Dear Professor Romano,

Thank you for handling our submission to HESS on the hydrological impacts of reforestation of degraded grassland (and subsequent over-intensive usage of the planted forest) in Central Nepal.

We have taken considerable effort to address the points raised by the three reviewers in the revised manuscript and this is reflected by significant amounts of additional text and a revised Diagram. As such, we are confused by your remark considering the need for 'additional refining' and improvement of the 'structural presentation' of the findings, also because we have the impression that you have not seen the actual revision.

In view of the above, the short time available for submitting these minor revisions (one week) and above all the fact that the lead author is leaving for extended fieldwork in Madagascar the day after tomorrow (with intermittent and poor internet access after 4 September) we have ventured to upload the revised manuscript (after going through it once more) for your perusal and trust you will agree with us that every effort has been made to accommodate the main points raised by the reviewers. Although the text describing the research sites could indeed be shortened somewhat by adding a (landscape-format) comparative table listing the main characteristics of two of the sites, we have not opted for this because we do not have as complete information for the third investigated site (degraded pasture) as also communicated in our response to Dr. Farrick.

We look forward to your hopefully positive final decision.

With kind regards

Dr. Chandra Ghimire

**Hydrol. Earth Syst. Sci. Discuss., 11, C758–C759, 2014**

# 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.

**D. Tongway (Referee)**

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HESS Review 010414 David Tongway CSIRO Ecosystem Sciences, Canberra ACT This is a very impressive manuscript, with many very nice measurements and a very useful output summary. I am an applied ecologist interested in taking the findings of papers like this one and developing monitoring systems that local land managers can use to improve landscape functioning by modifying their behaviour. With colleagues in CSIRO, I have developed a monitoring procedure that might be put to use in the lands described in the Ms. This procedure is described in "Restoring Disturbed Landscapes: Putting Principles into Practice" by David J Tongway and John A Ludwig, Island Press, 2011. This book will direct people interested in implementing this landscape function procedure to a website where data sheets and spreadsheets can facilitate self-improvement in management. This procedure in no way calls into question the results of this Ms: rather the Ms underlines the usefulness of the monitoring for people who cannot do the experiments themselves. The functional differences between sites where local people harvest tree litter to the detriment of hillslope functioning might be modified by adopting leaving proportions of the hillslope litter intact. I have appended a few questions which, if attended to might make the Ms a bit more useful to a wide range of readers.

***Reply:*** *We sincerely thank Reviewer#1 for being interested in and recognizing the value of our paper. In the following we respond to each comment one by one. Line numbers referred to pertain to the revised manuscript.*

**1.** When was the pasture created and what has been its grazing use?

***Reply:*** *The exact date when the pasture was created is unknown. However, it has been heavily grazed for at least 150 years according to various local sources as mentioned in the text (lines 305-306).*

**2.** Temperature – evaporation differences?

***Reply:*** *The investigated pine forest and degraded pasture were located close to each other and at almost the same elevation. Therefore, overall weather conditions* above the vegetation *at the two sites are likely quite similar. Although the natural forest site was located at a slightly lower elevation (1500 m vs. 1620 m) and can therefore be expected to be slightly warmer given the local temperature lapse rate of 0.6 $^{o}$C/100 m, such an effect is also likely to be counteracted by the northwestern exposure of the NF site. Note that in view of the difference in slope aspect between the three sites corrections were made for its effect on incoming radiation loads as the main determinant of (dry-canopy) evaporation in the case of the two forest plots. We have added a sentence to this effect in Section 2.2.1 (lines 349-350). Any further differences in evaporation among the two forest sites are largely attributable to differences in Leaf Area Index and tree density through their influence on vegetation water uptake.*

**3.** Any recognition of differences in soil macro-faunal activities?

***Reply:*** *Although no specific observations were made in this respect, the little disturbed natural forest had a well-developed litter layer plus (much) higher soil organic matter content and macroporosity judging by the much higher values obtained for* $K_{fs}$*, presumably due to greater soil macrofaunal activity compared to the planted forest and the degraded pasture. We have inserted an indication of this fact in the revised manuscript in lines 282-284.*

**4.** Need to acknowledge differences in precipitation (amount and rate, esp storms) and temperature between different sites. Maybe a table would assist here. The text is "too global" and doesn't distinguish between sites with very different rainfall/ evaporation/cultural/slope backgrounds.

