

Interactive comment on “Negative trade-off between changes in vegetation water use and infiltration recovery after reforesting degraded pasture land in the Nepalese Lesser Himalaya” by C. P. Ghimire et al.

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Overview:

This is an interesting paper that investigates the long term trade-off between leaving degraded land intact or re-establishing such land with forest cover. The authors successfully used a water balance approach coupled with a detailed assessment of the hydraulic properties of surface and sub-surface soils to show that the small increase in infiltration provided by reforestation was offset by higher evapotranspiration rates. The authors showed that water loss in reforested areas were substantially higher than that of degraded pasture and natural forest. This work has significantly important regional implications as the hydrological processes in the catchments of the Himalayas are poorly understood, despite the fact that these catchments supply water to millions of people. The results presented by the authors implore the need for better forest management in order to ensure that sufficient runoff be maintained through the dry season. The manuscript further adds to our knowledge of the impact of reforestation on the water availability, particularly in tropical regions, where such long term studies are generally very limited. I recommend this manuscript for publication. However, there are a few areas that must be addressed including minor grammatical and structural changes which should improve the quality of the manuscript.

Reply: We sincerely thank Reviewer#2 for the detailed reading and valuable suggestions. In the following, we respond to each individual comment. Line numbers pertain to the revised manuscript.

Specific comments:

- 1) P3439. L27. It may be a good idea to include examples of the unsustainable forms of land use.

Reply: We have added a few examples to this extent (lines 104-105).

- 2) P3440. L14-16. Can it be specified if the natural forest or the plantation trees have access to groundwater.

Reply: This general remark (and the two cited Indian examples) in the Introduction pertain to areas that are much less steep than the Himalayan sites studied in the current paper where groundwater is found mostly in valley bottoms and depressions while on steep slopes perched groundwater tables are typically transient and occurring during very wet periods only (Hessel et al., 2007). In view of the conclusion of Andermann et al. (2012) that groundwater in fractured rocks in the Himalayas is a significant contributor to baseflows and in view of the fact that the roots of the studied forests extended into the underlying bedrock (see site descriptions, lines 299-301, 314 and 337-338) it cannot be excluded that the trees had access to some groundwater in rock fractures during non-monsoon times. However, given the low

transpiration totals that were obtained (Table 1 and Ghimire et al., 2014b) it is unlikely that large volumes of groundwater were involved.

3) P3440. L17-18. Please indicate if the infiltration capacity is limited to surface infiltration or flow through the entire soil profile.

Reply: We meant surface and near-surface conductivities and have revised the text accordingly (line 126).

4) P3441. L16-21. These studies suggest that infiltration rates were improved as a result of reforestation. Can more detail information regarding the increase in infiltration be provided?

Reply: Indeed, the cited positive trends in baseflow over time since reforesting severely degraded land do suggest an increase in infiltration capacity in due course following reforestation. We have provided the respective references to these studies to which the interested reader can refer for more detail. However, it should be noted that none of the cited references provides such detail which is why we refer to observed changes in stormflows and baseflows. Information on infiltration capacity changes for one of the cited studies (Krishnaswamy et al. 2012/13) can be found in Bonell et al. (2010).

5) P3445-3446. L5-27. This is a very well described section of the land use. However, I do suggest that many of these characteristics can be included in a table, which makes comparison among sites very easy. I would also suggest the inclusion of site specific meteorological data as this is absent from the text.

Reply: Since the degraded pasture does not have any quantitative vegetation characteristics as could be tabulated for the two forest sites, we had decided against using such a table. As for specific meteorological values, we measured rainfall at all three sites and the overall distribution of daily rainfalls was not significantly different between sites. A crossed sensitivity analysis using daily rainfall data from either forest plot to estimate the effect of potentially contrasting rainfall amounts between sites on (modelled) rainfall interception did not yield any noticeable effect (Ghimire et al., 2012). We have added a sentence to this effect in Section 2.2.2 (lines 380-384). Remaining climatic variables were measured at the degraded pasture site only (lines 344-345). Since the investigated sites were close to each other and were at comparable elevation therefore overall environmental conditions in the studied sites are likely quite similar. Therefore, with all due respect, we decline the idea to add a separate table with climatic characteristics (see also our response to comment #2 of Reviewer #1).

6) P3446. L14. Suggest the inclusion of the elevation of the pine forest site.

Reply: Good point, we have added the elevation of 1580 m (line 317).

