Hydrol. Earth Syst. Sci. Discuss., 7, C1080–C1083, 2010
www.hydrol-earth-syst-sci-discuss.net/7/C1080/2010/

Hydrology and Earth System Sciences Discussions

Interactive comment on " Modelling the hydrological behaviour of a coffee agroforestry basin in Costa Rica" *by* F. Gómez-Delgado et al.

Anonymous Referee #1

Received and published: 8 June 2010

General comments

This is a well-written and extensively documented paper, presenting interesting data from the humid tropics where such intensive experiments are scarce. Therefore I would surely recommend to publish the paper, but I have some reservations about the hydrological model and its use in the study.

First, for being a conceptual hydrological model, the model is a very complex and highly parameterised. One of the main advantages of a conceptual model is its parsimony, avoiding over-parameterisation and lack of identifiability. But with 20 parameters, this advantage is largely lost. Granted, some of the parameters are fixed beforehand,

based on either field observations or literature values, but the number of parameters is still high, and the lack of sensitivity of many of them (table 3) does indicate that a simpler model may provide an equally good fit.

Indeed, I am not convinced that the complexity of the model is fully justified. Not less than 6 different runoff pathways (surface and subsurface) are identified. Not only will it be very difficult to identify these pathways by calibrating a model on a single discharge time series, but I am even unsure that all pathways are justified from a physical perspective. For instance, the depth of the root zone is identified as 1.6m (p.3035/7), which coincides with a soil transition from porous volcanic material to a more clayey, compact and stony deposit. Given the small size of the catchment and the steep slopes, it does not seem unlikely that most of the hydrological response will occur in the zone above this clayey layer. Most likely, infiltration in this layer will be small, and the portion that will eventually make it to streamflow negligible. Trying to identify four different water pathways below this layer, of which 3 contribute to streamflow and one does not, seems pretty challenging!

This is related to a second comment, about the purpose of the modelling. It seems that the main purpose of the modelling exercise is to quantify the different hydrological fluxes. But would this not have been easier which much simpler methods? Especially with high-resolution flux tower measurements available, simply solving the water balance would probably give a good estimation of deep infiltration. It seems a petty that the flux tower measurements are only used to evaluate the hydrological model, rather than using them directly in the calculations. Similarly, with high resolution streamflow measurements, a peak flow / base flow separation method may have given equally good results to estimate surface runoff.

One way of justifying the use of a more complex model is the ability to include additional information (e.g., ground water levels, soil water content), although I would rather expect to see them used for a multi-objective model calibration rather than an evaluation of the model. As explained higher, with the risk for overparameterisation and parameter interaction, I am not sure whether the additional data will add predictive capacity, but this could be rather easily tested by calculating the uncertainty bounds of the predictions of the different fluxes.

However, the uncertainty analysis is a bit strange. It seems a rather ad hoc addition of some uncertainty by varying the parameters by 30%, with very little justification. Indeed, although the uncertainty limits are referred to as 95% and 99% confidence intervals, they bracket only resp. 20% and 43% of the observations, which makes it obvious that not all uncertainty is accounted for. A method based on clear assumptions, either subjective (e.g., GLUE) or more formal (Bayesian statistics) may provide more insight in this discrepancy and therefore the quality and relevance of the model predictions.

Finally, although I am not being a sensitivity analysis expert, the use of the correlation-based measures (table 3) and the procedure of the time-dependent analysis are not clear to me. Maybe simple Hornberger-Spear-Young sensitivity analysis style cumulative distribution plots may be more illustrative.

Specific comments

3019/13-14: the use of potential evapotranspiration is quite standard in hydrological models for a long time so this statement seems exaggerated.

3019/20 - 27: Again, this is probably exaggerated. Interception, as well as evapotranspiration routines have been part of hydrological models for quite a long time (see e.g., Beven 2001, Rainfall-runoff modelling p78).

3023/21: How good was the relation between the flux observations and the Penman Monteith model?

3027/6: Is it really realistic that infiltration depends on the water content of a layer below 1.6m depth?

3028/14 Is this the potential evapotranspiration Kc*ET0, or the reference evapotranspi-

ration ET0?

3034/20: Why only at 30 min timesteps if you have 10 min frequency calibration data?

3036/8: Why is a split sample test not possible on a different season? If your model represents the system reasonably well, it should give good results in non-stationary conditions. This is even a good check of lack of parameter interaction, and surely compatible with Klemes, 1986 (Operational testing of hydrological simulation models, Hydrological Sciences Journal 31, 13-24).

3039/27: I don't really see the reason for this conclusion. FAO does indeed recommend a crop constant of around 0.95 for coffee systems, while here it seems that it would be rather around 0.6, but as there may be water stress during at least a part of the year (theta observations in Fig 8), this may not be completely unrealistic?

Technical comments

3055/13: Koppen

3055: revise citations, e.g., I could not find Poulenard et al in the text

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 3015, 2010.