Hydrol. Earth Syst. Sci. Discuss., 7, C1879–C1891, 2010
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Hydrology and Earth System Sciences Discussions

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Interactive Comment

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

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Received and published: 18 August 2010

We appreciate the constructive reviews from the anonymous referee 1. All the technical comments and corrections will be incorporated to a revised manuscript. The general and specific comments are replied in the same order they were stated by the referee.

General comments: Anonymous Referee 1

"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."

The authors: we clarify that the number of parameters to calibrate is only ten (given in Tables 2 and 3 and in Fig. 10). The other parameters, which allow the model to be applied in different contexts, have been fixed and should be regarded as any other modelling assumption when building a conceptual model. In relation to the lack of sensitivity of the model to its parameters, the power of simple sensitivity approaches (like the one presented in Table 3) was assessed through a time-varying sensitivity analysis (summarized by Fig. 10). This latter analysis confirmed a high sensitivity of the model to the variations of seven out of these ten parameters. In the revised version of the paper we propose to fix the values of the three parameters for which the model was not sensitive (f_c, α and k_D), recalibrating the model for the other seven parameters (β, k_B, k_C, E_X, k_{E1}, k_{E2} and k_{E3}) and producing an updated water balance, as well as new uncertainty and sensitivity analyses.

"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."

The authors: We include an additional figure (Fig. 1) in this file of comments, presenting

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the first (simple) model that we proposed to explain the behaviour our experimental basin. This approach led to unsatisfactory results, suggesting that we were facing a higher complexity during peakflows, but also (and especially) over recession times. It was also insufficient to account for variables like interception, water content only in the root zone and water drainage below this zone. It must be noticed that the calibration on a single discharge time series was not the only tool we used to identify the six pathways (two per soil zone), but also the validation on measured evapotranspiration, soil water content and water table level. These measurements were also useful during the model parameterization. To verify whether the complexity of the model is justified, we offer to simplify the already reduced seven-parameter model, by removing two lateral pathways (Q_{B2} and Q_{E2}), to preserve in the model only one lateral flow at each soil reservoir. Such operation would remove one additional parameter (k_{E2}). Then we can compare the resulting Nash-Sutcliffe coefficient with that of the original model. If the efficiency is not very different, we propose to replace the original model by this simplified six-parameter model in the revised version of the paper.

"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!"

The authors: our description of the soil profile might be misleading. This clayey deposit is still a volcanic soil, with properties being far from those of an impermeable layer. This fact is reflected by the annual hydrograph (Fig. 5a, Fig. 7a), in which a quick increase in the baseflow rate can be noticed within the duration of the largest rainstorm

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events, thus confirming rapid aquifer recharges occurring under 1.6 m deep (please refer to Fig. 5e, which presents the variation in water table levels below 1.6 m). Then we aimed to identify those three different contributing-to-streamflow pathways in the non-saturated/non-root zone and in the saturated zone, basing on the properties of the recessions over the year. In addition, we consider that a streamflow time series at 30 min time step and for such a small basin provide highly process-detailed patterns (either during peak events or recessions), which are not necessarily well explained by simple models.

"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."

The authors: our modelling exercise primarily focused on understanding the integral behaviour of the different components of the hydrological system, and to provide a model that could be useful as a common platform for different applications, e.g., hydrological, ecophysiological and environmental studies. The simultaneous functioning of processes from plant interception to deep percolation is of our best interest, going from runoff formation processes that can be used to investigate erosion and basin sediment yield, to the precise partition of vertical and lateral fluxes allowing to study the trans-

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port and fate of contaminants. On the other hand, though we agree that surface runoff could have been estimated separately by simpler methods, in the case of deep infiltration the eddy-flux alone does not allow separating the drainage term from the soil water accumulation term, at least not at high time resolution. To our understanding, rather than a drawback, the use of additional measurements at the validation phase, instead of using them for calibration, is a strength of our study. Our model was designed to be generic, in order to allow its use in other basins where streamflow is available for its calibration, but neither evapotranspiration, nor soil moisture nor ground water level. Simple basin experiments with only streamflow are the most common (especially in developing countries) and such a model-inversion approach of the intermediary variables (evapotranspiration, drainage, surface runoff, etc.) could be very useful. Our purpose was to test how realistic could be a model that is calibrated only with streamflow observations to simulate other processes, and under this particular context. What we show here is that after adjusting the modelled streamflow to the measured one, the intermediary variables can be successfully validated from independent measurements, yielding some confidence in the model and opening to more generic future applications. Then, for instance, we preferred to use the flux tower measurements to have an independent validation of the evapotranspiration (ET) model. We consider that the difficulties encountered in model building and parameterization, that were leading us to more complex modelling schemes, could be a warning about the use of too simple models to assess hydrological processes at high time and space resolutions. For example, the assumption that ET = 0.95 PET (FAO) is too simple to model the seasonal variations of ET in coffee.

"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."

