

Comments to HESS-2015-244

Rezaei et al. present an interesting analysis of soil water availability in grassland as a function of irrigation, precipitation, and ground water levels. They successfully model crop growth over two seasons as well as successfully determine hydraulic parameters required for the model. The approach used to optimize the hydraulic parameters is creative and robust. Their modeling efforts can improve irrigation scheduling, although the improvement achieved in this study can only be achieved with complete foreknowledge of precipitation amounts. Overall the paper is well-written, though some sections still require clarification (see specific comments below).

### **Specific comments:**

Title, keywords and throughout the MS: The term “soil water stress” is misleading. Soils don’t experience water stress, plants do. I suggest using “soil water availability” or some other alternative, and to avoid using soil water stress unless plant transpiration/root water uptake is mentioned as well.

### **Abstract**

P1:L20, which model? The crop growth or the soil hydrological one? Or should it be models?

P1:L23, after “A sensitivity analysis” include “of the hydraulic parameters”: overall you show that soil water content is affected by boundary conditions more than by hydraulic parameters (as you mention later).

P2:L2, “For this case” – For which case?

P2:L5, if there are a large number of stress conditions, how are the different stress conditions defined?

### **Section 1: Introduction**

P3:L26, in the rest of the manuscript, the bottom boundary condition evaluation is the last step, not the first.

### **Section 2: Materials and Methods:**

P4:L8, would it make more sense to keep the soil profile information together? Also, if the Ap horizon thickness was between 30 and 50 cm, why did the authors choose a thickness of 33 cm for their modeling efforts?

P4:L12, how was Fig. 1 generated?

Regarding equations, symbols and units; please be consistent with italics (e.g. P8:L5, P9:L1-6, P10:Eq.11-14) and give the actual units used, i.e. instead of length (L) and time (T) use e.g. “cm” and “h” or “m” and “d”...

P7:L7, which model?

P9:L4-9, this is a repetition of P7:L25-26

P9:L26-27, perhaps move the reference to equations after defining said equations.

P10:Eq.12, Should “ $\Delta x$ ” be “ $\Delta x_i$ ”?

P11:L6-7, between 1 March and 26 November 2012, there are 3240 2-hourly intervals, and 12960 ½-hourly intervals. Please clarify.

According to P3:L25-26; the first step in the evaluation was to determine effects of changing bottom boundary conditions on model performance. The methods lack a description on how this was done; and what ranges were used. P4:L8 mentions groundwater levels of 80-155 cm, Figure 1 indicates the GWL at the measurement site is 130-150cm, and P5:L16-17 mentions measured GWL of 140-136 cm... please clarify.

### **Section 3-4, Results and Discussion, Conclusions**

P15:L21, If the model performance is lower in 40cm than at 10-30 cm, does that mean the Ap horizon was easier to calibrate than the Cl horizon?

P17:L6, This doesn't make sense to me. Temperature and light limitations will reduce  $ET_0$ , and shouldn't affect stress parameters.  $K_c$  will still be 1,  $ET_0$  will be lower... Please clarify.

P17:13-14 and P19:L19, these efficiencies can only be reached if rainfall is known a priori: while the soil water status could indicate *when* to irrigate, it would be impossible to know *how much* to irrigate if the rainfall cannot be accurately predicted. This should be mentioned more explicitly.

### **General comments**

General comment figures, font sizes are quite small; especially Fig. 4 is hard to read. Fig. 7 and 8 are fine, Fig. 6 has different font sizes, Fig. 3 and 5 font sizes are small, Fig. 4 font size is very small, Fig. 2 is great and Fig. 1 font size is small.

Fig. 1, GWL is not defined.

Some paragraphs have poor English, e.g. 2.6