General response to reviewer comments

We (K. E. Clark and co-authors) would like to thank the two referees; M. B. Gush and L. A. Bruijnzeel, for their thoughtful and thorough reviews, which we think have considerably helped us to improve our paper during revision. In particular, we have adopted the reviewers' suggestions and now corrected the rainfall estimates for wind speed, revisited the calculation of evapotranspiration, and expanded the consideration of soil water as a temporary store. The revised values bring the annual water budget closer to balance (now balanced to within -1.6±13.7 % uncertainty). We have also addressed the range of other constructive comments during revision. Please see our detailed responses below to the comments and suggestions raised in the reviews. In the response that follows, *italicized text* is the original reviewer comment, and normal text is our reply.

Detailed response to interactive comment from Reviewer #1 (M.B. Gush)

General Comments: This is a well-argued and thought through paper, with results that are comprehensively supported by the data. The authors have competently described the derivation of the water balance for a mountainous Peruvian Andes catchment under Tropical Montane Cloud Forest. The limited hydrological gauging equipment in the catchment necessitated a combination of interpolation, extrapolation and modelling exercises, combined with short-term in-field sampling to derive the necessary data. This introduces a degree of uncertainty, which the authors acknowledge and address. The grammar and overall quality of the paper is of a high standard.

We thank the reviewer for the positive comments on this paper and for the recognition of the challenges inherent in our analysis. The reviewer additionally provided a number of constructive and helpful specific suggestions that we very much appreciated and have addressed in our revision.

Specific Comments:

Page 8612, Section 3.3. Transpiration and evaporation of intercepted water by undercanopy vegetation may contribute significantly to overall forest evapotranspiration. Was this taken into account? If the NDVI estimate was only based on the highest forest canopy layer this may have underestimated overall leaf area of the forest, and hence resulted in an underestimate of ET by the PT-JPL model?

The reviewer raises an important question. Understorey is taken into account by our implementation of the PT-JPL model. The NDVI integrates both overstorey and understorey. If there is not much overstorey, then the NDVI (and PT-JPL) sees the understory. If there is a lot of overstorey, then there is likely not to be much contribution from the understory because of light limitation on energy supply. The nuanced role that an understorey might play in ET is that it probably has a shallower rooting depth than the overstorey, so the transpiration dynamics out of the understory would be a little different than the overstorey, though they would still both be controlled by major climate influences, especially given that this study region is generally pretty wet.

Page 8617, Section 4.5. Is it possible that contributions by cloud water to streamflow may have been pulsed, (i.e. related to specific seasons or climatic conditions where there was significant cloudiness)? If this is the case, then the timing of streamflow sample collections would have been important in order to capture the isotopic signature of the cloud water as it was passing the collection points. Or is cloudiness at the site relatively consistent throughout the year, and hence assumed to be constantly contributing to streamflow to a greater or lesser degree?

The Kosñipata catchment has high cloud frequency throughout the year (Halladay et al., 2012), with the lowest overall cloudiness in the dry season (~55% of the time, but with dry season clouds tending to be low-lying, at ~1800 masl; Rapp and Silman (2014). Since cloudiness is a yearlong phenomenon and not restricted to particular seasons, we think it is unlikely the cloud water (CW) inputs have a strong seasonal bias. Indeed, the isotopic samples we used to infer CW inputs were collected throughout the year (on a weekly to monthly basis), and based on these samples we find that the CW inputs were relatively evenly distributed at ~11±4 % throughout the year measured. If CW input were highly pulsed over monthly to seasonal timescales, we would expect to see evidence in the river water isotope mixing modelling results on an individual and seasonal scale, which we do not. It is nonetheless possible that some pulsed CW input events may occur in the system over short time scales (e.g., days). Mixing occurring as water makes its way into the river means such short-term pulses would not be distinguishable in our analysis but also means that such pulses would not be expected to bias our longer-term (monthly to seasonal timescale) estimates of the amount of CW input.

Page 8622, Section 5.2.2. This argument and equation could potentially be reversed, assuming the 40% volumetric water content of soils, and a soil depth of 0.5 m, to back-calculate Excess Discharge (ED), and the result could be used to justify down-sizing the runoff total in the final water balance. This may contribute to reducing the 10% unaccounted for portion of the water balance.

