Dear authors,

This paper presents a study of using a machine learning framework, “FarmCan”, to forecast irrigation demand in 4 farms in Canada using machine learning. The authors find that soil moisture shows a strong correlation with precipitation and that ET and PET are effective predictors of NI. The study shows the potential of using machine learning models to improve the timing of irrigation and therefore to save water and achieve sustainable agricultural production.

The Editors would like to acknowledge the efforts all of the reviewers who have made comments on this manuscript. Due to an editorial issue during the invitation of reviewers phase, we have received 12 reviewer comments on the submitted manuscript, and an additional two community comments (14 reports to address in total). Invitations for review were sent out to a large volume of reviewers and we had an unusually large number of request for manuscript reviews accepted. Although this highlights the novelty and importance of the work undertaken, it is unrealistic to expect you to address all of these comments in turn.

In light of the editorial issues, I recommend that the Authors reply individually to three Reviewer comments (RC 1, RC 2, RC 3), which request relatively minor revisions. Further, I recommend that the Authors also reply to this Editor comment addressing the specific and general comments which have been summarised and taken from the remaining reviewer comments below (divided into general and specific comments, as well as those relating to the figures/tables).

Generally, most reviewers recommend that a reviewed version should be accepted after minor revisions (with the exception of Reviewers 9 and 12; RC9 recommend work due to some methodological shortcomings, and RC12 recommend rejection of the paper as they do not recognise the added values for predicting future conditions).

Once again, we apologise for the unusually large amount of reviewer comments received, but we hope this solution helps in responding to all of the reviewer’s comments whilst recognising the inputs from all reviewers. Thank you again for submitting your manuscript to this Special Issue in HESS and we look forward to receiving your response.

Kind regards,

Dr. Daniel Green

General comments:

[RC12] First, it is unclear for me if their statistical model has a significant added value for predicting future conditions. Since they found that there is no significant relationship between P and PET (and ET), I guess that the initial condition of ET and PET is the major source of predictability of them (although they did not clarify this point). In this case, the authors may be able to replace their prediction of ET and PET with the persistent model. I think Figure 7 also implies that the persistent model is effective to predict them and it is not absolutely necessary to predict the dynamic change in ET and PET. In addition, the temporal change in soil moisture is also important, but the skill of their model to predict soil moisture is not actually good according to Figure 9. Although the precipitation prediction significantly contributes to the prediction of needed irrigation through equation (1), precipitation prediction comes from the existing data and is not the contribution of this work. In summary, without more detailed comparisons between their prediction and some benchmarks such as a persistent model, I cannot be very convinced that the authors' statistical model really provides an added value.

Second, it is unclear for me how this work contributes to estimating crop water conditions at farm scales since they fully relied on satellite observation with coarse grid sizes. Specifically, the size of the original footprint of SMAP is approximately 50km, which is apparently not a farm scale. Although it might be possible to integrate local information into the authors' proposed framework, I could not find any contributions to farm-scale water resource management in the present work.

[RC9] There are already numerous tools available to predict water demand for crop management. The novelty of this study is for the user to select a specific farm location, which alone is not sufficient for publication. Therefore, the novelty of this study needs to be better explained.

[RC4] There are really a lot of acronyms: please list them with the correspondent explanations at the end. **[RC10]** There are too many abbreviations in the text, and many of them are given without explanation; and it makes reading the text difficult. I would suggest putting all notations into the table in the Annex.

[RC7] The model, for example, produces a KGE for fourteen-day predications of something like 0.4. The authors provide limited discussion of the implications of this performance. What level of uncertainty does that imply? What could be the social and economic costs (crop loss, reduced yield, water costs, etc.)? How does this prediction accuracy vary over the season? How does accuracy vary over the prediction horizon? These questions could be discussed qualitatively (in leaving work for later) or quantitatively (in trying to add more work here). The provide, either way, a more robust understanding of the utility of this model.

[RC8] In my opinion the term "Needed Irrigation" is not appropriately used in this paper and could be misleading. Since FarmCan has been applied to rainfed cropping system, I think that the term "water deficit" (as in the title) is more appropriate.

[RC8] The paper should also clarify how this tool could be practically employed in "near real time": what kind of strategies could be implemented "to minimize potential crop failure and losses" in rainfed cropping systems?

Specific comments:

[RC9] L14: “four” instead of “4”

[RC9] L16-18: This statement already indicates that the ML method was not sufficiently tested.

[RC9] L32-35: It is unclear why and to what extent irrigation demand forecasts are important to rainfed farmers. It is very unlikely that they would change their farm management just because irrigation demand forecasts are available. This statement is also not included in the citations.

[RC4] In the abstract you affirm “...our algorithm was able to forecast crop water requirements 14 days in advance…”: I do not understand why in the rest of the paper (for example figure 7-8) the predictions are up to 10 days.

[RC9] L60: Use consistent capitalization

[RC9] L66: What do you mean by “subfield”? Please consider that the SMAP data has only 36 km resolution.

[RC9] L67: I don't understand why this method is tailored to this area, as the method seems to be generic.

[RC10] Lines 78-79: Please, provide the numbers while describing the climatology of the region (length of winter and summer seasons, [annual/growing period] amount of precipitation, relative humidity, etc.).

