Interactive comment on "The hydrological regime of a forested tropical Andean valley" by K.E. Clark et al.

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General comment: This is a valiant attempt at closing the water budget for a meso-scale (164.4 km^2) Andean mountain catchment subject to significant cloud incidence in eastern Perú. In doing so a combination of ground-based and satellite-based estimates of rainfall inputs (*P*) are used to derive catchment-wide precipitation inputs. In addition, a three-component mixing model involving the stable isotope signatures of wet- and dry season rainfall, cloud water, and streamflow is used to derive the area-wide contribution of intercepted cloud water (CWI) to the catchment water budget yielding a plausible value of 316 ± 116 mm yr^{-1} (10% of rainfall). Instead of estimating actual evapotranspiration (AET) from the annual water budget equation itself, AET was modeled. Pertinently, standard deviations (errors?) were determined for all water balance components. The overall water budget was balanced within ca. 10% although losses (AET + streamflow *Q*) exceeded inputs (*P* + CWI) by ca. 360 mm yr^{-1}. Precipitation inputs (Table S4, excluding CWI) for a 48.5 km^2 headwater basin nested within the larger catchment exceeded estimated *Q* (section 2.2.2) and AET (Table S2, higher stations) by ca. 1245 mm yr^{-1}. Although the authors acknowledge that point rainfalls are likely to have been underestimated because of wind losses around the gauge and the occurrence of inclined rain falling on steeply sloping terrain, the problem was not addressed despite the availability of techniques to correct for this (Sharon, 1980; Holwerda et al., 2006) and successful previous work to this extent in similarly steep and windy cloud forested terrain (Mulligan and Burke, 2005; Schellekens, 2006). Although the model used for AET was shown to give plausible results for transpiration, it remains unclear whether the model is equally good at estimating wet-canopy evaporation. Below, some suggestions are made as to how to test this (if not done already by the authors), for example using the rainfall interception and climatic data collected at comparable elevations near the study area by Gomez-Peralta et al. (2008) and Catchpole (2012), respectively. Based on the seasonal water budget an important groundwater component is inferred to sustain dry season baseflows. In view of the latter conclusion it is somewhat surprising to not see any specific recommendation being made in section 5.2.3 with regards to further investigations into the hydrogeological (and soil physical) characteristics of the substrate, such as saturated hydraulic conductivities of the soil profile and weathered bedrock, as well as specific yields of fractured rock types. Arguably, another important recommendation for follow-up work should be the characterization of the interactions between the topography, wind speeds and amounts of rainfall received on steep slopes of contrasting exposure. Finally, by also sampling throughfall (crown drip) for stable isotope content (next to precipitation and cloud water), alternative estimates for CWI may be obtained using a two component mass balance model for the respective cloud forest types (or the *puna* grass- and scrubland for that matter) in a variant of the wet canopy water budget approach of Holwerda et al. (2006) (cf. Scholl et al., 2011). Nevertheless, this is an important paper that promises to become a classic reference (cf. Zadroga, 1981) in future provided the rainfall issue referred to above can be addressed more satisfactorily. Below, a number of specific comments are offered aimed at further improving the paper.

Specific comments:

1) Page 8604, title: why not use 'catchment' instead of 'valley' to avoid any confusion?
2) Page 8605, 1st paragraph: the importance of the Andean headlands as a whole for the suspended sediment and solute loads of the Amazonian river system is beyond doubt and well referenced here. However, most if not all of the cited work concerns the Andes as a whole whereas the current paper focuses rather on the upper half of the elevation transect where deforestation (and hence sediment production, etc.) is likely to be much less. In other words, much of the sediment etc. encountered in the lowlands derives from largely deforested intermediate elevations. I would welcome some sort of demonstration of the hydrological importance of these more or less pristine headwaters themselves (see also the next comment).
3) Page 8605, line 27: how small an area is covered by TMCF? You may usefully refer here to Mulligan (2010) his Tables 2.6 and 2.7 and to related work on the importance of the Andes to Amazonian hydrology (http://www.policysupport.org/links/waterworld).
4) Page 8606, line 2: rather these forests ARE the valuable and diverse ecosystems. Suggest rephrasing.
5) Page 8606, lines 10-12: The studies cited here all pertain to perhumid locations where CWI proper makes up a small proportion of the total input. In more seasonal and drier areas CWI tends to become correspondingly more important as in Pacific Central and South America (Mulligan and Burke, 2005 – modeled patterns; Guswa et al., 2007 – observations in NW Costa Rica; Marzol-Jaén, 2010 and García-Santos and Bruijnzeel, 2011 – Canary Islands). Idem, lines 12-15: whilst it is true that runoff responds rapidly to rainfall in these wet steeplands, the formulation used suggests that no groundwater recharge takes place ('rainfall is quickly exported') which is obviously not the case. Suggest rephrasing.
6) Page 8607, lines 8-13: you could usefully add a reference here to Guswa et al. (2007) who applied stable isotopes to evaluate the importance of wet season rainfall etc. inputs to subsequent dry season flows (i.e. as in your section 4.4); idem, line 12: suggest to make this a bit more explicit than 'examine ecohydrology' as the latter is a vague

