

Interactive comment on “Reduction of predictive uncertainty in estimating irrigation water requirement through multi-model ensembles and ensemble averaging” by S. Multsch et al.

Anonymous Referee #1

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The paper describes an attempt to study uncertainties in irrigation water requirements simulated for wheat growing in the Murray-Darling Basin (MDB) in Australia caused by the structure of the model and by the parameters used in it. In general the paper is well written and interesting but I have serious doubts that the setup of the study is useful to address the objectives described in the manuscript. My major points of criticisms are:

1.) The authors use six empirical methods to calculate evapotranspiration in order to study the structural uncertainty of the model and five crop coefficient sets (needed to convert the ET of a reference crop to the one of a wheat crop) to study the parameter uncertainty in the model. Results of the so created ensemble of model runs (6 ET methods x 5 kc-sets) are weighted then according to their performance in repre-

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senting measured data to derive a weighted ensemble mean and the ensemble range around the weighted mean. Such a setup is useful to compare results of complex models when the knowledge about the accuracy of the models is limited. However, the methods used to compute ET in this study have been extensively evaluated in previous research. Based on comparisons with lysimeter measurements performed in different climatic environments it is well known that the Penman-Monteith (PM) method usually performs best when high quality measurements of all the required weather variables are available. The other ET-methods used in the study are less precise because they ignore some important weather variables or relationships between them determining site specific evapotranspiration. As such, I don't understand the value of applying additional inaccurate methods and of creating some “artificial” uncertainty. In other words: what is the additional value of the weighted ensemble mean as compared to the direct use of the PM method (maybe with site specific adjustment of the resistance terms)?

2.) To “evaluate” the performance of the ET methods the authors used class-A pan data measured at 34 stations (page 7532, lines 16-23). By doing this the authors evaluate evapotranspiration calculated for a reference crop (ET) by measured evaporation from an open water surface (E) which is certainly not the same. The agreement derived from this comparison is then used as a weighting parameter to compute the weighted ensemble mean. Again, I doubt that this weighted ensemble mean will represent an improvement to the direct use of the PM-method. But the authors can test this by comparing the performance of PM56 to the ensemble mean of the other methods.

3.) Parameter uncertainty is evaluated by using 5 sets of crop coefficients. These coefficients relate the ET of a wheat crop to the ET of the reference crop surface. The factors describing the differences between the ET of wheat and the one of the reference grass surface are described in detail in Allen et al. (1998), for example. Methods to reduce uncertainties in crop coefficients would be to (i) adjust standard crop coefficients by considering the local conditions (wheat management, wetting interval, aridity, growing period length . . .) or (ii) using a process based crop model that directly

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accounts for the underlying processes. APSIM, for example, has been developed in Australia and was frequently applied for the local conditions. I doubt that the set of the crop coefficients used in this study really provides a representative picture on the expected parameter uncertainty.

4.) While the authors focus on potential uncertainties caused by the ET calculation method and the crop coefficients, there is little explanation why these two factors were selected and how some other factors may affect the uncertainties in irrigation water requirement calculated in this study. The model applied here uses some very crude assumptions (e.g. that runoff is fixed to 20% of precipitation, see equation 2). In addition, it does not account for the spatial heterogeneity in soil or crop conditions. From this perspective it's hard to see what readers can learn from the results and what can be generalized for other sites, models and investigated factors.

Specific comments:

Page 7527, lines 9-11: "We find that structural model uncertainty is far more important than model parametric uncertainty to estimate irrigation water requirement." Please notice that only one parameter was tested. Therefore this conclusion is too general.

Page 7527, lines 16-18: "We conclude that multi-model ensemble predictions and sophisticated model averaging techniques are helpful in predicting irrigation demand and provide relevant information for decision making." To support this conclusion it is required to show the additional value of the multi-model ensemble predictions, as compared for example to a single application of the Penman-Monteith method. I can still not see it here.

Page 7527, lines 21-25: "Globally, the proportion of fresh water consumption by agriculture is large (9087 km³ yr⁻¹) (Hoekstra and Mekonnen, 2012) and is projected to increase in the future in order to support the increasing world population. More precisely, most of the change in freshwater consumption will arise from the increasing irrigation demand by crops (De Fraiture and Wichelns, 2010)." It's required to be more

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precise. The first figure on fresh water use refers to the sum of irrigation water and natural rainfall while the second statement refers to irrigation only. That future irrigation water requirements will increase is not sure. Models accounting for the reduction in transpiration due to increased atmospheric CO₂ concentration show constant or even declining trends. Therefore this section does not reflect the state of knowledge.

Pages 7527-7531 (introduction): The authors describe here what they have done in the paper but the objectives remain unclear. Is the objective to quantify uncertainties in irrigation water estimates in models of the same type or is it to develop and present a new method for uncertainty assessment? How does this study compare to all these crop model comparisons published within the last 2-3 years? Wouldn't it be better to replace the crop coefficient approach by a real simulation of crop growth instead of just applying different sets of kc-values with unknown representativeness?

Page 7531, lines 15-17: "The applicability of six different ETo methods is evaluated by using available measured class-A-pan evaporation measurements of 34 stations in the MDB over a 21 years time period" ET is evaluated with E => does not seem to be very useful

Page 7532, section 2.1 Study site and data: What about uncertainty in input data (e.g. land use, weather) and their interaction with model structure? Uncertainties in humidity and wind speed will likely affect PM but not some other methods like Hargreaves or Priestley-Taylor

Page 7533, equation (2): Which data or findings support the very basic assumption that 80% of total precipitation becomes effective?

Page 7536, lines 17-18: "The median daily ETo for APET is 3.6 mm d⁻¹, PM56 3.9 mm d⁻¹, HS 3.8 mm d⁻¹, PPET 5.2 mm d⁻¹, PT 6.4 mm d⁻¹ and TURC 3.4 mm d⁻¹." Please check the calculation routine and the underlying data for the calculations with Priestley-Taylor. An overestimate in the here reported range is very unlikely and not supported by the previous literature!

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