The reviewer makes an excellent suggestion. Before directly responding to this comment, it is relevant to point out that the second reviewer (L.A. Bruijnzeel) suggested recalculating AET and determining wind-induced rainfall loss. In light of these revised calculations, the water budget residual reduced to -56 ± 469 mm for the study year. During the April to August period, however, there remains an ED of 373 mm. For soil water to account for this flux, ΔV would need to be 75%.

The back-calculation suggested by the reviewer is a useful addition to the manuscript, but the appropriate values should be d = 0.5 m and ΔV = 5, 15, or 0.4 % depending on the location in the catchment, with a basin wide average ΔV of ~13% (see below). Thus, the catchment wide ED stored in soils has the seasonal variation capacity of 65 mm, making seasonal soil water variation more important than we suggested in the previous version of the manuscript (i.e., it may account for as much as 17% of the seasonal ED), but it is still not sufficient to explain all of the seasonal ED. We have revised the text at the end of section 5.2.2 to include this calculation.

More specifically, we are now able to use recently published, more comprehensive papers describing soil water content in soils from the Kosñipata catchment (Girardin et al., 2014; Huaraca Huasco et al., 2014; Teh et al., 2014). Soil water contents were found to range spatially from 32 to 71 % in the three main ecosystem types (Teh et al., 2014), a range than encompasses the 40% quoted in our original manuscript submission but that better reflects natural heterogeneity. ΔV is a measure of seasonal variation in soil water content, which is 5.4% in LMCF, 15% in UMCF and 0.4 % in puna grassland soils (Girardin et al., 2014; Huaraca Huasco et al., 2014; Teh et al., 2014). For the back-calculation, we divided the catchment into the three ecosystem types (Consbio, 2011), carried out the back-calculation for each type, and then determined the catchment wide soil water variation using the areal proportion of each ecosystem type (8.3, 80.6, and 10.1% of the catchment area, respectively).

Page 8623, Section 5.2.3.The authors comment that it is uncertain how the annual water balance derived from the stated observation period (Feb 2010 to Jan 2011) might differ from past or future trends (e.g. based on anticipated landuse changes / climate change). This is largely attributable to the paucity of streamflow data in this catchment, and intensification of streamflow gauging in the catchment may consequently be worth recommending.

We thank the reviewer for this comment. In the section of the manuscript on future recommendations, we now mention that longer-term streamflow gauging in the catchment would help monitor changes in the hydrologic cycle that are expected with climate change.

At the end of the discussion section in section 5.2.3, we have inserted the following text: "conducting long-term streamflow measurements (Larsen, 2000)".

Technical Corrections:

Page 8613, Section 4.1, line 9. Units should be mm, not mm.yr-1 as the sentence has already stated that the estimate is for a single year (2010/2011).

Corrected.

Page 8613, Section 4.2, line 20. Units should be mm, not mm.yr-1 and the sentence could be altered to read "was estimated to have a mean discharge of 14.6±0.7m3 s−1 and runoff of 2796±126mm (Table 2) over the 1-year study period."

Corrected.

Page 8614, Section 4.2, line 1. As above Units should be mm, not mm.yr-1.

Corrected.

Page 8614, Section 4.3, line 9. As above Units should be mm, not mm as the estimate is only for the single study year.

Corrected.

Detailed response to interactive comment from Reviewer #2 (L.A. Bruijnzeel)

We thank the reviewer for such a positive, attentive and thorough review of our paper, including several excellent suggestions that have made our analysis more robust. The time spent on this review is very much appreciated. The suggestions for recalculating AET and determining wind-induced rainfall loss were particularly valuable and have brought the water budget closer to balance.

General comment: This is a valiant attempt at closing the water budget for a meso-scale (164.4 km2) 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 uncertainties on the water budget equation are indeed propagated errors, generally reported as 1 standard deviation in most cases in the manuscript. We have worked to clarify the definition of reported uncertainties in the revised text (specifically at Section 4.1). Standard error was reported in Tables S1 and S5. This was deemed more appropriate as they were multi-year datasets, where each additional year provided another measurement which allowed us to get closer to the true value.

The overall water budget was balanced within ca. 10% although losses (AET + streamflow Q) exceeded inputs (P + CWI) by ca. 360 mm yr-1.