***Reply:*** *Amounts of precipitation differed little between sites. In an earlier analysis (Ghimire et al., 2012) it was shown that using daily rainfall totals recorded at the two forest sites in an analytical model of interception did not produce a difference in canopy hydrological functioning at either site. As such, we do not feel a detailed discussion of climatic differences between research plots that are located so closely to one another is really justified. For other climatic variables we refer to our response to comment #2. However, we have added a sentence on the similarity of rainfall distributions in Section 3.1 (lines 380-384)*

**5.** Yes, rainfall amounts, relative to soil infiltration capacity need to be more prominent in the text. P 3442, line 10.

***Reply:*** *Information on seasonal rainfall totals in the study area at large is given in lines 257-262. Since it is not the rainfall amounts but rather the rainfall intensity relative to surface- and subsurface hydraulic conductivities that plays a key role in partitioning the rainfall into overland flow, lateral subsurface flow and deep percolation we have added some basic information on rainfall intensities at the study site and their frequency of occurrence early on in Section 2.1 (lines 262-265). Note that Figure 3 also contains information on the median and maximum 5-minute rainfall intensities at Dhulikhel.*

**6.** The objective in p3443, lines 5-8 needs to be related to a time period, as forest maturation takes some time to develop and equilibrate.

***Reply:*** *This is a good point. We have added the age of the planted forest in line 219 as well as extended the central research question to take this into account more fully (line 222).*

**7.** P3443, last lines: need to say what vegetation type has replaced the forest and what soil disturbance has occurred.

***Reply****: As implied by the original formulation, the forest was replaced mostly by agricultural cropping. Soil disturbances associated with the conversion include accelerated surface erosion and increased incidence of shallow landsliding but obviously the actual processes will vary in magnitude and intensity with slope gradient, underlying soil type, and land management (e.g. terracing and terrace maintenance, cover crops, etc.). However, we do not feel such degree of detail is necessary in a general description of the study area, more so because detailed descriptions of the study plots are given later.*

**8.** P3444, line 16: delivered to what exactly – a gauged stream perhaps?

***Reply****: The term 'delivered' was simply used to denote rain falling. We have revised the text to avoid any possible misunderstanding (line 196, idem in line 260).*

**9.** Can differences in clay and sand content be attributed to differential erosion or just to innate differences?

***Reply:*** *In view of the fact that the average silt contents of the soil at the three plots were very similar at 46% in the natural forest, 45% at the degraded pasture and 42% at the pine forest it would seem unlikely that differential erosion is important. In addition, the respective soil textural components at 0.5 – 1.0 m depth underneath the DP and PF are essentially the same whereas sand content at this depth beneath the NF is much lower and clay content much higher. It is concluded therefore that the somewhat different texture of the soil under the NF reflects a less sandy bedrock type (see Ghimire et al., 2013 for additional soils information with depth). We have added a reference to the latter paper (lines 301-302) for the interested reader.*

**10.** I would be interested to know to what extent litter harvesting etc gives rise to hardsetting or physically crusted soils, compared to soils where litter is retained and allowed to be decomposed by fungi and soil fauna.

***Reply:*** *The removal of understory vegetation, the regular collection of litter from the forest floor and compaction by grazing cattle and people can all be expected to have an adverse effect on soil faunal activity and biomass and therefore on the incorporation of organic matter, with adverse effects, in turn, on soil aggregate stability, macro-porosity and pore connectivity as demonstrated by the infiltration and hydraulic conductivity measurements. Whilst it is possible that the erosive force of drip from the canopy (which typically is much greater than that of rainfall in the open) falling onto bare soil patches has led to hard-setting, slaking and crusting of the topsoil it is impossible to separate this effect from that imposed by multi-decadal grazing and trampling without additional detailed soil physical analysis which is beyond the scope of the present paper. Rather, the emphasis of the current paper is on the net effect of the various processes (i.e. reduced infiltration and percolation upon advanced soil disturbance). A similar case of multi-decadal intensive plantation forest usage not far from the Dhulikhel study sites is discussed in Ghimire et al. (2014) to which we refer repeatedly in the Discussion section.*

**11.** Is pattern of runoff examined outside the plot outflow context? Is runoff all inter-rill or are there rills and gullies involved?

