

## ***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.***

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Received and published: 23 April 2009

Reply to the interactive comments by the Editor and the referee #4

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.

G. Blöschl (Editor) bloeschl@hydro.tuwien.ac.at Received and published: 11 April 2009

Reviews have been provided by referee #1 (also termed #3) and referee #2. An addi-

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tional review (#4) is provided below.

The judgement of referees #1 and #2 is very different. Referee #1 is supportive of the paper and has only a small number of minor suggestions for improving the manuscript. Referee #2 has scrutinised the paper in more detail and is more critical of it. He/she notes that the material and methods section does not provide sufficient detail about the soils and that the model should be tested against the practical implications of irrigation scheduling such as irrigation water saved by using the model. He/she also states that the study is rather limited (a single soil and a single crop) so the claims of the general suitability of the method are unfounded. Referee #4 requests a justification of why weather messages are used in the first place and points to the limited applicability of the study.

Referee #2 is perhaps a little harsh but his/her points are fully valid. The comments have partly been addressed in the author comments. I would like to have them fully addressed in the revised paper. The study is indeed more limited than what is claimed, both in spatial scope and what it achieves in practical terms. Rmse does very little for the farmer. Extending the study to include irrigation scheduling based on the soil water model would perhaps go beyond the scope of the paper. I rather suggest that the authors are clearer about the limited scope of their analysis.

Reply: we do not understand what has not been responded to reviewer #2. We suppose that he/she assumed that the model requirements were similar to those for a flux model. However, requirements for water balance simulation are different and simpler than for a flux model. Thus the scrutiny performed is often out or marginal to the objectives and content of the study. However it was clearly indicated the type of model used, respective requirements and consequent calibration and validation.

The study bases upon 15 years of past studies that involved climate/weather, particularly the reference evapotranspiration (ET<sub>o</sub>), soils, crops, and models applied to the area focused in this study and with various applications to the North China region. The

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irrigation schedules developed years ago from research developed by the authors, e.g. exploring the irrigation scheduling model used in this study, are now being applied by the farmers advisers in the area. Bibliography is added on these matters.

The treatments adopted in this study include these schedules and several alternative ones. The practical implications are now well known. The idea beyond the paper is to produce further weather information where it is not yet available in the region using weather forecast messages (WF) instead of installing a dense network of weather stations. In the paper Cai et al. (2007) it was observed that WF provide acceptable estimates of ETo for synoptic stations. In this one we intend to show that using ETo estimated from WF for a non-synoptic location provides acceptable estimates of the soil water dynamics when using a calibrated/validated irrigation scheduling simulation model. Further research is being developed for spatializing WF and ETo information when using the irrigation scheduling model operating in a GIS framework. This last step of research is being developed and needs the results of the present step dealt with in this paper. This will be better clarified in the paper.

Why should we do the experiments with a second crop if the main irrigated one is wheat? We could find a horticultural crop to have a second one but they represent very little in terms of water economy. Using a summer crop would be possible but all efforts are oriented to have them just rainfed.

Why to use more than one model if the intention is not to compare models? The selected one is largely tested in China and was improved for capillary rise and percolation estimation in North China when using it in combination with the flux model WAVE; hence, there is no need to use a second model for irrigation scheduling purposes. Moreover, there is experience on using it with a GIS data base (Fortes et al., 2005), which is the intention of the research group as a later step of this research. The Lisbon group just produced another irrigation scheduling model but it is aimed at partial cover crops despite it may be used for cereals. Maybe, using past data and data of this research we will compare the models, but this is not opportune for the objective of the

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[Interactive Discussion](#)

[Discussion Paper](#)



present study.

We do not understand the sentence "RMSE does very little for the farmer";. The fact is that developing irrigation schedules with a calibrated/validated model where estimations produce small RMSE, not large RMSE, are supposed to provide reliable information for farmers. Thus, we may assume that RMSE indirectly does something for the farmers.

The sentence "Extending the study to include irrigation scheduling based on the soil water model would perhaps go beyond the scope of the paper"; is unclear because the model aims at irrigation scheduling and the treatments refer to various alternative schedules applied in the field. We suppose that what would go beyond the scope is to apply the model in the farmers practice.

Moderate revisions of the paper are needed and the revised manuscript needs to be re-reviewed. I trust that the authors are able to fully address all the review comments in the revised paper, in particular those of reviewer #2.

Günter Blöschl

REFeree #4 Review of Simulation of the soil water balance of wheat using daily weather forecast messages to estimate the reference evapotranspiration By Cai et al.