Based on the revised calculations proposed by this reviewer, our estimate of the excess loss is now less than in our original analysis (now -56±469 mm yr-1, or -1.6±13.7 % of the total budget).

Precipitation inputs (Table S4, excluding CWI) for a 48.5 km2 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.

The revised inputs for the headwater basin (Wayqecha) are 2750 mm yr-1 (with wind-loss corrected rainfall at 2519 mm yr-1 and assuming a CWI of 232 mm yr-1 based on estimated CWI for the San Pedro gauging station), and the outputs are 3709 mm yr-1 (Q = 3066 mm yr-1 and AET = 643 mm yr-1). Thus, outputs exceeded inputs by 960 mm yr-1 (35%), but we re-emphasize (as stated in the text) that there are very large uncertainties particularly on estimated discharge for this sub-catchment. We have added a section (S2.2.4: Water budget for Wayqecha) in the Supplement that summarizes the water budget for this sub-catchment.

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).

We would like to thank the reviewer for bringing this previous work on wind speed correction to our attention. We have now used the correction technique presented in Holwerda et al. (2006) to correct for wind losses of rainfall around the rain gauges in our study site. With an average wind velocity of 1.32±0.03 m s-1, we estimate rainfall loss to be 2.50±0.14% of the rainfall per year (See new Table S3). Catchment wide rainfall estimates have been modified to include this rainfall loss. These changes have been made throughout the revised manuscript and in the methods outlined in section 3.1. Also note our further comments below (see comment #10).

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.

This is a very useful comment and has helped better explain our results (summarised in the revised manuscript in section 4.3. The PT-JPL model includes AETi (interception from vegetation) and wet-canopy evaporation in the model. AETi determined from the PT-JPL model was 225±45 mm yr-1, composing 6.6±1.6% of the bulk precipitation (3428±430 mm yr-1) at our site. At another upper montane cloud forest in the Andes, Gomez-Peralta et al. (2008) measured onsite a loss of 7.7% of the bulk precipitation via canopy interception. We are encouraged that the canopy interception losses predicted by the PT-JPL model are similar to those measured at a comparable site. At the end of section 3.3 in the revised manuscript, we have clarified that canopy interception is included in the PT-JPL model, and we have compared AET_i from the PT-JPL model with the onsite measurements made by Gomez-Peralta et al. (2008).

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.

The reviewer raises a good point here, and our discussion has been amended in the revised manuscript to include this idea. The following text has been inserted at the end of the first paragraph in section 5.2.3:

"Further investigations into the hydrogeological characteristics of the soil profile and weathered bedrock, such as measurement of saturated hydraulic conductivity (Larsen, 2000) and specific yields of fractured rock types (Domenico and Schwartz, 1998), would be beneficial in order to understand the role of groundwater in sustaining dry season baseflow."

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).*

These additional recommendations for follow-up work are good ideas and have also been included at the end of section 5.1.

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.

We would like to thank the reviewer for this flattering complement on our work and appreciate the comparison to such as classic paper!

Specific comments:

1) Page 8604, title: why not use 'catchment' instead of 'valley' to avoid any confusion?

We have made this change in the title and throughout the text.

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).

The reviewer raises an interesting question: what is the relative importance of the pristine, high elevation Andes, in comparison to the deforested intermediate elevations in supplying material to the lowland Amazon? While intermediate-elevation sediment sources are important, especially where modified by land use, the steep slopes of the high elevations are prone to landslides (Clark et al., in prep-b; Clark, 2014) that may be significant sediment sources (Blodgett and Isacks, 2007). In related work, we have estimated that area-normalized sediment yields in the Kosñipata are ~900 t km-2 yr-1 (Clark et al., in prep-a; Clark, 2014) which compares to 1000 t km-2 yr-1 measured at the Madeira gauging station at 200 masl (Guyot et al., 1996). The similar specific sediment yields suggest that the pristine high eastern Andes do produce sediment at a rate within the same order of magnitude as these intermediate areas. We also note that model-predicted erosion rates for the upper Madre de Dios basin (which encompasses the Kosñipata) are high (several mm/yr) throughout the basin (Lowman and Barros, 2014).

Although we view this question as somewhat beyond the scope of the analysis in this paper, we revised the 1st paragraph of the Introduction to explicitly mention the relative importance of steep high elevation slopes as sources of material transported into the lowland Amazon.

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).

