Hydrol. Earth Syst. Sci. Discuss., 11, C822–C826, 2014
www.hydrol-earth-syst-sci-discuss.net/11/C822/2014/

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Using hydro-climatic and edaphic similarity to enhance soil moisture prediction" *by* E. J. Coopersmith et al.

H. Bogena (Referee)

h.bogena@fz-juelich.de

Received and published: 7 April 2014

General comments:

In this study, the simple soil moisture model developed by Pan et al. (2003; 2012) was calibrated using in-situ soil moisture measurements from the Soil Climate Analysis Network (SCAN). The optimised parameters are transferred to ungauged locations via a hydro-climatic classification system and the decrease in soil moisture prediction capability is analysed.

The authors claim that the extrapolation of model parameters to ungauged locations via a hydro-climatic classification system will enable near real-time irrigation decision making. They showed that the decrease in soil moisture prediction capability is less

within a hydro-climatic class than between different classes. However, from existing 200 SCAN sites they selected only 15 for this analysis of which most are not located in regions with significant irrigation demand. If the main goal of the study is to support irrigation decision making this study should focus on more relevant sites.

The authors applied the model developed by Pan et al. (2003; 2012) at an hourly resolution without further adapting the method. The model was explicitly developed for daily resolution as it assumes that the daily soil water loss can be described with a sinusoidal function describing the inter-annual change in evapotranspiration (ET) demand. Cleary, at hourly resolution, the changes in ET demand during the day need to be considered as well. This needs further model development. In addition, the model results should be analysed in more detail. For instance, it should be tested whether the sinusoidal function is able to capture the seasonal variation in soil loss (also after application of the machine learning algorithm).

The machine learning algorithm that was used to further optimise the model results (bias correction) actually produced also worse results, especially during dry periods (see specific comments for examples). Furthermore, the model even adopts measurement artefacts (see New Mexico SCAN station). These problems need to be critically discussed and the usefulness and limits of the machine learning approach should be challenged.

The paper is mostly well written and structured. However, given its numerous problems concerning its contents, the parameter optimisation approach as well as the interpretation of the results of the analysis I cannot recommend the publication of this paper. Nevertheless the topic is relevant and well suited for HESS. I therefore recommend a major revision.

Specific comments:

The introduction section has several errors and inconsistencies:

- The analytical model proposed by Entekhabi and Rodriguez-Iturbe [1994] does not belong to the API based model group.

- Soil moisture models are typically not subject to recalibration.

- The second group should refer to “process based model approaches”.

- It should be mentioned that process based models are typically forced by evapotranspiration demand and precipitation (upper boundary) and, if applicable, by groundwater (lower boundary).

- HYDRUS does not necessarily needs soil temperature, ionic chemistry etc. for the simulation of soil water dynamics.

- The third group is actually not model-based, but refers to a field instrument for the characterization of near surface soil properties. Therefore, this group should be omitted.

- The model of Pan et al. (2003) is a simplification of the linear stochastic partial differential equation proposed by Entekhabi and Rodriguez-Iturbe [1994].

- Clearly the model of Pan et al. (2003) does not address all shortcomings of other existing modelling approaches.

The discussion section has several weaknesses:

- The model of Pan et al. (2003) ignores lateral water flow. Therefore, an application of this model to sites with significant topography is not appropriate without considering lateral flow processes, which cannot be implemented in meaningful way in such a simple model approach.

- Similarly, the enhancement of the model with respect to overland flow is not reasonable. In addition, since the main goal of the model is to support irrigation management overland flow is not important.

- The link to the SMAP mission is unclear and should be skipped.

- Instead, problems and uncertainties involved in the modelling and in the parameter transfer should be addressed in greater detail.

- Last but not least the benefit of this study should be presented more clearly.

P2323 L7: Delete “precipitation”

P2324 L17: Which problems are you referring to?

P2326 L11: The index “4” of parameter “c” is unnecessary.

P2326 L16: Delete: “and cannot increase its moisture content”

P2326 L27: “n hours”

P2327 L1-2: This sentence should be rewritten in a more scientifically way.

P2327 L10-11: Please change the sentence into: “Soil water loss at hour i, e.g. due to evapotranspiration or deep drainage, is expressed by coefficient n.”

P2327 L13: The term “eta series” is not appropriate because it neglects loss due to drainage.

P2327 L17: In the original model of Pan et al. (2003) this sinusoidal wave function is used to represent the changing evaporation demand during the year. I wonder whether the hourly resolution used in this paper is not violating the assumption of Pan et al. (2003). For instance an additional function to represent the daily fluctuations of evaporation demand could be added.

P2332 L26: 100 % would mean pure water, therefore change “are measured in percentage terms (0–100)” into “are presented as volumetric percentage”.

P2333 L7-11: The authors claim that the high soil water contents are due to flood events occurring only during the validation years. This is, however, not true. First, saturated soil conditions can in principle also happen also during times without flooding. Second, I checked the data from this SCAN Site and found that soil moisture exceeds the given porosity value frequently during the training period (e.g. Nov 2009 soil moisture reached 50 Vol.%). Please revise this part accordingly.

P2333 L20-24: This is not entirely true. During several recessions (e.g. 4300 – 4500, 4700 – 4800) the machine learning correction clearly degrades the previous result (overprediction). This is especially alarming since the degradation happens during dry soil states, making the model more unreliable for irrigation management.

P2333 L25-29: First, the machine learning correction code seems to fit the diurnal cycles in the New Mexico data by transforming the annual sinus function into a daily one. This is clearly a violation of the original model of Pan et al. (2003). Second, the reason for the diurnal cycles is not a water related process. Actually it reflects the dependency of the electromagnetic soil properties to temperature change. Therefore, the apparent permittivity, which is measured by the soil moisture probe to infer soil moisture, decreases with temperature (from 88 at 0 °C to 76 at 30 °C). This is a well-known problem; see e.g. Rosenbaum et al. (2011).

Figures

Fig. 9 and 10 should be merged.

Fig. 11 should be presented as a Table.

References

Rosenbaum, U., Huisman, J.A., Vrba, J., Vereecken, H., Bogena, H.R. (2011): Correction of temperature and electrical conductivity effects on dielectric permittivity measurements with ECH2O sensors. Vadose Zone Journal, 10 (2): 582-593.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 2321, 2014.