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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.

Anonymous Referee #3

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GENERAL COMMENTS

The authors investigate the nature and causes of hydrological changes in an Andean headwater catchment. The importance of this work is clearly demonstrated in the introduction, and what follows is a valuable analysis of climate and land use change as it pertains to changes in catchment discharge. The conclusions can inform watershed management and sustainability of water supply, with implications outside the study area. From my reading of the paper, the primary objectives are to answer the following research questions: have changes in precipitation led to changes in the hydrograph (reductions in total discharge and changes in partitioning between quick flow and baseflow)? What changes in land use have occurred over the period of the study? What changes in land use are most responsible for altered catchment discharge?

The methods and analysis are appropriate given the aim of the study and limitations of data availability. However, the methods rely on a number of assumptions which introduce uncertainty and/or bias that could be affecting the conclusions. In the Specific Comments section I suggest ways the authors can clarify their approach and evaluate some of the uncertainties present in their assumptions. The writing is clear and easy to read, but there are a couple clarification and organizational changes I suggest in the Specific Comments section below.

SPECIFIC COMMENTS

On organization:

- 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.

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

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.

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?

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".

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.

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).

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.

TECHNICAL CORRECTIONS

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

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.

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.

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.

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.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 5219, 2015.