term, e.g. 'plant physiological functioning'. Incidently, Goldsmith et al. (2012) did not even bother to include cloud water in their isotopic study of the inner workings of a Mexican 'cloud forest'. Idem, line 15: you could usefully refer here to the related work of Windhorst et al. (2013) on stable isotopes in rainfall in nearby southern Ecuador.

7) Page 8607, lines 16-23: these are very valid questions, even more so given the paucity of comparable catchment studies for montane cloud forests and the fact that all but one (Schellekens, 2006) failed to address the (gross) underestimation of precipitation inputs properly (Zadroga, 1981; Caballero et al., 2013).

8) Page 8608, line 29: but see Gomez-Peralta et al. (2008) who determined interception losses for a lower montane rain forest at 2468 m and an upper montane cloud forest at 2815 m not too far from the current study area (albeit on leeward slopes) in conjunction with observations by a passive fog gauge and forest LAI. In addition, above-canopy climatic and visibility (i.e. fog) data were collected by Catchpole (2012) at the same locations.

9) Page 8609, lines 1-2: it would be interesting to know why you expect a similarly wide range (2-2000 mm yr^{-1}) for CWI in the study valley (e.g. based on the increase in wind speed with elevation noted by Bendix et al. (2008) for nearby southern Ecuador). Note that the 1000-fold range compiled by Bruijnzeel et al. (2011) included a semi-arid island site and a super-wet and windy coastal site. Leaving these two extremes out (a reasonable proposition for an inland site where rainfall decreases with elevation) the range narrows a little to 50-1200 mm yr^{-1}. Idem, line 5: information on reference evaporation or PET and wind speeds in particular would be helpful (see previous comments on possibly underestimated rainfall inputs). Idem, lines 17-18: for subsequent interpretation of the conclusions information on saturated soil water content and field capacity would be desirable. Suggest including these if available.

10) Page 8610, lines 18, 19, 23, and section 5.1 line 9: suggest to abandon the colloquial term 'met station' (idem in supporting materials and some figure captions). Idem, line 21: for catchment-wide estimates to be truly 'robust' one must include some sort of wind loss etc. correction (see general comment for references).

11) Page 8612, section 3.3: given the allegedly high wet-canopy evaporation component in (some) montane forests (page 8614, lines17-19) it would be good to demonstrate the capacity of the PT-JPL model to adequately predict interception evaporation. Even if such data are lacking from the studied valley itself, the information for the Yanchaga-Chemillen forests obtained by Gomez-Peralta and Catchpole (comment #8) should allow this to be done. On a related note, according to Section S1.3 (line 99) net radiation R_n was obtained from global radiation R_s by simply multiplying the latter times 0.75, 'a typical fraction for tropical forests'. How was this done for the *puna* grasslands (20% of the catchment)? I wonder to what extent the use of a single fraction instead of

regressions for specific cloud forest types (e.g. Motzer (2003) p. 154 for lower montane cloud forest in southern Ecuador: $R_n = 0.63R_s - 16.02$ W m^{-2}; Hafkenscheid (2000) p. 92 for upper montane cloud forest in Jamaica: daytime $R_n = 0.78R_s - 11.10$ W m^{-2}; Holwerda (2005) p.43 for UMCF in Puerto Rico: Rn/Rs = 0.70; idem p. 139 for elfin cloud forest (comparable to sub-alpine cloud forest in terms of stature): Rn/Rs = 0.765.) would affect the results. Inserting a reasonable average value of 150 W m^{-2} gives a lower value of R_n in all but the ECF/SACF case compared to the fixed fraction of 0.75 used in the present study. As such, it cannot be excluded that AET was overestimated by at least 5% (or ca. 45 mm yr^{-1}). This would go some way towards closing the water budget equation more fully.