We agree this is an interesting question, but we were not able to find previously published estimates that would be appropriate to cite in this context in our revision. We view this as an interesting prospect for future work but after careful consideration have decided not to include a consideration of TMCF extent in the present paper, since we lack robust constraints and feel that it is ultimately outside of the main scope of our analysis in this study.

4) Page 8606, line 2: rather these forests ARE the valuable and diverse ecosystems. Suggest rephrasing.

This sentence has been changed to: "The hydrology of TMCFs is of particular interest because these forests are valuable and diverse ecosystems"

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.

We thank the reviewer for noting this overgeneralisation and have revised the text in response (Section 1 at the end of the 2nd paragraph), to now read:

"In many perhumid TMCFs, CWI makes up a smaller proportion of the total input (Holwerda et al., 2010a; Holwerda et al., 2010b; McJannet et al., 2010; Schmid et al., 2010; McJannet et al., 2007; Eugster et al., 2006). In more seasonal and drier areas CWI is a more important component in the water budget (García-Santos and Bruijnzeel, 2011; Marzol-Jaén et al., 2010; Guswa et al., 2007; Mulligan and Burke, 2005).

Additionally, we have removed the sentence about rainfall quickly being exported.

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.

This is a helpful reference; we have modified the text to address the reviewer's suggestions and have included the suggested additional references (Section 1 at the end of the 4th paragraph).

"Stable water isotopes have been used in studies of cloud forest hydrology for a number of applications, including evaluating the importance of wet season precipitation in supplying dry season streamflow (Guswa et al., 2007), estimating local water recycling (Scholl et al., 2007; Rhodes et al., 2006), determining temporal and spatial variation of rainfall (Windhorst et al., 2013), tracing water paths through soil layers in a catchment (Goller et al., 2005), identifying water sources in stormflow (Muñoz-Villers and McDonnell, 2012), estimating water mean transit time (Timbe et al., 2014), exploring seasonality in ecohydrologic processes (Goldsmith et al., 2012), and examining the interaction between fog and vegetation (Dawson, 1998). Here, we extend this application to constrain the contributions of different precipitation sources to annual streamflow, and in the process we add valuable new water isotope data for a widely studied TMCF in the Andes (Windhorst et al., 2013)."

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).

We have added a sentence to the text at this point in the manuscript to re-emphasize the lack of previous work along these lines.

"There are few studies evaluating the water budget in a similar TMCF (Caballero et al., 2013; Schellekens, 2006; Zadroga, 1981), with only a few which comprehensively estimate precipitation inputs (Schellekens, 2006)."

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.

The reviewer is thanked for these recommendations; the manuscript has been revised in section 2 in the middle of paragraph two to mention the Gomez-Peralta et al. (2008)study.

"In the Yanchaga-Chemillen forests in the central Peruvian Andes, near the Kosñipata catchment, interception evaporation in the UMCF contributed 210 mm yr^{-1} or 7.7 % of the bulk precipitation, and LMCF contributed 660 mm yr^{-1} or 33% of the bulk precipitation input (Gomez-Peralta et al., 2008)."

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.

We have corrected the range reported in the text to read 50 to 1200 mm yr-1. Appropriate references for evaporation, wind speeds, and soil water content are provided in our Methods, Results, and Discussion sections.

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).

This is a good point; "met station" has been changed to "meteorological station" throughout the manuscript and supplementary materials.

As described above, in our revision, we have now included a wind-loss correction for the estimates of catchment wide rainfall. This correction was estimated at 2.50±0.56% yr-1 using the method described by Holwerda et al. (2006). Appropriate revised text is inserted at the end of section 3.1 and catchment wide rainfall has been corrected throughout the text.

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.

These are important and pertinent points. We have responded to the wet-canopy evaporation question in the general commentary section above, and in reply to comment #8. The PT-JPL model does include AETi (interception evaporation). Fisher et al. (2008) did not test just the interception evaporation component against data because appropriate data were not available at the time of their publication. But the total ET measured at FLUXNET sites includes that component, and PT-JPL does well. Additionally, as mentioned in the text, PT-JPL has been the most widely tested ET model at tropical sites. In Gomez-Peralta et al. (2008) there is a loss of 7.7% (210/2753) of bulk precipitation via canopy interception (this value increases to 30% in the lower region of their study site). At our study site, given our revised AET (see comment on revised calculation, immediately below), this would equate to a loss due to AETi of 6.6% (225 AETi/3428) of total bulk precipitation (CW + R + wind-induced rain loss). We have added relevant text at the end of section 3.3.

