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Response to Reviewer #1 Comments
####Authors' response in Blue####

Review on hess-2020-146 "Combined Simulation and Optimization Framework for Irrigation Scheduling in Agriculture Fields"

We are grateful for the time and effort that the reviewers spent on the manuscript. Our response to the reviewers is attach to this document. We believe that our responses and the revisions made to the manuscript fully address the issues raised by the review. These revisions have helped clarify some aspects of our work and improve its interpretation.

GENERAL COMMENTS

This study proposed a method to locate optimal irrigation schedules considering the soil water movement. The research question is interesting, but I am not sure if it is relevant within the scope of HESS. In addition, I found the complexity of modeling practices could be consistent and optimization methods could be improved so that the results could be more reliable and practical. For instance, the crop yield model looks too simple (only a function of ET), compared to that of the soil water model (HYDRUS1D). There are more comprehensive crop models such as DSSAT and EPIC. The brute force method used when trying to locate the optimal scheduling could be fine if the authors wanted to see the relationship between two factors or objective functions, but it is not an efficient way to explore the multi-dimensional parameter space. Such a limitation did not allow the authors to explicitly investigate the trade-off between the objective functions and develop a Pareto front in the study.

We know that Stewart (1977) model is simple but we decided to use this model because it has been widely accepted and recommended by FAO and used by several authors in recent years (Domínguez et al., 2012; Irmak et al., 2016; Martínez-Romero et al., 2017; Saadi et al., 2015). Note though that the ET values used by this model are determined from HYDRUS and are therefore consistent with soil water movement, salts and crop stress factors. This makes the application of this model quite more complex an accurate than used in practice. Nevertheless, it is important to point out that the paper presents a general methodology to optimize irrigation scheduling in agricultural fields. Therefore, how to model each compartment is not that important. We rather focus on the interplay between them. We added some clarifications specifying that several crop yield model can be applied instead Stewart (1977) model (Line 189):

"The model presented by Stewart et al. (1977) Eq. (13) has been widely accepted and recommended by the Food and Agriculture Organization of the United Nations (FAO) (Doorenbos and Pruitt, 1975, hereafter FAO24). In addition, it has been recently used by several authors (Domínguez et al., 2012; Irmak et al., 2016; Martínez-Romero et al.,

2017; Saadi et al., 2015). Note though that methodology proposed is not limited by this model and can also be used with other soil – water – crop productivity models if needed."

We agree that the brute force method used to locate the optimal scheduling is not efficient. We actually state this in the manuscript in Line 318. We preferred to use brute force in this case in order to explore in detail the objective function as a function of the irrigation parameters. Of course, the methodology and the results presented do not depend on the algorithm to minimize the objective function.

The text in the manuscript where all of this is explained reads as it follows (Line 317):

A large number of algorithms can be used to maximize the crop net margin cost function NM with constraints. Here, we chose to maximize NM over a given range by brute force, which simply consists in computing the function's value at each point of the parameter space to find the global maximum. This can be inefficient in practical applications but provides detail insights about irrigation scheduling as well as the full shape of the NM cost function, which is the objective here. To do this, the parameter space (τ, h^{*}) *was discretized into a* 4×10 *regular mesh, where* τ *ranges between 1 and 4 hours and the threshold pressure head* h^{*} *varies between -100 KPa and -10 KPa.*

SPECIFIC COMMENTS:

Lines 49 to 50: I do not agree with this statement. The ET based method can provide information on irrigation water application timing when it is combined with soil water content accounting.

Agreed. We modified the sentence as it follows (Line 49):

"*If water requirements are not combined with soil moisture sensors measurements, they do not provide the frequency and duration of irrigation (stakeholders do not know when to apply this volume of water) and requires accurate estimations of weather conditions.*"

Equation 13: This model accounts only for the impacts of ET or soil water content on crop growth, and I think this is too simple compared to the complexity of using the soil water simulation model, HYDRUS-1D.