12) Page 8613, lines 9, 10 and 20 contain both 'annual' and 'yr^{-1}', remove either. Idem, lines 15-16, 'predominantly sourced from 2400 m': suggest rephrasing this awkward wording (e.g. in terms of maximum *P* occurring at ca. 2400 m etc.).
13) Page 8614, lines 1-6: although I do not object to the premature mixing of Results and Discussion here and in lines 20-22 on the previous page one wonders what the value is of comparing baseflow fractions that were determined in an entirely different manner (e.g. the Mexican work applied a straight-line separation at a constant slope of 0.030 mm h^{-1}). Idem, line 11: the cited ET range concerns lower and upper montane cloud forests only whereas ECF/SACF had a much lower value of ca. 545 mm yr^{-1} (Bruijnzeel et al., 2011). Idem, lines 17-19: yes, but this high interception evaporation holds mostly for LMRF and some LMCF only as demonstrated by the throughfall/P ratios for the respective forest types: LMRF ca. 0.70; LMCF ca. 0.80; UMCF ca. 1.0 and ECF/SACF ca. 1.05 (see Bruijnzeel et al., 2011 for details and discussion of the reasons). Idem, line 23: sapflow is measured in trunks (or branches) whereas canopy 'sap flow' (read: vapour exchange) is determined e.g. via porometry/IRGA. Please specify what technique was used.
14) Page 8615, lines 1-2: see comment #11 regarding the possible need to also validate the interception evaporation component.
15) Page 8615, ad section 4.4: in section S1.5, 1st paragraph (lines 160-162) it is stated that there is an isotopic offset between the lower and upper gauging station due to the elevation difference but that this cannot be addressed with the available isotope data for rainfall. Windhorst et al. (2013) determined an altitude effect for $\delta 1^8$O in rainfall between 1800 and 2800 m a.s.l. in southern Ecuador of -0.22‰ /100 m vs. -1.12‰ /100 m for δ^2H which might be used as a further check.
16) Page 8617, lines 4-16: this is arguably one of the most exciting findings of the present work. As indicated in the general comments, a strong check of the plausibility of the presently found CWI may be obtained by applying the wet-canopy water budget

method in the form of a stable isotope mass budget for *P*, fog and throughfall (or net precipitation).

17) Page 8618, lines 14-16: looking at Figure 4 (highlighted bit) it would not seem impossible for soil water storage to be different between the start and finish of the 2010/11 water year although this might increase rather than decrease the residuals. Looking at Figure 3 the initial and final discharge for the studied water year are very much alike so no change in groundwater storage can account for the observed residuals. Even allowing for a potential overestimation of AET (see comment #11) it is most likely that the discrepancy is due to underestimation of *P* which may be remedied as per the general comments. Idewm, line 21: suggest adding a minus sign to 148 mm.
18) Page 8619, lines 1-2: see general comments and #9 regarding the need for wind speed information to address the underestimation of *P*. Again, suggest to make this more explicit in the suggestions for follow-up research later on.
19) Page 8621, lines 4-8: this is a classic example of the traditional 'forest sponge' concept, the loss of which may lead to lowered dry season flows due to reduced infiltration opportunities despite lower vegetation AET after forest removal (cf. Bruijnzeel, 2004).
20) Page 8623, lines 9-10: this may be true or not and therefore needs to be confirmed by hydrogeological investigations (suggest to add to recommendations for further work) but what about valley fills and lower slope colluvial deposits as well as the peaty top layer in the UMCF and any rotten rock just beneath the soil proper? Somehow it seems a lot of water required from 'deep' fractured shales to maintain such baseflow levels.
21) Section S2.1, lines 203-208: you could usefully refer here to the LMWL derived by windhorst et al. (2013) in southern Ecuador which (not entirely unexpectedly) is even closer to the presently found LMWL. Idem, lines 219-220: alas, Guswa et al. (2007) were the first to do this in Pacific NW Costa Rica.

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