[RC10] Line 156: the abbreviation P was previously explained in the text, there is no need to repeat it here.

[RC4] Line 167: there is (?) - please correct it. **[RC9]** L167: There seems to be a citation missing.

[RC12] L165-167: This description is a bit ambiguous for me. I believe that the authors used the forecast of P. Please explicitly say the P prediction was used in this paper.

[RC5] The study area section needs to be more explanatory such as past climatic scenarios which will directly or indirectly affect the crop.

[RC9] L181-185: This procedure is not clear to me. How do you calculate the radii? How do account for the different spatial resolution of the different data sets? Are you averaging over the SMAP grid? How representative is the SMAP soil moisture for a specific field? [RC12] L181: “P” appears twice.

[RC9] L187-189: It is unclear how the extension of the SMAP data to 2010 was done in detail. Furthermore, it is likely that a machine learning method will lead to very uncertain estimates of soil moisture, and I therefore do not see its benefit for the predictive modelling. Either explain in more detail how and why the extension was done, or better leave it out.

[RC9] L190-192: From a viewpoint of a soil hydrologist, it is very strange and arbitrary to first predict RZSM and than use this for the prediction of SM, as SM should be much stronger controlled by P than RZSM due to infiltration processes. Please explain in more detail reasoning behind this. In addition, explain why you predicting SM at all?

[RC9] L195-202: Eq. 2 is not correct. To obtain the correct weights, the result from Eq. 2 must be divided by 800. Furthermore, this procedure is a strong simplification, as it does not distinguish on which day P fell within the 8 days, which makes a big difference in reality. Let us assume, for example, that 100 mm P fell on the first day of the week. In this case, the irrigation requirement for the crops for the following days would be much lower compared to if 100 mm P would fell on the last day.

[RC10] Line 255-256. I would suggest not using the term "climatology" when discussing statistics estimated from the 5-year period.

[CC1] It's a misprint in Line 250, "0.5x1000", I think the correct phrase is "0.2x1000".

[RC9] L284-285: But high PET values can be found also for positive delta values of RZSM and SM for all regions.

[RC9] L292-294: This statement contradicts the time series of soil moisture shown in Fig. 6. If it were true, one would expect soil moisture to decrease continuously during the growing season. However, the delta values of SM and RZSM both show fluctuations around zero, suggesting that P is sufficient to compensate for ET losses. This discrepancy may be due to uncertainties in the PET and ET data.

[RC9] L309: In Fig. 8, M1 and M2 show strong P events of about 30 mm on 1 July. This results in an increase of SM to about 0.5 m^3/m^3 indicating soil saturation. Nevertheless, the model predicts a decrease in ET which is not plausible.

Figures and tables:

[RC4] In general some figure and tables are not very clear. If the information in Table 1 are divide in two parts the two new figures fits horizontally and this improves the readability. Figure 6 contains a lot of information and the colour and the bars confuse the reader: maybe also here is possible to split the figure in two parts. Figure 7-8: I do not understand why you show the "observed variables for farmS2" separately and you don't do this for the other farms; maybe the "growth stage" (it is simply a line) is useless.

[RC5] Figure 1 should include the scale and North direction symbol.

[RC5] Figure 3 explains day 8, PET, ET etc. Why day 8 parameters are important and what about other days. Figure 6 also only describes the 8-day variability. What is the significance of the day 8 events? [This links back to the previous comment about 10 day predictors, versus 14 days in advance]

[RC9] Figure 4: I don't see the benefit for showing the spatial distribution of P, ET, PET, SM and RZSM as this study only concerns time series analysis. Instead, monthly climate diagrams would be very useful to better understand the climate and soil

hydrological situation in the four farms used in this study to develop and test the model. **[RC4]** Figure 4: I would suggest adding the explanation of all abbreviations used in the plots. Also, please remove the term “climatology” from the notation to the figure.

[RC9] Figure 6: Colours for ET and SM not distinguishable.

[RC10] Figure 7: I would suggest rounding predicted values for PET, ET, P and NI: like P=30.46 mm -> P=30 mm since the precision of the modeled value never becomes better than the observed (measured) value. The precipitation is usually measured with the precision of 1 mm; if the precision of the observed precipitation is better than 1 mm, please provide the description of the measuring technique or the reference describing it.

[RC9] Figure 8: There should be no subtitles under the subplots. The plots are difficult to understand because of the large number of symbols. The meaning of the growth stage line is unclear.

[RC9] Figure 9: Font size is too small. NI is not an observed value.

[RC9] Table 1: The source for the crop water needs is missing and not all crops are covered. The values for P are much too low (in the text values between 400-1100 during the vegetation period are given). The values of P/PET seems to be too low as well. **[RC10]** I would suggest providing the numbers for the precipitation rounded to tens of mm (i.e 122.45 -> 122).

References:

[RC9] Reference: Taghvaeian, S., Andales, A. A., Allen, L. N., Kisekka, I., O’Shaughnessy, S. A., Porter, D. O., ... & Aguilar, J. (2020). Irrigation scheduling for agriculture in the United States: The progress made and the path forward.

[CC2] mentions some useful references which may also add to the discussion/context of the work undertaken and some of the key governing principles.