect. However, since we thought that the good fit between simulated and observed hydraulic heads were "enough" for this hypothesis, we did not include the salinity observations in the original manuscript considering the clarity and succinctness of the manuscript. In the revised paper, we closely followed your suggestions and included the observed salinity data. We believe that the salinity data provide stronger support to our hypothesis.

Additionally, even before the first submission, great efforts had been made to calibrate the salinity data using the two-dimensional variable-density groundwater model MARUN, but the match between the two-dimensional numerical model results and the salinity measurements was poor. Owing to this, we only report the numerical simulation results that neglected the density effects. We have added relevant statements in our revised manuscript(lines 350-360):

"The salinity along the bald beach transect is assumed to be constant due to the small variation of the observed salinities in wells along that transect (Figs. 4b and 6), so the density effect and solute transport were not considered in the model. Considered the relatively significant salinity variations along the mangrove transect, great efforts had been made to simulate the observed salinity variations using a two-dimensional cross-section numerical model, but the match between simulation results and the salinity measurements was poor. This is most probably due to the significant density-dependent three-dimensional flow near and within the observation wells, i.e. well effects on the groundwater flow. In order to quantify the well effects, a very fine three-dimensional mesh near each well is necessary. However, such a great numerical effort is beyond the scope of this paper. Owing to this, here we only report the numerical simulation results that neglected the density effects related to solute transport."

In addition to above improvements, our revision also provided detailed qualitative explanations to the observed salinity variations along the mangrove transect. The paragraphs read:

(Lines 227-229) "However, the quick dropping of the salinity observed at M0 during lower tides also implied that there was significant freshwater recharge from inland, namely, the averaged flow direction at M0 should be seaward, which excluded the possibility of landward drainage."

(Lines 243-246) "During high tides, the ground surface at wells M1-M6 was submerged, and the observed salinities of deep water in these wells were close to those during the low tides. However, the salinity of shallow water in well M1 was obviously lower than those of the deep water. This is possibly because the freshwater from M0 diluted the shallow water salinity in well M1."

(Lines 256-260) "Note that although the salinity of the deep water (18.6 ppt $\sim$ 25.2 ppt) at M8 is much higher than that of the shallow water (3.9 ppt $\sim$ 8.4 ppt) there, it is much lower than the salinity of the pore water at M1-M7 (27 ppt $\sim$ 33.6 ppt) and close to that at M0 (12.9 ppt $\sim$ 26.1 ppt, see Fig. 4a). These observations strongly indicate that a freshwater discharge path (tube) was formed near M8 (see Fig. 2a)."

(Lines 265-267) "The salinities of deep and shallow water at M7 were close to each other and relatively high during low tides. This implies that the freshwater discharge tube was deeper than the bottom of M7."

2> Original comment 2-2:

From my point of view, the presentation of the findings is fairly unbalanced. While a lot of detail is given on the hydrogeological field work and groundwater simulation, the eco-hydrological coupling mechanisms are only summarily discussed and not quantitatively simulated. However the novel aspects of this study clearly are related to the eco-hydrological phenomena, while the hydrogeologic field work and modeling is standard. I suggest the authors rebalance both the presentation and possibly also the modeling. Eco-hydrological feedback mechanisms (e.g. phytotoxicity, transpiration stream concentration factor) can be incorporated into the quantitative simulations (e.g. Bauer et al., 2006, Bauer-Gottwein et al., 2008). It would be very interesting to see salt and water balances for the two transect as simulated in such an approach. In my view, the modeling of the land surface boundary is over-simplified in this study. Precipitation and evapotranspiration may be important processes in this system and have an influence on subsurface flow and concentration patterns.

Response 2-2:

We agree that the study of eco-hydrological feedback mechanisms based on the precipitation and evapotranpiration is very important and interesting in salt marsh eco-hydrology. We appreciate that Bauer et al. (2006) and Bauer-Gottwein et al. (2008) made great contributions in this aspect, and we have added these valuable references in our revised paper. Considering our current study, however, we have to decline the quantitative model simulation of the precipitation and evapotranpiration owing to the following reasons: (1) the period of our field work is short (only three days), and there was no precipitation occurring during this period; and (2) our field work was conducted during the winter, the evapotranspiration was not significant and could be neglected. In fact, from Fig. 4 one can see that, during low tides, the observed water table elevations of several wells (e.g., M2, M3, M4, M5, M6 and B4, B5) in the middle intertidal zones of both transects were essentially constant and very close to the ground surface elevation, indicating that there was no evapotranspiration, or at least the evapotranspiration was less than the measurement error of the water table elevation. Therefore, quantitative modeling of the precipitation and evapotranpiration needs at least long-term, high-accuracy field measurements, which is beyond the current study.

