Hydrol. Earth Syst. Sci. Discuss., 6, S348–S362, 2009
www.hydrol-earth-syst-sci-discuss.net/6/S348/2009/

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.

J. Cai et al.

Received and published: 16 March 2009

The reviewer made very demanding comments that require both proper replies and numerous improvements in the manuscript. Replies are given below and are numbered for easier identification. Text changes will be performed when all reviewers comments will become available.

1 - The paper is generally well organized, except for the description of the calibration that appears in the Results and Discussion but which I would prefer to see in Material and Methods.

Reply: there is no description of the calibration in the Results Section but the presentation of the calibration and validation results. These results should remain in the Results

Section.

2 - 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.

Reply: a final revision of the text will be performed after receiving all comments. The words &#8216 and &#8217 are not identifiable

3 - 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.

Reply: The essential information on soils and climate for the purpose of the soil water balance studies are given in Section 2.2 and in Table 1 and Figures 1 and 2 similarly to other publications referring to soil water balance (e.g. Popova et al., 2006; Cholpanulov et al., 2008; Pereira et al., 2009). Soils and climate have been formerly studied in detail e.g. Pereira et al., 1998, 2003; Ding, 1998; Xu and Mermoud, 2003). Other references to North China Plain (NCP) could be considered ()References will be added The experimental set-up is described lines 22-27 of page 703. Indicators of the goodness-of-fit are those already used in former studies (e.g. Popova et al., 2006; Cholpanulov et al., 2008). Further references will be added.

4 - 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.

Reply: The meaning of this comment is not fully understandable because the paper focus exactly the fact that using weather forecast messages provides information required for irrigation scheduling with an accuracy similar to that relative to the use of a local weather station. Results in Table 6 show that RMSE when forecast messages are

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used range 0.007 to 0.013 m3 m-3, thus being very low and similar to those referring to the use of observed weather data, ranging 0.003 to 0.010 m3 m-3 (Table 5). The relative error is 4 to 5% in first case and 3 to 4% for the latter. Contrarily to the reviewer, we are sure that these results demonstrate that the approach is appropriate for irrigation scheduling. However, it cannot be used by a single farmer because the farm size in the area is between 0.5 and 1 ha and therefore the information has to be used by a farmers advising service.

5 - 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.

Reply: The problem focused is not water saving but to adopt an irrigation scheduling that supports an optimized irrigation taking into consideration the constraints imposed by the irrigation systems. Former irrigation scheduling studies were developed after previous experiments adopting the appropriate wheat varieties (e.g. Liu et al., 1997, 2000, 2004, 2005; Fernando et al., 1998; Liu and Pereira, 2003,). Water saving results not from irrigation scheduling alone but together with improvements in basin irrigation systems (Pereira et al., 1998; 2003; Fernando et al., 1998; Liu et al., 2000, 2004). Salinity is not a problem in the area where the monsoon rains provide for natural leaching (Pereira et al., 1998; 2003). Selected references will be added

6 - 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.

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Reply: We never saw using two or more water balance models with the same data except when it is intended to compare models. Using one model that is purposefully calibrated and validated is definitely appropriate. The authors have formerly used the soil water flux model WAVE (Vanclooster et al., 1994) for appropriate identification of soil water behaviour under rainfall and irrigation (Pereira et al., 1998; Xu and Mermoud, 2003; Liu et al., 2006) but after analysing results and model requirements decided to use ISAREG for irrigation scheduling purposes. The authors know well that the selected model is appropriate for irrigation scheduling as reported in several applications in several parts of the world. Numerous applications to China (Liu et al., 1998, 2000, 2004, 2005; Fernando et al., 1998; Liu and Pereira, 2003; Campos et al., 2003; Pereira et al., 2007) and elsewhere (Cancela et al., 2006; Сholpankulov et al., 2008; Fortes et al., 2005; Pereira et al., 2009; Popova et al., 2006; Popova and Pereira, 2008; Oweis et al., 2003; Rodrigues et al., 2001; Teixeira et al., 1995; Victoria et al., 2005; Zairi et al., 2000;) may be used to better inform about the model utilization. Selected references will be added.

7 - 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.

Reply: Past studies developed in the North China Plain have shown that soils have relatively similar characteristics since all the area of interest is formed by deposits of the same loess formations that origin silty soils. Consequently the soil hydraulic properties relevant for water balance vary little in that area (Pereira et al., 1998, 2003; Liu and Pereira, 2003; Ding, 1998; Xu and Mermoud, 2003). Selected references will be added. The main crop irrigated in the area is the one considered, wheat. Summer crops are generally not irrigated because monsoon rains are sufficient for most cases. More than two years experimentation are used when there are discrepancies in results for the first two years, which did not occur in this experiment; hence there is no justification for the human and capital investments required for a third year of experimentation.

