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Discussions

Interactive comment on "Long-term effects of climate and land cover change on freshwater provision in the tropical Andes" *by* A. Molina et al.

A. Molina et al.

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We thank reviewer #3 for his constructive review, based on a profound knowledge of the area and the research topic. Given the spatial and temporal scale of our analysis, it is not possible to get into all details of the hydrological processes as a limited amount of information is available for a catchment that is heterogeneous in topography, land cover and geomorphology. However, we feel that our approach is valid as it provides insights in the potential effect of land cover change on streamflow, which is currently an under-studied subject at the catchment scale.

We have made the necessary changes to our water budget approach, and now evaluate also the potential impact of horizontal precipitation on streamflow (see below).

SPECIFIC COMMENTS

- I had to read the paper through to understand the research objectives of the authors. I suggest they add a couple sentences to the introduction specifically stating their objectives, such that the methods, results, and conclusions directly follow from the objectives.

Reply: We have added two sentences at the end of the introduction where we reiterate our research objectives: "The main objective of this paper is to quantify the potential long-term effect of land cover change on streamflow in the Tropical Andes. By analysing multi-decadal time-series of hydrometeorological data, we specifically tested the relative sensitivity of streamflow to climate and land cover change."

- The results from the ET model should be placed in the results section, prior to the discussion.

Reply: Our discussion starts with the analysis of change in the partial water balance based on the land cover change analysis and ET model results. We prefer to keep the results of the ET model here, to avoid repetition between the results and discussion section and to enhance the quality of the discussion.

The analyses are appropriate for the case study, but the authors should provide additional context with which to assess their assumptions, analysis, and conclusions.

Reply: We made several improvements to the text, tables and figures based on the comments of reviewer#3.

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1. The authors separate quickflow and baseflow using monthly streamflow timeseries. What are the timescales of quickflow in the catchment? If they are considerably less

than a month, it would be more appropriate to conduct this analysis using daily streamflow data.

Reply: Thanks for remarking this. There was an error in the text which caused this confusion. The separation of the streamflow into quick and slow flows was done on the daily data. This is clarified in the text.

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2. Given that all land use maps contain some uncertainty, it is valuable to the reader to know how accurate the maps are. For the remote sensing classification, the authors use a method previously developed in a separate paper but do not discuss any accuracy assessment. I suggest reporting the accuracy of the described method and describe how it would apply to their case study. Then, what kind of error can the reader expect in the results? What were the limitations to conducting an accuracy assessment?

Reply: In the previous version of the paper, we did not provide full details on the accuracy assessment of the land cover maps. We now report the accuracy of the methods that we used, and refer to our previous work on land cover monitoring – where necessary:

“The accuracy of the land cover change analysis is function of the errors on the individual land cover maps. Land cover maps for 1963 and 1977 were extracted by manual on-screen digitalisation on high resolution copies of aerial photographs. As such, the accuracy of the land cover maps mainly depends on the horizontal positional accuracy of the orthorectified photographs, and is systematically below the spatial resolution (30 m) of the aggregated land cover maps (Vanacker et al., 2003; Guns et al., 2014). A thorough validation of the land cover classification was realized for the 2001 satellite derived map (Balthazar et al., 2012), based a stratified sampling of 300 points for which the reference class was identified on very high resolution aerial photographs. The error matrix reveals an overall classification accuracy of 94% (Balthazar et al., 2012). Given

the temporal dependence of land cover time series, the uncertainty on the amount of bi-temporal land cover change is not the limiting factor in our analysis.”

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3. The residual trend of water depth from the empirical mode decomposition declines from 1974-1990, after which it is nearly stationary (Figure 5, bottom panel). I assume, based on the other analyses, that this does not necessarily entail that the decline in discharge occurred entirely before 1990. Please confirm and/or clarify. If the opposite is true and streamflow is mainly stationary after 1990, it would entail that changes in discharge cannot be attributed to tree plantations which were weren’t introduced until the 1990s. This also relates to the statement on p5231, line 11, describing "two periods of change".