In the section "Discussions" of our revised paper, we have added the following discussion regarding the influence of the vegetation on the hydrologic cycle and our experiment results (lines 479-497):

"The presence of the vegetation tends to increase the salinity due to evapotranpiration, and thus may increase the pore water salinity and decrease the water table. However, the observed constant water table elevation very close to the ground surface at wells M2, M3, M4, M5 and M6 during low tides indicated that the evapotranspiration during the field work was less than the measurement error of the water table elevation. And the observed low salinity at several wells along the mangrove transects indicated that the increasing effect of vegetation evapotranspiration on the salinity is overwhelmed by the freshwater recharge. The salinity of the groundwater recharge into the high-permeability zone near M0 is very low (may be even fresh in deep part of M0). On the other hand, the salt through the vertical leakage of the mud zone into high-permeability zone is limited owing to the fact that the permeability of the mud is several orders of magnitudes less than that of the high-permeability zone (Fig. 8). Therefore, the salinity of the pore water in the whole high-permeability zone may be significantly lower than that in the mud zone. Thus, the high-permeability zone may provide opportunity for the plants in the mangrove marsh to uptake freshwater through their roots extending downward into the high-permeability zone, which may prevent the accumulation of salt in mud zone caused by plant evapotranspiration. Thus, it may be concluded that the bald beach (not covered by mangrove plants) is due to the lack of freshwater recharge

for generating a brackish beach soil condition essential to mangrove growth."

The present study is focusing on the hydrogeological field work and groundwater simulations in the marshes and their ecological implications (qualitative). Conducting the modeling of eco-hydrological feedback mechanisms is beyond the scope of present paper, but we are going to conduct such study in the next step. In the revised paper, we have also stated the issues that have not been considered in the present study, please see the last paragraph of our revised manuscript (lines 573-580). It reads:

"Finally, there are many issues that have not been considered here but should be examined urgently. These include, e.g., the long-term observations of precipitation, evapotranspiration, pore water salinity and water table variations along the mangrove transect, the quantifications and modeling of eco-hydrological feedback mechanisms based on these observations (e.g., Bauer et al., 2006, Bauer-Gottwein et al. 2008), and the effects of the mixing and diffusion of solute inside the observation well on the groundwater flow that are essentially three-dimensional or at least locally radial, i.e. the well effects on the density-dependent groundwater flow near and within the observation wells."

3> SPECIFIC COMMENTS

1. Page 5129, Line 16-23: This paragraph describes an electronic dipper system, which is a standard technology in hydrogeology. This paragraph can be deleted.

Response: Following your comments, we have deleted this paragraph.

2. Page 5133, Line 1: Replace permeability by hydraulic conductivity. Permeability has units of m2.

Response: Corrected as advised. Thank you.

3. Figure 11: It should be made clear in the caption that the flow rates are not field observations but extracted from the numerical simulation. There is a typo in "Inflow" in the figure.

Response: Corrected as advised. Thank you.

4. Tables 1 and 2 should include hydraulic heads as recorded in the field. Hydraulic head data are qualitatively described on page 5131 but should also be presented, either in tabular or graphical format.

Response: Figure 4 in the previous manuscript has shown the time series of hydraulic heads (water levels) in each well.

5. References Bauer, P., R. Held, S. Zimmermann, F. Linn and W. Kinzelbach, 2006: Coupled flow and salinity transport modelling in semi-arid environments: The Shashe River Valley, Botswana. Journal of Hydrology 316(1-4): 163-183. Bauer-Gottwein, P., Rasmussen, N.F., Feificova, D. and Trapp, S., 2008: Phytotoxicity of salt and plant salt uptake: Modeling eco-hydrological feedback mechanisms. Water Resources Research, 44: W04418.

Response: We thank the reviewer for providing these references. We have added them in our revised manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 5123, 2011.