Stewart (1977) model is based on ET and a crop response factor. We assume that it is a simple model but we specified in "General comments" section, that several authors applied this model recently and it is recommended by FAO. However, even this model is simple, input variables necessary to apply the model (ET values) are extracted from a HYDRUS simulation who contemplates soil water content patterns and salts concentration through the root zone. Based on FAO recommendation, the authors content that these values are representative. Therefore, if used as input variables in Stewart (1977) model crop yield results must be a good approximation.

Line 221: Please briefly describe what these devices for.

we explained what these devices for in the following sentence, please, let us know if you need more details (Line 229).

"*Whereas the HYPROP device is capable to measure SWRC and HCC, WP4c can complement SWRC in the dry region. The KSat system does the same for HCC. A comparison of approaches has been reported by Schelle et al. (2013).*"

Lines 256 to 257: Does this mean that the differences between them are not "statistically" significant? Please clarify it.

Sorry, the use of the word "significantly" in this sentence is confusing since it seems that we want to express "statistically significant". This is not what we meant. It is a simple appreciation of the results, pointing to the fact that they do not substantially deviate from the initial estimate. We have substituted the word "significantly" by "substantially" to avoid this confusion. (Line 275)

Line 266: I am not sure if we can say this. Please try to justify the evaluation using literature.

We are not completely sure what is the reviewer asking here but we agree that the fitting is not that good at depth 20 cm during the first 200 days after sowing compared to the simulations obtained at depth 10 cm. We have changed the text in the manuscript to acknowledge this:

We modified the sentence as follows (Line 287):

"*Figure 3 compares simulation results with soil moisture field measurements obtained at two different depths. Simulations are in good agreement with soil moisture data, except for a relatively small underestimation of the water content measured at depth 20 cm by a factor of about 1.15 during the first 200 days after sowing*"

Lines 271 to 272: Please describe the weather conditions in detail.

We described the weather conditions as follows (Line 300):

"*The year with more water demand was 2016 with an atmosphere demand of 478 mm and a total rainfall of 80 mm. During this period of time, the maximum and minimum temperatures were 39ºC and 21ºC, respectively."*

Line 272: Please justify such selection of weather condition in terms of the reliability and applicability of the results. I think it is worth adding other weather conditions (e.g., most favorable and average) and comparing the efficiency of the proposed method.

Firstly, we want to clarify that the methodology proposed in this work can be applied with any weather conditions, from the most unfavorable conditions until the most favorable ones. In order to prove that the methodology will work under unfavorable conditions, we decided to simulate this particular case with the most unfavorable conditions because we think that the crop is more susceptible to be under water stress conditions than a year with low atmosphere demand. In this case, we assume that it will be more complicated to solve the simulation – optimization problem with a realistic result when atmosphere demand is high. The reason is because it will be more difficult to maintain soil under optimal soil moisture conditions.

Lines 292 to 293: I do not think the brute force sampling strategy can locate the global optimum.

The brute force explores the entire parameter space and therefore, by definition, can detect local and global maximum values at the expenses of CPU times. It is true though that the exploration requires defining discrete points where the objective function is evaluated and therefore the exact global optimum can slightly deviate from our results. We see though that the objective function is quite smooth and this smoothness is larger than the sampling frequency. This gives confidence to the results provided.

Lines 293 to 294: Please provide examples of showing the detail insights about irrigation scheduling.

The "detail insights" about irrigation scheduling are already provided in section 4 of the present manuscript. In this section, we provide the maps of the objective function as a function of the parameters. From these maps, we give guidelines for improving irrigation.

Lines 298 to 301: I do not think this is a "realistic" traditional irrigation scheduling method, which may determine daily (rather than weekly) irrigation timing and amount based on daily (rather than weekly) weather conditions.

Our experience in the Segarra-Garrigues agricultural fields in Spain indicates that it is more convenient defining weakly water requirements. The reason is that agricultures must then reprogram the irrigation controller once per week and not daily, which is too time consuming and annoying for them.