On a related note, according to Section S1.3 (line 99) net radiation Rn was obtained from global radiation Rs 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: Rn = 0.63Rs – 16.02 W m-2; Hafkenscheid (2000) p. 92 for upper montane cloud forest in Jamaica: daytime Rn = 0.78Rs – 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 Rn 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.

We appreciate the reviewer raising this question. In the revised manuscript, we still use the PT-JPL model, but we have changed the way we have determined AET based on the reviewer's helpful comment. As a result, the catchment wide AET is now 688±138 mm yr-1 (rather than 909±182 mm yr-1, as in our original submission) for the Kosñipata catchment at San Pedro. The modified calculation uses variable Rs/Rn fractions depending on the ecosystem type (as suggested by the reviewer; see below) and uses meteorological station measured PAR or R_s instead of the GL model derived R_s. Additionally, we have remapped the puna grassland based on Clark et al. (in prep-b), with the revised area covered by puna now ~7.3% of the basin at San Pedro (see revised Table 1). As per the reviewer's suggestion we divided the catchment into different ecosystem types (puna/transition, UMCF, and LMCF/LMRF) and ran the model for each ecosystem type using available meteorological station data (Table S2) and published Rn/Rs values for comparable ecosystems (Malhi et al., 2010; Fisher et al., 2009; Gilmanov et al., 2007; Holwerda, 2005). We agree that this new approach is more appropriate than our original analysis. The revised values have been updated throughout the text, and an explanation of the new method is located in the supplementary section S1.3. These changes have resulted in a reduction of AET by 24% (equivalent to ca. 221 mm yr-1).

In greater detail, to avoid confusion and to evaluate different ecosystem types, we estimated meteorological station R_n by multiplying R_s by either: 1) 0.5 for transition/puna grasslands (Gilmanov et al., 2007) from > 3450 masl, covering 10.1% of the valley, 2) 0.7 for UMCF (Holwerda, 2005) from 2000 to 3450 masl, covering 80.6 % of the valley, or 3) 0.75 for LMCF and LMRF (Fisher et al., 2010; Malhi et al., 2002) ranging from 1350 to 2000 masl, covering 8.3 % of the valley. In the absence of data for mountainous grasslands, we have adopted the values from Gilmanov et al. (2007) from European grasslands as the best estimate for puna regions in our study site. The elevation ranges for each ecosystem type were derived from Consbio (2011) and from the ecosystem type at the meteorological stations (Table S2). There were two stations in the puna/grassland ecosystem type, three in the UMCF type, and two in the LMCF/LMRF. This breakdown was also used in the revised proportional based calculation to determine catchment wide variation in seasonal soil water (Eq 2) (see general comment by Reviewer #1 and revised text in section 5.2.2).

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.).

Duplication of "annual" and "yr-1" has been corrected. We have also rephrased the awkward worded sentence as follows: "Although most of the catchment area is located at mid to high elevation ranges (~2400-3400 masl), maximum rainfall occurs at ~2400 masl (Fig. 2c)."

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.

These are a helpful set of suggestions. We have removed the reference to the study by of Muñoz-Villers and McDonnell (2012) in Mexico at this point in the manuscript.

The evapotranspiration (ET) range has been changed to include the lower ET estimate from the literature of 545 mm yr^{-1}, as follows: "In previous work in lowland tropical forests, AET was estimated to be 1000-1300 mm yr^{-1}, while TMCF has lower ET values, between 545 and 1200 mm yr^{-1} (Bruijnzeel et al., 2011)."

We have removed the comment about increased interception evaporation.

The sap flow was measured in the trunks using the trunk segment heat balance method (van de Weg et al., 2014), as we now mention more clearly at the end of section 4.3:

"Sap flow was measured in tree trunks in the Wayqecha forest plot (2900 masl) for one month from mid-July to mid-August 2008."

14) Page 8615, lines 1-2: see comment #11 regarding the possible need to also validate the interception evaporation component.

See our response to comment #8, above; we view the similarity of our model results to the empirical observations at a comparable site as encouraging validation of our interception evaporation.