7) P3447. L23. Suggest providing a description of the plot dimensions for each forest site.

Reply: Apologies for the partial omission. These have been added in Section 2.1 rather than 2.2.1 (line 284 for natural forest, 304 for pasture and 318 for pine forest).

8) P3448. L8. Suggest including the depth of the gutters in the description of the dimensions.

Reply: Although gutter depth is not required for the conversion of recorded throughfall volumes to equivalent depths in mm we have added this information (15 cm, line 367) to alleviate any fears of splash-out effects that might lead to underestimation of measured throughfall.

9) P3450. L24. Authors should be cautious with the wording, as it suggested that K was measured over the entire hillslope. Suggest including the number of point measurements that were made and then used to estimate the hillslope scale K.

Reply: Although in view of the large numbers of replicates it is probably fair to say that saturated hydraulic conductivity was measured at the hillslope scale, we have removed this indication to avoid further confusion. We have added the (large) numbers of point measurement in Section 2.2.3 (lines 454-456). For brevity's sake we have referred to Ghimire et al. (2013) which describes the measuring procedures and sampling strategy in some detail.

10) P3454. L15. Suggest that IOF is changed to HOF, which is more commonly used in the hydrology literature and is easily recognised.

Reply: The term IOF has been used in several recent related key papers (e.g. Bonell et al., 2010). Moreover, IOF is equally easy to understand for the non-hydrological reader as the term HOF which requires an explanatory definition with appropriate references. As such we prefer to retain the term IOF as is.

11) P3454. L20-21. This is a good assumption. Does the soil moisture data from the TDR probes support the assumption?

Reply: Although not presented in this paper, our soil moisture data do support this assumption as the soil down to 75 cm depth remained at near-saturation level during most of the rainy season (Ghimire et al. (2014b), particularly during times of rainfall. Similar findings were reported by Hessel et al. (2007) for the Indian Middle Mountain Zone (new reference added to the paper).

12) P3455: L2-5. Looking at Fig 3, the K at the surface of the pasture and pine forest soils are significantly lower than the R15max. This suggests that under the rainfall conditions being referred to that water should not infiltrate through the surface or the 0.05-.15 m layers as has been suggested. The authors may want to review this and provide a clear description of which events would percolate through the soil. The perched water table should not develop if just below this layer the K increases to values above the R15max (L11-13).

Reply: This is a very important point indeed and we thank the Reviewer for pointing this out although we referred to this later on in the original manuscript (p.3455, lines 13-16). The main 'throttle' to percolation at the DP and PF sites lies at the surface rather than deeper in the profile as would normally be the case. It follows that the maximum amount that can percolate through to the 0.05–0.15 m layer equals the surface intake rate at these sites which does not exceed the K_{fs} at 0.05 m depth and therefore does not create a perched water table at that depth. We have adjusted the text accordingly, also later on in this paragraph (lines 567-570). We also deleted lines 10-16 on p.3455 in the original ms.

13) P3461: I agree with the recommendations for a multi-use forest system in these regions. I would like to suggest that something be included about the recommendations for this local area under investigation, particular what can be done in terms of reforestation i.e. natural vs. pine and also what approaches may be used to better manage the pine forest i.e. slow re-introduction of the native trees to the pine forests.

Reply: The Dhulikhel area may be considered typical of much of the Middle Mountain Zone in Central Nepal which is precisely why the Jikhu Khola watershed has been the location for much environmental research activity (Merz, 2004 (op. cit.); Schreier et al., 2006 (op. cit.)). As such, there is no need for any particular restoration techniques that would set this area apart from others (as suggested somewhat by the reviewer's phrasing). Because pines as pioneer species are well adapted to the harsh conditions prevailing

on south-facing slopes in the region they have been (and may remain) the genus of choice for reforestation in the Middle Mountain Zone. Attempts to regreen severely degraded slopes in the Jikhu Khola area with mixed broad-leaved species have met with limited success (Schreier et al., 2006) but as shown in a companion paper (Ghimire et al. 2014a), pine plantations can have a well-developed broad-leaved understory as long as the latter is not harvested intensively or grazed. Likewise, reducing fuelwood and litter collection intensity should have a beneficial effect on forest hydrological functioning but this requires their replacement by alternative energy sources (e.g. biogas for cooking stoves) (cf. Schreier et al., 2006). We believe that a more detailed discussion of the topic of forest restoration at this point is beyond the scope of the paper and might detract from the key message that for improved low flows, (deep) infiltration will need to be improved sufficiently to compensate the higher water use of planted trees. This is why we have left it at the suggestion that agro-forests having lower water use yet adequate soil protection ability may become a viable alternative for the currently used pines. We have provided several key references that the interested reader may consult. We have also incorporated the above points in the Discussion (lines 750-751 and 762-765) and added several references including Ghimire et al. (2014a) which discusses these matters more fully.