Reply: We now provide more details on the results of the EEMD in section 4.2. (“Long-term trends in precipitation and streamflow”). Two periods of change can be identified, and the strongest decrease in streamflow is observed before the beginning of the 1990s. It is correct that the residual flow trend is rather flat from the 1990s onwards. This does not imply that the pine plantations have no effect on the overall water balance, as the precipitation records shows an increasing trend till the early 2000s (so net effect is negative).

We have provided more information on the rate of change in the text, particularly in section 4.2., and have revised section 5.1. based on your comments.

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4. In the catchment water balance, horizontal precipitation (HP) is ignored because in 2009 the land cover of montane cloud forests, the primary land use where HP occurs, was small compared with the total catchment area. But the change in montane cloud forest land use over the course of the study period (10.9% of catchment area) is larger than the total size of exotic tree plantations in the catchment (5.3%). As the authors

note, previous work has indicated that horizontal precipitation can account for 5% to 20% of total precipitation. If one were to assume HP is equal to 20% of measured precipitation for cloud montane forest and 0 for other land cover, average annual rainfall of 1400 mm, and 10.9% of the watershed was converted from cloud montane forest to other land cover, then the average annual water loss would be close to 30 mm across the catchment, a number that is comparable with the total water loss to ET from tree plantations (Table 3). Would such assumptions be reasonable? This type of sense check would be valuable for the reader.

Reply: In the previous version of the paper, we made abstraction of the "extra' moisture input from interception of fog and clouds, and no quantitative estimate of the contribution of horizontal precipitation exists for the Ecuadorian Andes. We agree with the reviewer that horizontal precipitation can be important, and have adapted our method based on previous work reported by Bruijnzeel (2004, 2006) on interception losses. We now include the potential effect of a reduction in occult precipitation after forest clearance, and have reorganized our discussion accordingly.

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5. Some concerns related to ET: (a) The equation for evapotranspiration for montane cloud forest reduces to the equation for the total catchment water balance (ET = P - WD). Is this an appropriate assumption? Furthermore, reporting values for throughfall and stemflow are unnecessary and confusing. (b) The strong correlation between P and ET in Figure 2 suggests the catchment is water limited (PET > P), with ET reaching values of nearly 3000 mm/yr. However, the authors suggest that plants rarely undergo water stress in this region, potentially suggesting an energy-limited catchment (P > PET). The value of PET is given from INAMHI as 1000 mm/yr which is comparable with average annual P. Each of these scenarios would have different implications for water balance modeling. (c) As a cross-check for the applicability of the models, I suggest comparing results from the two methods for estimating E (direct water balance and hybrid approach on p5229).

Reply: If we would have information on the water yield for a catchment that is only covered by montane cloud forest, we would be able to estimate the ET as P – WD (as suggested by the reviewer). This is not possible in our case-study, as we do not have spatially disaggregated data on the hydrological response of individual land cover type. Therefore, we used a direct water balance to estimate the losses by evapotranspiration of montane cloud forest (see 3.4 Estimation of the long-term water balance), and we calculated transpiration losses, interception evaporation and evaporation from bare soil separately following Fleischbein et al. (2006).

Reply: The long-term average potential evaporation of the area is estimated to be between 1000 and 1100 mm, according to previous records of potential evaporation (as determined by pan evaporation measurements) by INAMHI (2009). Here, we take the potential evaporation (PE) as determined by INAMHI (2009) as the reference crop evapotranspiration, and do not apply an empirical correction factor (of 0.85 based on average wind speed, fetch and humidity following Allen et al., 1998) to convert PE into ETo. The reason is twofold: (1) there is a good correspondence between the long-term ET (1097 mm) as estimated from the catchment water balance and PE (1000 - 1100 mm, INAMHI, 2009), and (2) the validation of evapo(transpi)ration data and models for the Southern Ecuadorian Andes indicates that ETo determined by the Penmann-Monteith method corresponds within 10% with the measured values of PE by INAMHI (Baculima et al., 1999). This estimated crop reference evapotranspiration is lower than the mean annual precipitation over the catchment (Pyr estimated at 1656 mm, see Table 2). At the catchment scale, there is no evidence of water limited conditions from these measurements, although it is possible that water limited conditions occur at certain locations during the dry season. Given that the comparison of long-term ETyr, PE and estimated ET0 do not suggest water limited conditions, we consider our approach of water balance modelling to be valid.

