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Hydrology and Earth System Sciences Discussions

Interactive comment on “Multi-objective calibration of a surface water-groundwater flow model in an irrigated agricultural region: Yaqui Valley, Sonora, Mexico” *by* G. Schoups et al.

G. Schoups et al.

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We would like to thank the two anonymous reviewers and the editor for their constructive comments. In the following we reply to each of the comments. For convenience we have repeated the original comments, each of them followed by our reply.

Reviewer #1 ———

1. P. 16, L4. "... Kv for were estimated..." seems to need revision.

“for” was removed in the revised manuscript.

2. P. 18. Formula (12) for root mean square error differs from the one commonly used, e.g., in statistics. The sum of nk summands is actually divided by nk squared, meaning that, in the general case, the estimated RMSE decreases with increasing nk, while

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the quality of approximation may not improve at all. Is there any reason to write (12) like that or is this just a misprint? It should be mentioned though that the use of the conventional expression with the entire sum divided by nk will not affect the results in this case since RMSE estimates with different nk are not compared with one another in the Pareto procedure.

This is a typing error. The formula used in our analysis divides the squared sum by nk (ie the mean squared error) and then takes the root (ie root mean square error). Eq. (12) was changed in the revised manuscript.

3. P. 27, L23. "serous" -> "serious

This was corrected in the revised manuscript.

4. P. 27, L23. A period after "and" within a sentence.

This was corrected in the revised manuscript.

Reviewer #2 ————

1. The abstract should be slightly extended by including a short (2-3 sentences) description of the modelling approch.

The following was added to the abstract: “The model simulates three-dimensional groundwater flow coupled to one-dimensional surface water flow in the irrigation canals. It accounts for the spatial distribution of annual recharge from irrigation, subsurface drainage, agricultural pumping, and irrigation canal seepage.”

2. Evapotranspiration does not belong to the discharge mechanisms - please correct the sentense in the abstract.

With “discharge mechanisms” we mean any process by which irrigation water applied at the surface is removed from the system, so evapotranspiration does belong there.

3. Time step of the model (p. 2068) should be clarified: is it 1/10 of a year?

We added additional comments that clarify the time steps used in the model: “Each stress period was divided into 10 time steps, using a fixed time step multiplier equal to 1.2. At the start of each annual stress period the initial time step in the model is initially on the order of a week and increases each time step by a factor of 1.2 throughout the year. The model was tested with much smaller time steps (using initial time steps less than 1 s) and essentially the same results were obtained for the simulated water balance and aquifer heads. In addition, water balance errors were always less than 0.01%. Therefore, the degree of temporal discretization was sufficiently accurate and led to reasonable computational times for the 3,000 calibration simulations.”

4. Please explain, what means: "unlined" canals?

“Unlined” canals lose water by seepage to the groundwater system, as explained in the text in section 2.2.4: “Since the irrigation canals are unlined, significant amounts of water (up to 500-600 MCM annually) are lost by seepage to groundwater before canal water reaches the end of the canals.”

5. p. 2070: ’Evaporation from the canals was negligible’. Please include the estimation of the evaporation from the canals.

We added the following to the revised manuscript: “Direct evaporation from the canals constituted less than 0.5% of the monthly seepage losses and was neglected.”

6. Please define the relative evaporation (p. 2073).

We added the definition of relative evaporation to the revised manuscript: “Relative evaporation is the ratio between actual and potential evaporation”.

7. Numbering of the Figures should correspond to their mentioning in the text.

We fixed the numbering, in particular we interchanged figures 4 and 5, i.e. in the revised manuscript Fig.4 becomes Fig. 5, and vice versa.

8. Figure 6 mentioned on p. 2077 should be Figure 4?

Yes, with the new numbering (see previous point) this was corrected to Fig. 5.

9. Please correct the legend in Fig. 11, the first pie chart: compare the current legend with the text on p. 2084.

Instead we modified the text describing Fig.11: "Figure 11 shows pie charts of the simulated time-averaged (1974-1997) annual water balance. The two main sources of water inflow into the groundwater system are, first, irrigation water applied on agricultural fields and seepage from secondary irrigation canals, termed “infiltration“ in Fig. 11 (2445 MCM/year or 84% on average), and second, seepage from the main irrigation canals (454 MCM/year or 16%)."

10. Please discuss in the Conclusions the model trasferrability to other regions: is it possible or not.

We added the following to the conclusions: “Although the model was developed specifically for the Yaqui Valley, our results are relevant to other irrigated agricultural systems with shallow water tables.”

Editor’s comments ——————

1. Both in the Abstract and in the Conclusions sections, the authors write that the developed model could be used “in future work to identify optimal groundwater management strategies.” This conclusion sounds as if the model performance was tested additionally to the calibration experiments. However, the presented results do not allow the authors to make such a conclusion. The test of the model through a calibration procedure, even though the latter is very detailed and careful, does not permit one to evaluate the capability of the model beyond the conditions of the calibration period. The results of the model validation by the data, which are not utilized in the calibration procedure, should be shown for such an evaluation. Otherwise, it should be stressed in the paper that the model validation is not carried out in the current stage of the study and, consequently, the conclusions should be more moderate.

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We agree that this conclusion is over-confident given that we have not presented an independent validation of the calibrated model. Therefore, we have removed it from the Abstract and Conclusions. Instead we have added the following to the conclusions: “Future work will focus on an independent validation of the calibrated model, such that the model can be confidently used to identify optimal pumping strategies in the Yaqui Valley.”

2. It would be interesting for a reader to obtain information on the uncertainty of the simulated water balance components in the Section 3.2.

This is a good point. We have added an extra table (new Table 4; old Table 4 became Table 5) which lists the water balance components for the various objective functions. We also added the following discussion to the text: “In order to get a sense of the uncertainty of the simulated water balance, Table 4 provides time-averaged water balances for the four objective functions of the multi-objective optimization. It is clear that most variation occurs in the drainage, evaporation, and canal seepage components. For example, when minimizing RMSEwt more water discharges by evaporation (720 MCM) with very little drainage (86 MCM). Note that the compromise solution (minimum Euclidean distance) simulates a similar amount of drainage as the RMSEdrain objective, and a similar amount of canal seepage as the RMSEseep objective.”

3. It is not clear enough from the text, what is the time-step of the model. A reader can suppose from Section 2.2.1 that it is close to 1 month but, anyway, it should be clarified. As far as the spatial resolution of the model is 2 km and the maximum assigned horizontal hydraulic conductivities can exceed 100 m/day, the time-step seems to be too large. Some remarks on the concordance between time and space resolutions of the model (taking into account typical values of soil conductivities) are needed in the paper.

We added additional comments that clarify and justify the time steps used in the model: “Each stress period was divided into 10 time steps, using a fixed time step multiplier equal to 1.2. At the start of each annual stress period the initial time step in the model is initially on the order of a week and increases each time step by a factor of 1.2 throughout the year. The model was tested with much smaller time steps (using initial time steps less than 1 s) and essentially the same results were obtained for the simulated water balance and aquifer heads. In addition, water balance errors were always less than 0.01%. Therefore, the degree of temporal discretization was sufficiently accurate and led to reasonable computational times for the 3,000 calibration simulations."

4. The term "Ss" in the Eq. 1 should read "S".

This was corrected.

5. The 1st sentence in the Section 2.2.6 and the 3rd sentence in the Conclusions should be corrected.

This was corrected, i.e. "evaporation after the crop growing season" instead of "evaporation after of the crop growing season".

Interactive comment on Hydrology and Earth System Sciences Discussions, 2, 2061, 2005.