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Hydrology and Earth System Sciences Discussions

Interactive comment on "The impact of forest regeneration on streamflow in 12 meso-scale humid tropical catchments" *by* H. E. Beck et al.

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Please note: To ensure an unbiased evaluation I have performed this review without reading the other reviewer comments on this manuscript.

Summary:

The manuscript entitled "The impact of forest regeneration on streamflow in 12 meso-scale humid tropical catchments" summarizes a modeling study performed on a number of meso-scale catchments in Puerto Rico in which the change of the water balance (and the associated Q-measures) by forest regeneration resulting from farm land abandonment was investigated.

The most interesting aspect of the study is that it challenges a widely accepted research paradigm which states that increased forest cover will also increase the evapotranspiration within the water balance of a landscape (Bosch & Hewlett, 1982; Stednick, 1996; Andréassian, 2004; Brown et al., 2005). Following this paradigm, reforestation of larger land areas would lower stream runoff, especially under low flow conditions. However, the results of this study question this paradigm at least for meso-scale tropical catchments by providing results opposing the paradigm. These results are further supported by a detailed comparison with similar studies from other, meso-scale tropical catchments were similar results regarding a low impact of reforestation on the water balance have been stated. Therefore I consider the manuscript as a valuable and unique contribution to the literature of forest-hydrology and very suitable for publication in HESS.

The overall quality of the manuscript is high. The methods are well developed, fully suitable for the research question and well described. The use of the spatially lumped HBV-light model seems justified for the given model purpose. The research hypotheses are well developed from a broad body of literature. The results are described clearly and concise. The discussion brings up the critical parts of the manuscript and discusses them sufficiently. The conclusion summarizes nicely the overall results.

Aspects that could potentially be improved concern primarily the model uncertainty, especially the discussion of the sensitivity of different model parameters and the uncertainty of input data. More specifically, the point that I would be the most cautious about it the 'problem of closing the water balance' for some of the catchments. It is obvious that the values used for the parameter PCORR are very variable for some of the catchments: F (1.215), G (0.712), I (1.415) and L (1.4). Presumably, this parameter will also have a very high sensitivity, because P dominates the water balance equation. To me this indicates that there must be additional mechanisms that cause these inconsistencies. Whereas some of these mechanisms are named in the results section (see P 3062, L2-4) they are only briefly discussed within the discussion section. I would

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suggest strengthening this part in the discussion. The points I would suggest for this are:

a. Uncertainties in catchment areas (You give USGS uncertainty estimates of +/- 10%). However, these estimates appear very small to me given that at least some catchments are underlain by limestone aquifers with well-developed karst systems. I believe that this geological setting could very well account for the inconsistencies of the water balance.

b. Another mechanism causing this could be 'water recycling' (e.g. that water that has been evaporated or transpired reoccurs in a catchment as precipitation). I am not sure if such a mechanism is discussed for the tropical regions of PR, but it could well account for some of the imbalances (especially considering that some of the catchments have corrections for P of -30% and others have +40%). An additional paper that may be interesting to add to the discussion regarding this aspect is given by Ellison et al. (2012).

c. Finally, the spatial uncertainty in P inputs. Even if the IDW regionalization may be powerful, it may simply miss some large convective events that have a small spatial extend, but high P intensities. Some type of uncertainty estimation on the regionalized P maps would also be great (maybe something like a 'leave out' approach for some stations). However, I do note that the manuscript is rather long already, so the authors should consider this as an optional thing.

References:

Andréassian, V. (2004). Waters and forests: from historical controversy to scientific debate. Journal of Hydrology 291(1-2), 1-27.

Bosch, J.M. & Hewlett, J.D. (1982). A review of catchment experiments to determine the effect of vegetation changes on water yield and evaporation. Journal of Hydrology 55(1-4), 3-23.

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Brown, A.E., Zhang, L., McMahon, T.A., Western, A.W. & Vertessy, R.A. (2005). A review of paired catchment studies for determining changes in water yield resulting from alterations in vegetation. Journal of Hydrology 310(1–4), 28-61.

Ellison, D., N. Futter, M. & Bishop, K. (2012). On the forest cover–water yield debate: from demand- to supply-side thinking. Global Change Biology 18(3), 806-820.

Stednick, J.D. (1996). Monitoring the effects of timber harvest on annual water yield. Journal of Hydrology 176(1–4), 79-95.

Small in-text edits/suggestions (please note: I am not a native speaker):

Abstract: P 3046, L2: I would remove 'comparatively'

Introduction: P3048 L1-5: is rooting depth also a variable that should be mentioned here? P3048 L22: remove relatively (or state relative to what. . .) P 3048 L29: maybe use 'regions' as a simpler term for physiographic units

Data and Methods: P3051 L7: any chance to get to know the resolution of the photo interpreters? Appears like an open question for a reader. . . P 3052 L21: 'The' should not be capitalized. P 3054, L7: I would maybe be good to state why the authors developed this methods. Otherwise it leaves the reader a bit with a method were it is unclear why it should be used. P 3055 L7-12: This could be shortened, if only IDW is used. Just state on which basis the authors chose their method. P 3055 L10: remove 'here' P 3056 L30: It may be good to state that inter-basin GW tranfers are not considered in the model. P 3058 L1: the authors may need to explain what 3D Q is in a lumped model. P 3058 L10: how where they combined? Please explain a bit more. P 3059 L13: the authors could consider removing this equation, given that it is just a linear trend that is assumed. P 3060: Interesting. . . using the Jackknife approach.

Results: P 3061 L 25: I am not sure if 'degraded' is the right term here. Maybe use 'negatively affected'

Discussion: P 3063 L20: 'trends', plural P 3063 L21: remove 'in turn' P 3064 L1: Maybe

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replace ‘accept’ with ‘support’ – for me a hypothesis can only be supported. . . P 3064 L11-16: this is great! P 3068, L1: replace ‘outstanding’ with ‘challenging’ P 3069, L2 and L 8: replace ‘errors’ with ‘uncertainties’ P 3069, L15-16: replace ‘is about’ with ‘is estimated to be’ From L 25: great reading.

Conclusion: Good, nothing to add.

A final remark after reading the other reviewers comments: I do agree that the level of detail, especially in the methods section is at the edge of being too detailed. However, this may at least in part be personal preference and I, personally, would rather prefer a detailed and complete description of what was done, than having an ‘incomplete’ manuscript. So I guess it up to the authors and the editor to decide what level of detail is needed.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 3045, 2013.