The authors: the approach for uncertainty and sensitivity analyses that we have chosen is fully described by Helton (1999). The use of certain percentage change in the parameter values was found to be a simple and apparently common technique, used in some papers that we reviewed (White et al., 2000; Ines and Droogers, 2002; Lenhart et al., 2002; Zaehle et al., 2005, Droogers et al., 2008) and methodologically defined by authors like Hamby (1994, 1995) and Frey et al. (2002). It can be seen as an alternative when the parameters are highly conceptual (like most of the parameters in our model), and when there is no physical evidence or criteria to attribute them a probable range of variation. The lack of accuracy of the model pointed up by the referee, moved us to carry out all the necessary calculations to avoid the use of indirect confidence bounds around the optimized streamflow estimation, as we first presented in the original version of the paper. This change entailed the calculation of 17520 frequency distributions (one for each time step) over 100 parameter combinations, to produce the empirical confidence interval that we present in the Fig. 2 of this file of comments. Then, the 95% interval of confidence contains 74% of the measured values, while a 99% confidence interval contains 83% of the measured streamflow values. We recognize that the simplified approach that we presented previously was inappropriate, neglecting the fact that most of the uncertainty was actually being accounted for in our analysis. We propose to include these new, more formal results in the revised version of the paper, replacing the previous Fig. 9 with the Fig. 2 that we present in this file of comments. In case the referee is still not satisfied with this methodological correction, we are open to discuss the relevance of including additional analyses, based on more sophisticated methods like GLUE or Bayesian statistics.

"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."

Even though it demands an exhaustive calculation procedure, the sensitivity analysis that we propose is very simple, and aims to clarify any possible interaction (linear or non-linear) between each parameter and the model response. In addition, this assessment was made for each time step, introducing a dimension that is, to our knowledge, frequently ignored: the change over time of model sensitivity to a particular parameter. This barely explored approach gave evidence that not all parameters are important all the time. It also allowed identifying what parameters are influential under particular modelling conditions (storm events, long recessions, high or low soil humidity, etc). Time-integrating objective functions like the Nash-Sutcliffe coefficient can easily hide the importance of several parameters, which can be verified by comparing Table 3 in the original discussion paper with Fig. 10 in the same file. Besides being a relatively new approach, it also proved to be useful (we just used it here to suggest how to simplify our model further). Therefore, we suggest preserving the sensitivity analysis as it is in the revised version of the paper.

Specific comments: Anonymous Referee 1

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

The authors: this idea was poorly explained and probably also exaggerated. What we meant is that potential evapotranspiration (PET) is the base of many hydrological models, but many of them use it in a very simplistic way, for instance, assuming that actual evapotranspiration (AET) is either equal to PET, or a constant fraction of PET, or a variable fraction of PET varying only with the soil water content in the root zone.

"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)."

The authors: we will moderate the statements of this paragraph, though it is not referring to particular hydrological processes (interception, evapotranspiration), but to global modelling approaches.

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

The authors: Penman-Monteith and measured evapotranspiration are presented in Fig. 5c of the Discussion Paper. The relation between them was proportional when the leaf area index (LAI, Fig 5.b) was not a limiting factor (LAI $\geq$ 4). However, when LAI was at its minimum values because of dry conditions or coffee pruning, the relationship blurred and the crop coefficient (AET/PET) went much lower. Thus, AET is a variable fraction of PET, and LAI is a main driver here, varying seasonally from 2 to 5.

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

The authors: it is probably not very realistic, and it was a recurring question during model building and parameterization. The first idea was to make infiltration a function of soil water content in the non-saturated zone (in general). Then, the splitting of root and non-root reservoirs brought this question for the first time. We initially considered that for most purposes, the two new reservoirs could remain integrated. But then these differences in soil layers came up (excavation experiments) and now this assumption may be unrealistic. We offer to make a test run of the model, removing this dependence, to see how much it affects the model response. If the influence is negligible, we will suppress this link in the revised manuscript.

*“3028/14 Is this the potential evapotranspiration Kc*ET0, or the reference evapotranspiration ET0?”*

The authors: it is the reference evapotranspiration ET_0. We incorporate the correction in the revised manuscript.

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

The authors: the evapotranspiration post-processing yields half-hourly values, not 10 min frequency values.

“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).”

The authors: we agree that this is an incorrect statement. The relevant idea here is that as we already had independent measurements for the validation process (evapotranspiration, soil water content and water table level), then, if we wanted to achieve

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the most complete and accurate streamflow-based calibration, we should use all the available streamflow record.

"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?"

The authors: the crop coefficient we observed here varied seasonally between 0.4 (in conditions of low LAI, and during the drier season) and 0.8 (max LAI, max rainfall). The defaut 0.95 value from FAO is thus probably over-estimated, but it might depend much on LAI and soil water availability. As the rainfall is present over the whole year (even during the driest season), there was no period of water stress (the relative extractable water simulated by the model and measured was always above the threshold of 0.4, value at which the T/PET ratio starts to drop due to soil drought and stomatal closure, according to Granier et al., 1999). Coffee plants were densely rooted down to 1.5 m, which makes a considerable water stock. Then, in the text we comment that the leaf area index is the controlling condition for AET. Crop phenology (seasonal variations of LAI) could thus be a more relevant factor than soil water content or other factors in this particular coffee system that does not experience drought. Too much reliance on a directly proportional estimation of AET as a function of ET_0, or of ET_0 and soil water only, might lead to great errors, especially to describe seasonal variations.