Reply: We have compared the results of the two methods to estimate ET losses in the forest: (1) direct water balance, and (2) so-called "hybrid" approach by Reviewer #3, used for estimation of ET for agricultural land, páramo grasslands and pine plantations. In the latter method, we take Kc for evergreen broadleaf forest = 1 following the literature. The results are very similar, which is logical as the estimate of ETo is very close to the long-term estimate of ETyr derived from the catchment water balance.

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6. In the conclusions, the authors suggest that reductions in catchment water yield could result mostly from increases in tree plantations. I suggest this statement be further placed into context. If a layperson were to read the conclusions, he/she might think that converting montane cloud forest to traditional crops would solve the problem. However, this would be incorrect given the ET model provided by the authors because crops transpire at 95% the rate of tree plantations, so there would be little change in ET after making this land use conversion.

Reply: Good point, and we have adapted our conclusions by including the partial water balance for páramo and cloud forest ecosystems. The latter was adapted based on your suggestions, and shows that a reduction in horizontal precipitation after forest clearance can have important consequences for the overall catchment water budget.

TECHNICAL CORRECTIONS

p5229, line 6: what is meant by "dry vegetation"?

Reply: We have rephrased this sentence into “where ãĂŰEtãĂŮ_yr is the transpiration loss or soil water uptake (mm yr-1), ãĂŰEiãĂŮ_yr the evaporation from wetted vegetation surface (interception evaporation) (mm yr-1)”

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p5229, line 12: While water depth may be a lumped parameter for the catchment, the authors already have a method for calculated precipitation distributed throughout the catchment. Therefore, there is additional information with which to spatially disaggregate P if so desired.

Reply: Correct. We have rephrased this sentence to clarify our idea. As we do not have information to spatially disaggregate ãĂŰWDãĂŮ_yr, we use lumped values for P_yr and ãĂŰWDãĂŮ_yr. “P_yr and ãĂŰWDãĂŮ_yr were established for the entire catchment, as no detailed hydrological information is available to further spatialize ãĂŰWDãĂŮ_yr.”

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p5229, line 15: It’s incorrect to refer to this equation as the Penman Monteith method. The Buytaert et al. (2006) paper references (Allen et al., 1998), which defines a specific FAO Penman Monteith equation for reference ET. The "Penman-Monteith" naming convention refers to estimating reference ET using the Penman-Monteith equation, contrary to this article in which it is retrieved directly from INAMHI.

REply: Correct. We have rephrased this sentence.

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Table 2, Figure 4c, 5, 6: For the figures I suggest using "streamflow water depth" instead of "water depth" in the captions, so readers can flip to the figures without necessarily referring to the text to know what "water depth" means.

Reply: Thanks for this suggestion. We have checked the captions of Tables and Figures, and clarified that we refer to “streamflow water depth” where necessary. Also, we systematically use the term “water depth” in text, tables and figures when we refer to streamflow measurements only.

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Figure 6: I suggest rewording the beginning of the caption to "Rainfall and streamflow with EEMD residual trends" because "residuals in rainfall" could be conflated with subtracting the mean from the rainfall timeseries. Also, I assume the trend in baseflow is not from the EEMD analysis because it is not monotonic. This should be clarified.

Reply: We have reworded the caption as suggested by the reviewer. All residual trends are from EEMD analysis (also the trend in baseflow). It is important to distinguish between the residual and the trend. The residual is always monotonic. However, the trend can be the sum of both the residual and the last IMF(s) (if the latter is/are significant). In this case the trend is not monotonic (as observed in Fig. 6b).

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