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8 - 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).

Reply: Experiments were not intending to induce large water stress that could cause yield losses because farmers in the area farm only 0.5 to 1 ha and can not afford such yield losses; therefore the soil water content could not be allowed to decrease much below the readily available water and, of course, not be above field capacity. Further information on irrigation regimes simulation is referred in studies quoted under item 6. Depths of observations are clearly referred in lines 2-3 of page 704.

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

Reply: The paper does not mention the first aspects because the analysis of yields was not the purpose of this paper. However they were observed and averaged 5198 and 7470 kg/ha for 2006 and 2007 respectively, while water productivities were 2.02 and 2.69 kg/m3 for the same years. These yields are similar to those obtained in previous experiments (Pereira et al., 1998). Relative to groundwater use, it was not mentioned because it is not a problem in this experimental area despite exploring the existing shallow groundwater requires appropriate control as defined in former studies (Pereira et al., 1998 and Randin et al., 1999). Additional information is available (Zhao et al., 2004. Nakayama et al., 2006; Wang et al., 2009). References will be added.

10 - 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.

Reply: We deeply disagree: a former study (Cai et al., 2007) has shown that estimating

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the reference evapotranspiration from weather forecast messages is feasible; this second paper shows that using that information instead of that provided by a local weather station is feasible. Results in Table 6 show that RMSE ranges 0.007 to 0.013 m3 m-3, thus being very low and similar to those referring to the use of observed weather data 0.003 to 0.010 m3 m-3 (Table 5). The relative error is 4 to 5% in first case and 3 to 4% for the latter. Contrarily to the reviewer, we are sure that such small errors definitely show that the approach is viable In other locations in China and elsewhere results may be different and further assessments are required, This fact does not invalidates the interest of this study which may constitute a first step that may be followed by other researchers in China or elsewhere. Moreover, because it is the first study on this subject, it opens alternative approaches for model-based irrigation scheduling not requiring to purchase, install, explore and maintain local weather stations since weather forecast messages provide for real time data at a much lower cost.

11 - Please give the dimensions of the variables on first use.

Reply: The text will be revised after receiving all comments

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

Reply: The text will be revised for formatting rules

13 - 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.

Reply: Fortunately in our computers making a zoom on the figures they become easily readable

14 - Add 1:1 lines to the regression figures.

Reply: this is not appropriate because the regression lines are very close to 1:1

15 - The crop/irrigation model used takes a central role in the study, yet the reader is

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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.

Reply: A description will be added. Literature quoted for this purpose refers to journals or books from recognized publishers

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

Reply: The word “balance” may be replaced by “content” in that line

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

Reply: the sentence may be modified: “Recent developments in irrigation management consist in tools to support real-time irrigation decision making”.

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

Reply: not necessarily daily since various time step computations may be used, from the hour to the month

19 - p.702, l. 8. I am an outsider in this field; please give a reference for the Angstrom equation. S94

Reply: Angstrom A, 1924. Solar and terrestrial radiation. Quart. J. Royal Meteorological Soc. 50: 121-125. This will be added to the text

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

Reply: Air temperature over a large water body (such as an ocean) is lower than above land (not a small island) and air humidity is higher, thus, due to advection, the air masses over land near these large water bodies has also characteristics different from

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air inland. This is not required to be added but text will be improved

21 - 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?

Reply: Field capacity is a well accepted concept and is defined as the bulk water content retained in soil at −33 J/kg (or −0.33 bar) of hydraulic head or suction pressure.The wilting point is defined as the water content at −1500 J/kg (or −15 bars) of suction pressure.

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

Reply: Irrigations were applied to refill the soil water to a level not exceeding field capacity.

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

Reply: This is a soil moisture measurement device using Time Domain Reflectometry TRIMEő-T3/IPH (http://www.imko.de/ENG/)

24 - 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.

Reply: This is not a hectic question. This is an enumeration of data used as input, not an information on the variables used. Indications on units and period of time are given in the model manual. Since references are given and these data are commonly known from readers using this kind of models, to save space related details were not presented similarly to other papers using the same model (e.g. Cancela et al., 2006; Сholpankulov et al., 2008; Pereira et al., 2007, 2009; Popova et al., 2006; Popova and Pereira, 2008). However, for clearness, further information will be added.

