

Hydrol. Earth Syst. Sci. Discuss., community comment CC1  
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## Comment on hess-2022-96

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Community comment on "FarmCan: a physical, statistical, and machine learning model to forecast crop water deficit for farms" by Sara Sadri et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2022-96-CC1>, 2022

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The authors address an important issue, and the topic deserves more attention in the hydrological community. The article is generally well written, and I recommend the editor to publish the manuscript after some minor corrections and clarifications. I've not worked through the other comments to this article, and I expect there will be some overlapping viewpoints and suggestions. Hence, below follows my independent comments.

### General comments:

The manuscript would benefit of a clearer distinction between modelling results, discussion, and conclusions. As it is now, the conclusion sounds more like a discussion. For conclusions in a scientific paper, you are not supposed to "... speculate ..." (line 341), or "... expect ..." (line 343). If the results are unclear or uncertain, it should be elaborated in the discussion. The phrase "FarmCan can become a promising tool ..." (line 352), also belongs to the discussion. What are the necessary requirements for FarmCan to be successful? Or why can FarmCan fail to meet the requirements? What are the critical factors? Everything written in the conclusions should be evident from the results and the discussion. The abstract should be consistent to the conclusions. The current abstract states that: "FarmCan is a promising tool for use in any region of the world ...", but this is not the same as written in the conclusions.

As far as I can see, the work is based on estimated data derived from remote sensing (Tab.2, line 131). The selected farms were parts of a network of field stations for soil moisture observations (line 82-86), but it is not clear to me whether validation data is empirical observations taken from the selected farms or extracted from remote sensing data. Is empirical data included in Fig.6 (for example) or not? If not, it should be written explicitly in the text and explained why, and also elaborated in the discussion.

I would also expect some kind of cross-validation exercises in a study like this. As far as I understand, this is in principle done in Fig.7 and 8., where simulation results after the given date (2020/07/02) is illustrated. But I can not see how these results fits the observations. The cross-validation results are more easy to understand in Fig.9, but the results need a more through discussion.

In general, I would also recommend the authors to elaborate the main results in the figure texts. What specific results (or features) should the reader appreciate in the figure?

The definition of Potential Evapotranspiration (PET) needs clarification to avoid ambiguities. FAO recommends to substitute 'PET' with 'Reference Evapotranspiration', which I support. In this case, the authors use pre-calculated data, but the term should be defined explicitly so that others can reproduce the results.

Specific comments:

Equation 1 (line 110) needs some clarifications. Why is NI approximate equal to the right hand side of the equation? I understand it is sum over 8 days, but that should also be evident from the equation. I guess the sum also go over  $\Delta SM$ ? That needs also to be written explicitly. I would also appreciate if  $\Delta SM$  was defined explicitly because a negative  $\Delta SM$  will increase NI.

In Tab. 1 it is written that the units for (P/PET) ratio is mm/day, but this is (probably?) a misprint. Units for P and PET is [Length/Time], thus the fraction must be unit less [-].

Figure 6. The first y-axis label says  $\Delta SM$  and  $\Delta RZSM$ . The legend says SM and RZSM, the figure text says  $\Delta SM$  and  $\Delta RZSM$ , please correct the legend. PET looks more like "teal green" than P, which is green bars (same color as in Fig.7).

Figure 5. What is the "take away" message of this figure? It's said that  $\Delta SM$  is plotted against 8-days P, but it is also data for  $\Delta RZSM$ . Please, also consider elaborating the figure text (c.f. general comments above).

Figure 7. Why does past NI decrease from approx. 9 mm/d to approx. 1 mm/d from June 25<sup>th</sup> to 26<sup>th</sup>?

It's a misprint in Line 250, "0.5x1000", I think the correct pharase is "0.2x1000".