Technical corrections:

14) P3441. L5. Suggest removing the word “become”.

Reply: Done (line 142).

15) P3442. L15. Suggest removing “reviewed by”.

Reply: We prefer to retain this as is because it was meant to avoid the suggestion that the two cited references constitute the only examples of hydrological impact studies in the Himalayas (line 201).

16) P3443. L23-24. Suggest starting with the vegetation found at lower elevations then moving to vegetation at higher elevations. Also, since *R. arboreum* is occasional, can this be removed unless it is an important species?

Reply: For the first part of the comment: Indeed, there was a mistake in the original formulation; the evergreen mixed broad-leaved forest dominated by *Schima wallichii* and various chestnuts and oaks occurs in the elevational belt between 1000 and 2000 m a.m.s.l., not above 2000 m a.m.s.l. We have amended the text accordingly (lines 237-240).

For the second part of the comment: We think it is important to retain *R. arboreum* in the descriptions as 5% of the trees in the natural forest plot were *R. arboreum*. Moreover, we did measure the water uptake rate and stemflow from *R. arboreum* trees (Ghimire et al., 2012, 2014b). As a compromise we have slightly amended the text by replacing ‘occasional’ by ‘as the chief associate species’ (lines 239-240).

17) P3444. L14-15. Suggest removing the sentence “the rainy season (Monsoon) begins early July and ends by late September”, as it essentially repeats the previous line.

Reply: Quite right. Done (lines 257-258).

18) P3453. L18. L24. Suggest removing “some”.

Reply: Done (lines 528, 533).

19) P3454. L24. Change from Fig 2 to Fig 3.

Reply: Correct. Done (line 559).

20) P3454: L25-26. Suggest removing “whereas” and simply state that the 187 mm of overland flow was produced annually. Suggest a similar restructuring for lines 26-28.

Reply: Note that the figure of 187 mm is not the annual total but the seasonal (monsoonal) total only although the difference with the annual total is presumably limited. We have replaced “whereas” by “while the” (line 560)

21) P3455. L9-10. Suggest talking about the limited vertical percolation, which then affects SSF formation and not SSF affecting percolation.

Reply: At the risk of becoming semantic, arguably both versions are right depending on the depth of vertical percolation considered. Limited vertical percolation at a specific depth indeed leads to enhanced SSF above that depth as stated by the reviewer. But these higher lateral subsurface stormflow losses also result in less water being available for further vertical percolation to greater depths and hence to less groundwater recharge. We have replaced ‘and thus’ (limited vertical percolation) by ‘because of’ (line 574) and thus follow the reviewer’s line of reasoning to avoid confusion.

22) P3476. Fig 2. Should the y-axis in 2b read evaporation or evapotranspiration? P3477. Fig 3. This is a very good figure. I would also consider placing the depth on the y-axis and K on the top x-axis, which shows changes in K with depth, similar to those shown in many hydrology articles.

Reply: In response to first part of the comment: the evapotranspiration from the degraded pasture site was assumed to be equal to the soil evaporation as like most of pasture land in the study area the degraded grassland site was heavily overgrazed and compacted and thus the capacity of the grassland to intercept rainfall was considered negligible. Moreover, in view of the minimal live biomass the water uptake of the grass was also considered negligible. We have added an extra sentence in Section 2.2.5 to make this clear (lines 480-483).

In response to the second part of the comment: this is a good suggestion. We have adjusted the diagram accordingly.

23) P3479. Fig 5. Suggest including a description of the arrow connecting over-used to near-undisturbed forest in the figure caption. This would help explain why it is different from the other two arrows.

Reply: The reason for using a broken arrow initially for this land use change trajectory was to indicate that this does not occur in practice. Likewise, because natural forest is not converted to pine plantations directly, no downward arrow was added although the associated hydrological consequences would be the reverse of those indicated for the change from pines to broad-leaved forest in Figure 5. In response to Reviewer’s remark we have reformatted the broken arrow to that of the other arrows in the diagram.

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