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Discussions

Interactive comment on "A field validated surrogate model for optimum performance of irrigated crops in regions with shallow salty groundwater" *by* Zhongyi Liu et al.

Anonymous Referee #2

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This manuscript describes a simplified surrogate model for soil water and salinity dynamics in the vadose zone above shallow water table. The model was calibrated using two years of field data in four fields with maize and sunflowers. Overall, the novel approach employed to simplify the input parameters is interesting, and the quality of writing and presentation in the manuscript is nearly prepared for publication. However, the authors should first address a few issues, detailed in the comments below.

General comments

1. Because the manuscript considers shallow groundwater and surface irrigation, it would help the reader to clarify what is meant by "surface irrigation" (which I assume to

indicate flood type irrigation) and differentiate this from irrigation that is supplied from surface water (as opposed to groundwater supplied irrigation).

2. In the introduction, the authors discuss basic soil physics of hydraulic flow under conditions which require considering matric potential. This is one of the primary contributions of this model and analysis. Regarding the potential for the model to inform irrigation optimization, additional review of salinity management in surface irrigated systems would be appropriate. A model of hydraulic flow and salinity is interesting and potentially very helpful, but should be posed in terms of operational considerations that are relevant to irrigators.

3. The authors employed five standard statistical measures of model performance (RMSE. MRE, Nash-Sutcliffe, Rˆ2, and regression coefficient). It would add to the mansucript to discuss why these particular measures reflect model performance, or how they complement each other in evaluating the robustness and representativeness of the model outputs. It would strengthen the results if the analysis included some hypothesis testing, beyond the validation and sensitivity analysis which are presented. There is certainly sufficient sampling (both experimental and modelled) to prepare a compelling significance test.

4. The authors refer to "soil moisture content" and "soil water potential" somewhat interchangeably. Here moisture content is referred to at -33kPa, which is a potential. Please be careful is clarifying this distinction here and throughout the manuscript. From line 248, the Brooks and Corey characteristic curve was used to relate soil moisture content to matric potential. The explicit treatment of the water and salinity flux in section 2.3.2 is helpful, but not sufficient, for me or the average reader to keep track of which parameters are modelled explicitly and which are derived, so keeping the units and variables clear will help a great deal.

5. In Figure 9, it is hard to relate how the predicted soil characteristic curve has been fitted to the observed data. The explanation about points being located to the left of

the curve due to mismatched rates of recharge and root extraction makes sense, but not if virtually all the observations do not fall on or near the curve. This becomes more important in the next section on sensitivity analysis. If the sensitive input parameters are, as the authors say, related primarily to soil hydraulic properties, then it would help the reader to understand how the authors addressed uncertainty in these parameters. It is clear that the authors have done substantial work to calibrate the model to

Minor comments

Line 168: I generally agree with the statement that "Finer resolution is not needed for managing water and salt content for irrigation". However, other aspects of irrigation management are managed on shorter time periods, and consider environmental variables that are not well represented by daily averages. I think it is important to specify the limits of any model, especially surrogate models and models that couple processes that operate over different time and spatial scales. As noted by the authors (line 89), surrogate models are not as versatile as complex models. In keeping eith the intent of making the model generally useful under real world conditions, please be more explicit about the range of conditions under which this model has been shown to work.

Line 206: Considering that maize and sunflowers have very different responses to drought and salinity, and different root development/depth, please discuss further why the same ïĄď values were used for both crops. Otherwise the discussion of root functions is adequate for this presentation.

Line 393: It is a minor point, since Figure 3 is not used except to provide a general visual reference, but please check your citation of the GE imagery. In general, include date of the image, and the date that the image was downloaded.

Lines 411-421: Were manual measurements of soil moisture taken in field B at any point in 2018 to calibrate/corroborate the Hydra Probe sensor measurements?

Line 437 and elsewhere: Some symbols (such as theta for volumetric water content)

are italicized at some points in the text, but not in other places or in tables. Please use a consistent symbol and font so that notation is clear throughout the manuscript, and define each symbol at first usage. Also, please be consistent with notation subscripts (f.c., 33, 15, etc.). Because the manuscript describes calculation steps and several equations and several cases for each equation, a table with all notation for variables and subscripts may be very helpful to the reader. Also, I could not find the first usage of ms cmˆ-1, which may need to be explained as millisiemens per centimeter, and is typically noted as mS cmˆ-1, with siemens capitalized.

Line 485: While the period in 2017 is five weeks longer than the period in 2018, this is still a remarkably large difference in reference ET between the two growing seasons. Please offer some explanation why ETref would differ so much in 2017 and 2018.

Lines 543-546: Even if there is not proper documentation, is there some supporting information to corroborate the suspected spillover event? Why would this event occur five times (twice in 2017 and three times in 2018) in field C (located in the center of the other fields) and not be observed in the adjacent fields?

Figures 11-13: Enclose the legend within a box so that legend entries can't be mistaken for data. This is especially evident in figure 11, where the legend entry is the same size as the data.

Section 4.4.3: Apart from a visual resemblance of correlation between the predicted and observed salinity in figure 16, the Rˆ2 and NSE do not support the idea that "the observed and predicted values were in close agreement (Fig. 6, 16 ", nor the claim that " the model can predict the law of salt concentration fluctuation during crop growth period and the prediction results are acceptable." Basedo n my reading, it might be more accurate to say that variability was low on a daily time step, and that initial salt concentration is the most important parameter to measure on a seasonal basis. However, I can see how this would undermine the usefulness of the model, and I do think the model and this work merit further attention. The authors have attributed a potential cause of low NSE to the low variability, but this is also related to the relatively small sample size. It might help to assess this with some kind of significance test. Here is one possible approach which I found with a quick google scholar search: Ritter et al., 2013, "Performance evaluation of hydrological models: Statistical significance for reducing subjectivity in goodness-of-fit assessments"https://doi.org/10.1016/j.jhydrol.2012.12.004

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