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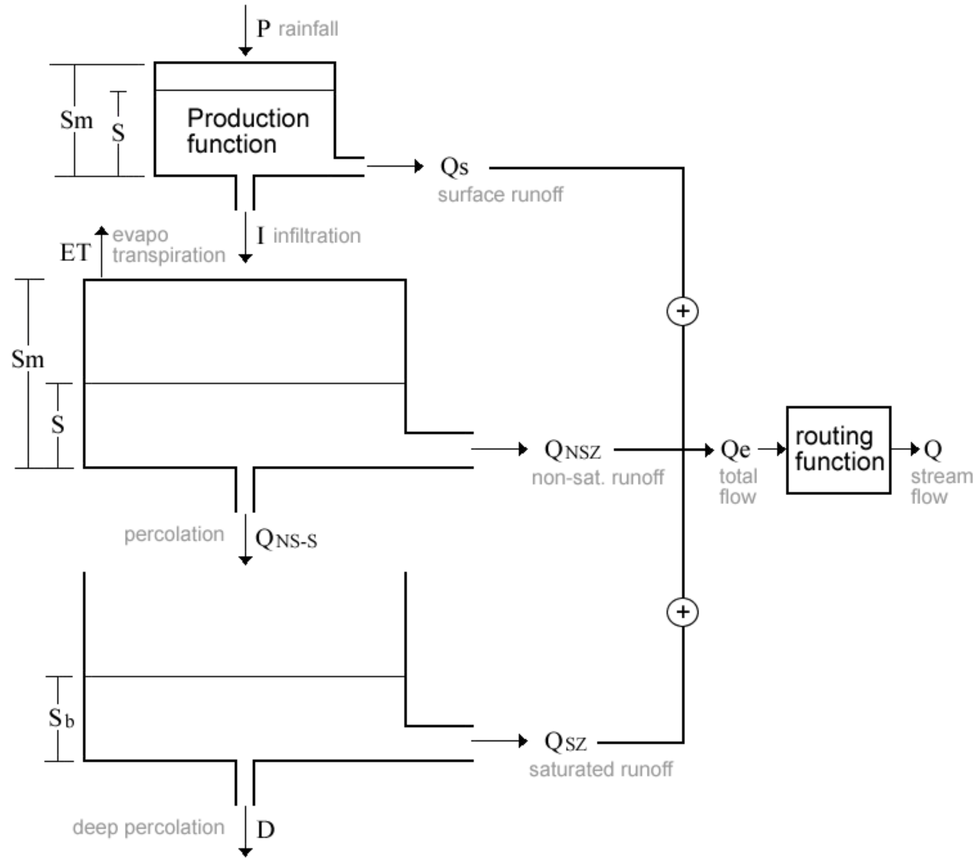

Fig. 1. First model conceptualization for the Aquiares experimental basin.

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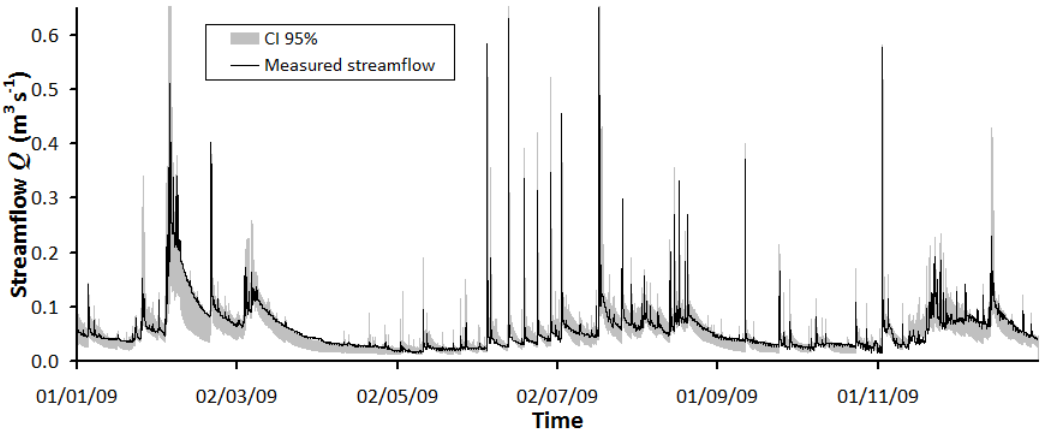

Fig. 2. Uncertainty analysis of model outputs for a 95% confidence interval (gray region), which contains the measured streamflow values (black line) for most of the time steps.

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