***Reply:*** *The overland flow in the more degraded parts of the study area (DP, PF) was largely concentrated in ephemeral micro-drainage networks around vegetation objects such as grass tussocks and the like rather than in well-developed rills and gullies. The connectivity of hillslope overland flow to the stream network in the area is mostly via the numerous footpaths as discussed more fully in Ghimire et al. (2013). Again, a detailed description of surface runoff processes in the current paper would detract from its central focus as per the previous response.*

**Cited references:**


Ghimire CP, Bruijnzeel LA, Lubczynski MW, Bonell M. (2012). Rainfall interception by natural and planted forests in the Middle Mountains of Central Nepal. *Journal of Hydrology* 475: 270-280. DOI: 10.1016/j/jhydrol.2012.09.051.

Ghimire CP, Bonell M, Bruijnzeel LA, Coles NA, Lubczynski MW. (2013). Reforesting severely degraded grassland in the Lesser Himalaya of Central Nepal: effects on soil hydraulic conductivity and overland flow production. *Journal of Geophysical Research, Earth Surface*, 118, 1-18, doi: 10.1002/2013JF00288, 2013.

Ghimire CP, Bruijnzeel LA, Bonell M, Coles N, Lubczynski MW. (2014). The effects of sustained forest use on hillslope soil hydraulic conductivity in the Middle Mountains of Central Nepal. *Ecohydrology* 7, 478–495.

Hydrol. Earth Syst. Sci. Discuss., 11, C997–C1000, 2014

# 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.

**K. Farrick (Referee)**

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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 RI5max. 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 reforesting 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:*** <u>*For the first part of the comment*</u>*: 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).*

<u>*For the second part of the comment*</u>*: 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:*** *<u>In response to first part of the comment</u>: 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).*

*<u>In response to the second part of the comment:</u> 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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Ghimire CP, Bruijnzeel LA, Lubczynski MW, Bonell M. (2012). Rainfall interception by natural and planted forests in the Middle Mountains of Central Nepal. *Journal of Hydrology* 475: 270-280. DOI: 10.1016/j/jhydrol.2012.09.051.

Ghimire CP, Bonell M, Bruijnzeel LA, Coles NA, Lubczynski MW. (2013). Reforesting severely degraded grassland in the Lesser Himalaya of Central Nepal: effects on soil hydraulic conductivity and overland flow production. *Journal of Geophysical Research*, *Earth Surface*, 118, 1-18, doi: 10.1002/2013JF00288, 2013.

Ghimire CP, Bruijnzeel LA, Bonell M, Coles N, Lubczynski MW. (2014a). The effects of sustained forest use on hillslope soil hydraulic conductivity in the Middle Mountains of Central Nepal. *Ecohydrology,* 7, 478–495.

Ghimire CP, Lubczynski MW, Bruijnzeel LA, Chavarro-Rincón D. (2014b). Transpiration, canopy conductance and decoupling coefficient of two contrasting forest types in the Lesser Himalaya of Central Nepal. *Agricultural and Forest Meteorology*, 197, 76–90.

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**Hydrol. Earth Syst. Sci. Discuss., 11, C2810–C2812, 2014**

**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.**

**M. van Noordwijk (Referee)**

M.vanNoordwijk@cgiar.org

Received and published: 30 July 2014

As others have stated this is an impressively comprehensive empirical effort on a topic that remains a major challenge at the interface of public-policy discourse and hydrological quantification. My only comments are that authors might be more explicit in the fundamental assumption for much of this work that rainfall is not influenced by land cover change – this is verified at the scale of paired catchments but might not hold at wider landscape scale. Stating the assumption in the introduction and returning to thepoint in the discussion will further increase the value of this study, which I read with interest.