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 δ18O in rainfall between 1800 and 2800 m a.s.l. in southern Ecuador of -0.22‰ /100 m vs. -1.12‰ /100 m for δ2H which might be used as a further check.

No clear correlation exists between elevation and δD in our precipitation data likely due to the fact that we do not know the precipitation volume for each sample and cannot appropriately weight the data when calculating a mean. Recently, Ponton et al. (2014) published isotopic lapse rates for our study site based on the analysis of small streams that drain only a narrow range of elevations (interestingly, the observed lapse rate in this study in Peru is much lower than found by Windhorst et al. (2013) in Ecuador). Effectively, the small catchments sampled by Ponton et al. (2014) act as large rain collectors and thus better reflect the mean isotopic composition of precipitation for a given elevation than our individual rainfall measurements. However, we do not know water transit times for each of these small catchments, so we do not exactly know over what timescale precipitation is integrated. Based on two different sampling campaigns in Ponton et al. (2014), seasonal variation in the isotopic lapse rate is evident. From the difference in median elevation between the catchments at Wayqecha and San Pedro, we calculate expected isotopic offsets of 11 ± 1 ‰ and 8.5 ± 1.5 ‰ between the two catchments using wet and dry season lapse rates of Ponton et al. (2014), respectively. These offsets are broadly consistent with our precipitation data and support our suggestion that the observed isotopic difference between the two catchments is due to elevation effects. Yet, without knowing more about the water transit times in the small streams sampled by Ponton et al. (2014), it is difficult to robustly utilize the isotopic lapse rates to extend our mixing calculations to the Wayqecha sampling locality. This discussion and the calculation of expected isotopic offsets have been added to the supplementary section of paper in section S1.5.

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).

We agree that a stable isotope mass balance including analysis of throughfall would be a robust test of our estimate of cloud water interception. Presently, no throughfall water isotope measurements for the Kosñipata are available to perform this calculation, so we cannot include it in our manuscript. Future collection of throughfall would be a good target for future work within our study site. This idea is now included as a recommendation in the discussion of our manuscript at the end of section 5.2.3.

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.

In the revised text we have quantified that seasonal soil water storage variation and estimated that soil moisture could contribute 65 mm yr-1. (Girardin et al., 2014; Huaraca Huasco et al., 2014; Teh et al., 2014), so we would not expect difference in soil water storage to have a significant impact on the water budget from year to year.

The comment about groundwater is helpful and is now included in the revised text (in the Conclusions). See responses to comments #10, #11 and the general comments where we have corrected AET and accounted for wind-induced rainfall loss.

We have added 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.

We have addressed this helpful comment, as described above.

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).

The reviewer is thanked for pointing us to this classic concept; we have added appropriate text at the end of section 5.2.1, as follows:

"This is a good example of the traditional 'forest sponge' concept, and suggests that the loss of forest may lead to lowered dry season flows due to reduced infiltration opportunities despite lower vegetation AET after forest removal (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.

This is another good suggestion. Since the water budget is closed within measurement uncertainty, our interest at this point in the manuscript focuses on how the valley sustains dry season flow, i.e. where is the seasonal excess discharge (ED) coming from? Given that seasonal ED is 373 mm from the wet-dry to the near end of the dry season and the variation in soil water content can account for 65 mm (~17%), there remains 310 mm (83%) of seasonal ED. In section 5.2.3 we have added a comment to note that some water may be stored seasonally in colluvial deposits or within peat or epiphyte vegetation within the catchment, in addition to fractured groundwater, and we have noted that better constraining potential water storage in such reservoirs would be valuable further work.

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.

These detailed and attentive ideas are appreciated. We have added in the two suggestions into the supplementary materials in section S2.1.

"In southern Ecuador the LMWL was found to be even closer to the GMWL (Windhorst et al., 2013)."

"Dxs has been used as a tool to evaluate water recycling in other tropical montane cloud forests (Scholl et al., 2007; Rhodes et al., 2006) and tropical forests such as the Amazon (Martinelli et al., 1996; Salati et al., 1979), to evaluate the sources of fog (Liu et al., 2007) , and to evaluate the contribution of seasonal precipitation to streamflow (Guswa et al., 2007), but as far as we are aware, this is its first use as a fingerprint of cloud inputs to streamflow."

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