This is a very applied paper in which the procedure of Cai et al. (1999) is used to estimate reference evaporation from weather message proxies to drive a soil water balance model. The water content simulations are then compared with simulations that use weather station data as inputs. While the paper probably does not further the hydrological sciences much it may be useful for readers with an interest in soil water management in data scarce situations.

The paper is reasonable well written and the overall message is clear.

There are two general concerns I have with the paper though. First, why use weather messages? Weather messages are generally prepared from the output of numerical

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Discussion Paper



weather forecast models. These forecast models resolve the land surface energy balance. Forecasts of the energy balance and evaporation hence exist. So why would one back calculate evaporation from the proxy information of the messages rather than directly use the weather model forecasts?

Reply: This question is strange: Would we try this approach if evaporation forecasts would be available to the public? Of course not. Would we produce the paper Cai et al. (2007) for testing ETo estimates from WF using data for eight synoptic stations supplied by the China Meteorological Administration (CMA) if this institution would provide that information for the public? Of course not, mainly considering that appropriate contacts were established with CMA with this purpose. If this approach is used is because it is required. (Otherwise, we suppose that the Journal of Hydrology would not publish that article).

Second, the paper is overly optimistic about the usefulness of the results. If one takes the conclusions (p. 698 l. 24, p. 711 l. 14, p. 712, l. 3) at face value one gets the impression that the proxy information of weather forecast messages could replace the numerical forecasts of evaporation - world wide and at all times. This is not supported by the data and is hardly what is intended by the authors. The authors need to make clear that use of the proxies are a last resort if no quantitative forecasts are available. They seem to work well for the case examined but it is hardly a method that can be generally recommended. The way the usefulness of the results is promoted is not appropriate for a scientific paper.

Reply: Following the precedent question and respective reply, the fact is that since daily numerical forecasts of evaporation or evapotranspiration are currently not available to the public (of course appropriate contacts were established with CMA) the weather forecast messages constitute the best source of information to compute reference evapotranspiration. This is not a question of being or not optimistic but of feasibly using the available real time information. In other countries the situation may be different. Maybe this situation will change in future, and maybe it will change because of the

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approach being developed at present

The English needs some streamlining. I have highlighted a number of changes that are needed below, but there are many more instances in the text.

Minor comments (p=page, l=line)

p. 698 l. 6: Penman Done

p. 699, l. 5: irrigation is not predicted by the models - irrigation is a human decision. rephrased

l. 16: relative? rephrased

p. 700, l. 7: Goodness is not a scientific term. Avoid promotional phrases. Rephrased. However, what is promotional when saying that a given equation is good? The papers where this equation is presented are quoted in 20 papers of HESS journal and are quoted hundreds of times each year in many other journals. There is no need for promotion.

p. 701, l. 16: messages using daily rephrased

p. 702, l. 19: be clear how they obtained kRs. Did they estimate the factors or assume them? Rephrased to explain that kRs was previously tested by the authors

pp. 701-703: Importantly, be clear on whether any of the ET model parameters were calibrated for the Daxing station. Parameters of FAO-PM equation do not need local calibration. See Allen et al. (1998) and some among the thousands of papers referring to its use

p. 703, l. 9: for the winter rephrased

l. 13-15: This is repetitive with p. 705, l. 14, so remove either of the two sentences. Rephrased in both places

p. 704, l. 3: dried in the done

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l. 5: use past tense done

l. 6-13: This paragraph sounds as if it were lifted from another paper. Relate it to section 2.1 of this paper. Rephrased in both places

l. 19: in detail done

p. 707, l. 14,15: forecast, lead done

p.708, l. 16: ‘may be used’; is another example of a promotional, overly optimistic statement rephrased

p. 711, l. 1-2: Another example where the conclusion is not supported by the results of this paper. This is simply too general and clearly not always true. rephrased

p. 717: I had to guess what Table 2 means. The numbers in brackets are probably dates which should be explicitly stated. Say what W1, W2 etc. are. They are not treatments (which is the process of irrigating the sites), they are different plots, from what I see. A Table footnote is used. W1, W2 etc, are described in section 2.2 and in Table 2. These are treatments, not plots. Each one refers to a different irrigation schedule (or process of irrigating as said by the reviewer). Results relative to each treatment refer to 6 observations by treatment

pp. 720 and 721: Replace treatment by plot (four times) These are not plots but treatments (see section 2.2 and Table 2)

p. 722: Give units in right panel of Fig. 1 done

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

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