*Reply: We thank Dr. van Noordwijk for the compliment and interest in our paper. With regard to the suggestion to include any effect of (large-scale) reforestation on landscape-scale precipitation, we hesitated to include this aspect originally, precisely because of the contentious nature of the topic as well as the lack of empirical evidence for such an effect. We will come back to this point when addressing comment #2 below.*

**Specific comments and suggestions**

(1) p3439 1. Introduction 1st sentence: The sentence is a bit difficult, Please split up in parts. There had been serious debate on this in the 1920'/'30's. A 'political ecology' interpretation of that debate between foresters and engineers is provided by: Galudra, G., & Sirait, M. (2009). A discourse on Dutch colonial forest policy and science in Indonesia at the beginning of the 20th century. International Forestry Review, 11(4), 524-533.

*Reply: We have broken up the sentence and have included several references to the long-term nature of the debate, including that in Indonesia as per the Reviewer's suggestion (also referred to by Bruijnzeel, 2004) (lines 73-77).*

(2) p3439 line 15. Somewhere here you might allude to a central assumption in these discussions of "hydrology given rainfall": if rainfall does respond to land cover at a scale above that of a paired catchment experiment, conclusions may need to be reconsidered. A number of recent analyses challenge the assumption of 'no effects'.

*Reply: Rather than introducing the impact of reforestation or deforestation on rainfall at larger scales at this point in the paper we have added a qualifier regarding observed changes in water yields across scales from small catchment areas up to large river basins of* *which several examples are given (lines 86-89). The rainfall issue is introduced in lines 161-177 just before zooming in on the Himalayas and revisited in Section 4.3 (lines 720-729).*

(3) 3440 line 9. This discussion might include reference to increased drainage of landscapes by roads etc that tends to coincide with loss of forest cover and soil changes. This point tends to be missed in reforestation efforts that often further increase drainage, rather than block surface pathways for water

*Reply: Yes, this is a valid point as roads not only act as rapid conduits for overland flow towards streams but may also intercept subsurface stormflow from upslope. We have included roads and other impervious surface areas in lines 106-108 and touch upon this aspect again in Section 4.1 (lines 627-630).*

(4) 3443 The central question probably was: "Does current reforestation ... restore..." This informs a discussion on whether it "could" if differently designed/implemented.

*Reply: Actually, when the project was conceived, we were unaware of the poor state of the reforestation in Central Nepal. Rather, we aimed at following up the initial improvement in hydraulic conductivity ($K_{fs}$) demonstrated 12 years after the pine trees were planted (Gilmour et al., 1987). By revisiting their sites after 25 years an observation of improvement in 'real time' would be obtained. Instead, conditions had worsened so much by over-intensive usage of the forest in the meantime that $K_{fs}$ in the 36-year-old stand were poorer than at 12 years after reforestation (Ghimire et al., 2014). It would therefore not be appropriate to rephrase the central question as proposed by the Reviewer although we have added an indication of the time frame as per the request of Reviewer #1 (line 222).*

(5) 3456 "As long as rainfall intensities remain below the surface Kfs threshold for overland flow to occur, soil water reserves are being recharged." missing something like "independent of the surface Kfs value"

*Reply: With all due respect, we do not see the added value of adding such a qualifier because the statement will remain true anyway. At the same time, advanced surface degradation and therefore a corresponding reduction in surface intake capacity, will limit the amount of water left for percolation into the soil after which further movement is dictated by subsurface $K_{fs}$ values.*

(6) 3457 line 3: Root turnover has been found to be an important contributor to macroporosity as well. In some situations surface sealing (slaking) dominates over soil macroporosity effects per se, but any litter (soil cover) can rapidly reverse this, while macroporosity takes more time to get back.

*Reply: This is a valid point. In view of the expectedly long time involved in creating macropores via root mortality we have added a reference to the role of soil insects as well as root turnover (lines 624-625).*

(7) 3460 line 15. Maybe worth repeating the 'rainfall effects' caveat here.

*Reply: A brief discussion of possible effects on rainfall has been added here including a discussion of the Himalayan setting (lines 720-729).*

**Cited references**

Ghimire CP, Bruijnzeel LA, Bonell M, Coles N, Lubczynski MW. (2014). The effects of sustained forest use on hillslope soil hydraulic conductivity in the Middle Mountains of Central Nepal. *Ecohydrology* 7, 478–495.

Gilmour DA, Bonell M, Cassells DS. (1987). The effects of forestation on soil hydraulic properties in the Middle Hills of Nepal: A preliminary assessment, Mountain Research and Development, **7**(3), 239–249.