Line 306: I expected to see a plot showing the trade-off between the objective functions (or a Pareto front), but Figure 5 does not show it.

The main goal of this figure is to represent NM, GM… of each simulation, providing a global perspective bout how all irrigation strategies affect the system. In fact, we define here the optimal irrigation strategy, but we also describe useful information that must be considered, such as, the relationship between GM and Opex.

Line 318 ("proposed method can increase the net margin by 7%"): Please describe how the amount of water irrigated and the corresponding cost can be improved (or reduced) by implementing the proposed method.

We added the information required (Line 343):

"*Results show that the proposed method can increase the net margin by 7%, decreasing by 6% the total amount of water applied at the end of the campaign, and reducing by 5% the costs associated by irrigation.*"

Lines 400 to 401: Considering the amount of uncertainty in the analysis and its results, I am not sure the 7% increase of the net margin is significant. Please try to quantify uncertainty of this analysis, as there are many assumptions and simplifications made in the analysis and modeling.

This is a synthetic case study designed to illustrate the method proposed. As such, the approach is deterministic. We think that a stochastic analysis of irrigation scheduling to account for the uncertainty in the spatial variability of the soil attributes is out of the scope of the present manuscript, whose objective is to present a general framework for optimizing irrigation. We do not discard to consider a stochastic framework in the future but this would require a full paper in itself.

Lines 404 to 408: Considering these shortcomings of this method, I am not sure if agriculture stakeholders can use this method in practice. I wonder how the authors are going to make this tool available to the stakeholders.

Thanks. In section 5 we have rewrite the sentence to clarify how a stakeholder must implement the method. The text reads as it follows (Line 436).

"In order to implement the method some measurements are required. Firstly, it is necessary to measure soil hydraulic properties to provide the model the information necessary to simulate soil moisture through the root zone. Secondly, it is recommended to have a weather station in the study to calculate the potential evapotranspiration demand. Unfortunately, some stakeholders have not the opportunity to have installed a weather station in the field, in this case, weather data must be downloaded from the nearest station. It is also highly recommended to install pressure head potential sensors to calibrate the model and verify that irrigation is triggered at the correct threshold."

Figure 3: The model underestimated soil water content at the 20 cm depth, which may lead to the overestimation of irrigation water.

During the field campaign one soil sample was collected at 10 cm depth. Soil hydraulic parameters are representative of this depth, that is why simulated soil moisture vales have a better agreement at 10 cm than 20 cm. Although, simulated soil moisture data at 20 cm depth is not exactly the same as field measurements, we consider that they are representative. One the one hand, the model reproduces the same soil moisture dynamics as field measurements. Thus, soil processes are well simulated. On the other hand, when irrigation is applied (from day 180th to day 225th) soil moisture data have a good agreement with sensors data. We specified in the manuscript that soil sample were taken at 10 cm depth (Line 226):

"One undisturbed soil sample was taken at 10 cm depth using a stainless-steel ring of 250 cm3 capacity."

Figure 6: The optimal scheduling requires to turn on and off the irrigation pump and system frequently, which may lead to increase in operation and maintenance costs. I am wondering if such additional potential costs can be considered in the optimization framework.

Yes, it is considered in the optimization framework in Eq. 5 where Operational costs are defined. Note that the variable Ce is the energy cost. During the example exposed in this work, we did not have information about energy costs. For this reason, we did not calculate energy costs.

Table 4: RMSE values, 0.12 and 0.08 look substantial when considering the fact that the amount of available water content is around 0.35 from Figure 3. 0.12 and 0.08 correspond to 33% to 23%. Table 4: How about the overall bias?

Agreed. We found a mistake in Table 4 and we fixed it. The real RMSE at 10 cm depth corresponds to 0.012 and not 0.12. As we explained before, we took a soil core at 10 cm depth. Thus, we consider that the model simulates soil moisture at 20 cm correctly but the agreement between simulated and measured data is better at 10 cm depth.