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

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Reply: storage will replace content

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

Reply: I suppose that the comment refers to page 705. If so, this is a set of equations whose description and definition of related variables require a lot of space. However, because not used in this application, and full details are provided in a widespread Journal (Liu et al., 2006), these information do not require to be added

27 - 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.

Reply: a linear regression is used to study how a variable relates to another, or how a variable explains the variation of the other. In here the simulated values are not to be explained by the observed ones but compared. Observed and simulated variables would be equal if all points would fall on the 1:1 line. The regression parameter of interest is therefore the regression coefficient, not the correlation coefficient. Because of that the correlation coefficient is computed differently from a linear regression non-forced to the origin

28 - p. 707, l. 5. 1’0 ?

Reply: not possible to identify this query. But there is an error in this line produced when the published version was prepared and that we did not noticed: 1’0 instead of 1.0

29 - 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

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that.

Reply: This study is a continuation of the former (Cai et al., 2007) hence results on estimating ETo are compared with the former one. The text needs clarification according to the reviewer comments. The reason relates to the fact that forecasts are made for regional weather and not precisely to the location where studies are performed. See reply to comment 30.

30 - 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 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.

Reply: the synoptic stations are those explored by the China Meteorological Administration to provide information on conditions of the atmosphere or weather as they exist simultaneously over a broad area and where observations are made at periodic times (usually at 3-hourly and 6-hourly intervals specified by the "World Meteorological Organization), The weather synoptic observations are used to provide for the weather forecasts at the same locations. Therefore when extrapolating these forecasts for non-synoptic weather stations, as it is the case in this study, it is expected that the forecasts will over- or under-predict the weather variables differently from the synoptic stations. Therefore, it is not a problem of inability of measuring but of different reliability of forecasts. This is the reason why the comparison is made, not only in terms of the computed ETo but also in terms of differences in results when ETo is an input to a water balance model. In this study we are less interested in understanding the deviations in ETo forecasting than in understanding how these deviations may decrease the accuracy of soil water predictions. The text will be improved to clarify these aspects.

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31 - p.708, l. 25. Delete select

Reply: OK

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

Reply: the term was used without definition because it is often used in soil water balance and crop water requirement studies. It will be defined in the revised text (as well as other commonly used model input variables)

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

Reply: We fully disagree because calibration and validation of the model are, definitely, very important results of the study

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

Reply: a sentence and a reference (Allen et al., 1998) are added in the revised text

35 - 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.

Reply: Of course, it has to be the water content integrated to the entire root zone. What else could it be? Nobody would do such water balance referring to the bottom of the root zone.

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

Reply: It would be enough to click on the zoom button!

37 - 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).

Reply: When the reviewer did not click the button for zooming the figures he(she) had difficulties to find results of interest since images were small. However, results presented in lines 7-22 of page 709, in Table 6 (in addition to Figures 6 and 7) clearly indicate the goodness of results achieved. The reviewer says to be more used to the

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RMSE indicator; however he(she) did not see as a good performance a RMSE of 0.07 to 0.012 m3 m−3 in predicting the soil water content, which corresponds to a relative error of only 4 to 5 %. It may not be excellent but it is definitely good and we do not understand which criteria the reviewer used to do not recognize that potential performance.

38 - 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.

Reply: These aspects are commented above about various strange judgments of the reviewer as for 37 above.

39 - 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.

Reply: remote sensing is not dealt in these lines!

References: Allen, R.G., Pereira, L.S., Raes, D., Smith, M., 1998. Crop Evapotranspiration. Guidelines for Computing Crop Water Requirements. FAO Irrig. Drain. Paper 56, FAO, Rome, 300p. Campos AA, LS Pereira, JM Gonçalves, MS Fabião,Y Liu, YN Li, Z Mao, B Dong, 2003. Water saving in the Yellow River Basin, China. 1. Irrigation demand scheduling. Agricultural Engng Intern Vol. V (http://www.cigrjournal.org/index.php/Ejounral/article/view/403).. Cancela J. J., Cuesta T. S., Neira X. X., Pereira L. S., 2006. Modelling for improved irrigation water management in a temperate region of Northern Spain. Biosystems Engineering 94(1): 151-163. Сholpankulov E. D., Inchenkova O. P., Paredes P., Pereira L.S., 2008. Cotton irrigation scheduling in Central Asia: Model calibration and validation with consideration of groundwater contribution. Irrig. and Drain. 57: 516–532 Ding KL. 1998. An investigation into the effects of soil management on soil properties and crop growth in the Huang-Huai-Hai Plain in North China. PhD dissertation, Silsoe College, Cranfield University. Fernando RM, Pereira LS, Liu Y, Li YN, Cai LG, 1998. Reduced

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