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Hydrology and Earth System Sciences Discussions

Interactive comment on "Simulation of the soil water balance of wheat using daily weather forecast messages to estimate the reference evapotranspiration" *by* J. Cai et al.

Anonymous Referee #2

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Review of Simulation of the soil water balance of wheat using daily weather forecast messages to estimate the reference evapotranspiration by J. Cai et al. Published in HESSD 6 (2009):697-728.

The paper investigates the suitability of public weather forecasts to estimate the reference evapotranspiration for use in real-time irrigation management support models. It reports a test of the approach on a limited set of field data.

Major comments.

The paper is generally well organized, except for the description of the calibration that

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Interactive Discussion
Discussion Paper

appears in the Results and Discussion but which I would prefer to see in Material and Methods. The English is mostly understandable, but not good enough for final publication (particularly the word ‘ relative’ is used in error on many occasions). I recommend that the authors consult an English editor.

The Materials and Methods section is inadequate: we learn hardly anything about the soil, the local climate, the layout of the fields, or the experimental set-up. Also, the focus on various statistical indicators of the goodness-of-fit seems to be unbalanced. The authors present very applied research, yet offer nothing in their assessment of their approach from which it can be judged if the approach did what it was supposed to do: allow a farmer to improve his irrigation scheduling without having to install a full-fledged weather station. Criteria that come to mind are amounts of irrigation water saved, yield improvement in good and bad years, reduced leaching requirements while still transporting the salts below the root zone, etc. This omission naturally spills over in the Discussion: the results are discussed according to the various statistical criteria but nothing is being said about practical implications.

Another serious concern is about the scope of the experiments. While the experimental methodology used seems OK (although it is not reported in sufficient detail to verify this) I find the study rather limited. The authors set out to investigate whether standard weather predictions for the public can be of use for irrigation scheduling (p.700, l. 14-21), yet the authors consider only one model (with which I am admittedly unfamiliar, and which does not seem to be widely used) in their test. More seriously, the entire field test was carried out on a single soil, on perhaps a few small fields (not clearly reported) very close to one another, with a single crop and only two observation years. Furthermore, the irrigation regimes were such that the water content varied within a rather narrow range. Also, all reporting is based on observed and modeled water content (but it is not clear at what depths). Irrigation scheduling typically is used to either maximize yields or water use efficiency (in terms of yield per volume of irrigation water), while minimizing salinization risks (the latter is admittedly of little concern if there is enough

HESSD

6, S92–S96, 2009

fresh water available since the groundwater is very deep). The paper mentions none of these aspects.

I therefore believe this paper has too little substance to warrant publication – it is a useful initial step in a full study, but more work needs to be done to convince the readership that a viable use of public weather forecasts has been found. It its current form it presents an incremental advance of model-based irrigation scheduling.

Detailed comments.

Please give the dimensions of the variables on first use.

There are inconsistencies in denoting variables in italics or regular fonts throughout the text.

The figures are so incredibly small that I had to use a magnifying glass and still had a hard time reading them. This obviously needs to be improved.

Add 1:1 lines to the regression figures.

The crop/irrigation model used takes a central role in the study, yet the reader is referred to the references for a (full) description. You could at least present the basic principles and equations of the model and cite earlier work for the details, especially since some of the relevant literature is not widely available.

p. 698, l. 19-20. Volumetric water content is not an appropriate unit for a water balance term.

p. 699, l. 2. Irrigation management (and management in general) always involves real-time decision making.

p.700, l. 8. on maximum -> on daily maximum

p.702, l. 8. I am an outsider in this field; please give a reference for the Angstrom equation.

HESSD

6, S92–S96, 2009

p.702, l. 15-19. Please elaborate on the way water influences the air mass

p.703, l. 11. field capacity is a dubious concept. I suspect you mean the water content at a particular matric potential. Please give details, since the reference matric potential used varies between countries. Is the wilting point defined at a matric potential of -1600 kPa?

p.703, l.26-27. Unclear, please rephrase.

p. 704, l. 2. Please give more details about the TRIME equipment.

p. 704, l. 21-24. This is hectic: several undefined variables appear, dimensions are missing, it is unclear over what period of time the inputs are defined, etc.

p.705, l. 8-9. I think you mean water storage instead of content.

p.707, l.14-15. Can you give the equation?

p.705, l. 19 and other occurrences. I do not understand why you forced the regressions through the origin. This masks bias and renders the correlation coefficient meaningless.

p. 707, l. 5. 1’0 ?

p.708, l. 1-11. You bring the comparison with another study into the discussion, but why you do so is not entirely clear. After reading this, I still do not know if your new method is reliable. The RMSE is the criterion with which I have most experience, and by comparing its value with your observations, I would say the WF estimates are not so bad. I am more concerned about the significant overestimation in 2006-2007 (Fig. 3b) and the less severe underestimation in 2005-2006 (Fig. 3a), but you do not address that.

p.708, l. 12-21. This seems to suggest that the weather station data are incorrect. If so, please elaborate. Also, I do not see how differences between observations lead to better or worse predictions by methods relying on data other than those observed. It

HESSD

6, S92–S96, 2009

Interactive Comment

appears to me that differences between different observation techniques demonstrate an inability to correctly measure a given quantity, which makes it harder to test methods to estimate that quantity. Finally: for the non-meteorologists you may want to explain the difference between a synoptic and a non-synoptic weather station.

p.708, l. 25. Delete select

p.708, l. 26. Define the depletion fraction.

p.708, l. 23-p.709, l.3. Move to Materials and Methods

p.709, l. 1. How did you correct for climate?

p.709, l. 4-14. You explain very little here. Which water contents did you use? Those near the bottom of the root zone possibly did not vary too much.

p. 709, l.15-24. I gave up on trying to read the figures here.

p. 709. Section 3.3 This is not that interesting, there is no indication of the true performance or potential of your approach (see the general comments).

p. 711. Conclusions. Some of the conclusions are rather bold, given the fact that the tests involved only one soil, one crop, and two years, and were carried out on a small area.

p. 712, l. 8-10. In the Introduction you stated that remote sensing can provide data on the larger scales. You seem to contradict that here.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 697, 2009.