

Interactive comment on “Do Himalayan treelines respond to recent climate change? An evaluation of sensitivity indicators” by U. Schickhoff et al.

1) Comments from referees

Anonymous Referee #1

Received and published: 20 November 2014

This multifaceted review provides a well-differentiated picture of the Himalayan upper treeline. The paper may be considered as a consequent follow-up of another paper by Schickhoff published in 2005. The recent paper is mainly based on the first authors own extensive investigations in many different regions of the Himalayas and on a profound evaluation of the literature. As in many other high-mountain areas of Eurasia already settled in prehistoric times, the present altitudinal location of the Himalayan treeline must mainly be attributed to human impact. Thus, explanation of treeline response (spatial pattern, succession, establishment of new trees) to the warming climate is still questionable. The treeline has been considerably lowered by human impact, and is therefore located far below the mountain tops and crests. Thus, also the lack of climatically shaped growth forms may be explained by this low and fairly wind-protected treeline. Anyway, this article is an essential contribution to a better regional differentiation of the Himalayan treelines and treelines in general. The paper is well-organized and well-illustrated with good maps and figures. However, those who are not familiar with this mountain region will miss some instructive photographs.

Anonymous Referee #2

Received and published: 10 December 2014

The paper presented by Schickhoff et al., represent a review of tree lines studies in Himalaya arc. Throughout field survey and experiment measurement of seedling at different plots located in east-central Nepal and along altitudinal gradient; authors report data about the typology of tree lines in Himalayan, the capacity of seedling/germination. Based a compressive review of existing publication, authors additionally provide evidence about the response of tree to climate as well as the causes of tree lines shifts. The key issue about if CC is responsible of tree lines shifts is in the ms addressed and partially rejected. Authors suggest, by contrast, that tree lines shifts are related with decline in the land use and call for studies carry out in “near-natural or less disturbed” areas. Therefore, they suggest a low response in those tree lines areas (natural or less disturbed areas) to significant move up. Based on their data, the also report conclusion about the role of site-specific issues controlling seedling. Along the paper, they also suggest some “scientific gaps”. The thematic of the paper fit properly in the target journal and it is generally well writing (although some issues still need to be addressed). The structure of the paper don't follow to a classical study, but a mix between a depth review with new contribution (which include some method). Under my point of view, the ms is quite dense and probably the 2 and 3 could be summarized to make the ms more attractive. Nevertheless, I have some concerns related with the basis to reach the conclusions. To simplify, authors address here the tree line typology, the seed-based regeneration and tree growth-climate relationship. For the first factor, authors report some values (page 14, line 18). I have some concerns about that: first I can not see on which

observation /methods etc.. authors base their observation. This should be clarifying. Moreover, for me it is complex to imagine that in one of the most active neo-tectonic area, where geomorphic processes are well presented everywhere, the % of orographic/edaphic tree lines (related with external process) is so low. Second: the seed-based regeneration: Authors provide data from two new sites, which it is good. However, they say that data in Himalaya is scarcity or “hardly available”. According with their own statement “Pag 17, line 26-28, etc..” where they affirm that many environmental factors, some of them site-specific, my issue here is to really understand if they have enough observation to draw conclusion at Himalaya scale. The same observation can be done to tree growth-climate relationship. Along the ms, authors say that there are not many studies dealing with this issue so far. However, conclusion provided by authors looks so robust. I was wondering if there is room here for doubts...Major revision Specific comments: 1) Short points 1,2,3, specifically 2,3 to be more concise and less repetitive with other sections. Pag 2, line 9: extensive. In the ms is not clear where and “how much” field observation was performed.. page3, line 1-5: this sentence is not clear. Please rewrite it. (from passive to active voice) Pag 3, line8: in this sentence looks like something is missed Page 3, line 24-28: I miss some reference here Page 4, line 13: here there is a room from geomorphic processes? Page 6, line 8-12: This sentence is difficult to follow. It is too large. Page 6, line16: I will not say “infancy” Page 10; line 2 “ Juniperus sp” Idem line 4 Page 11, line 10 : Include some references in this statement? Page 11, lines 10-30: here, there are statements without references. Page 14: line 18: For me this is difficult to accept. I don't know in what studies /observation authors base their observation, but in the Himalayan arc, I am expecting more orographic treelines since this area is completely affected by geomorphic processes (not only snow avalanches). SO I am wondering which data /methods are below this. Page 15, line 4: I don't know in what you base your statement. Reference here? Page 15, line 18: You say that “% anthropogenic tree lines in north-face slopes is slightly lower..” However, I cannot see on what data are your statements based. It is an existing inventory of tree lines, your observation? This should be clarify. Page 15, line 19: “south –east faces” Page 16, line 29, page 17 line 2: What is the role of natural processes as snow avalanches, rock falls etc.. here? Page 20, line 10: include the scientific name Page 21, line 12: Negative correlation: I cannot see quantitatively these values. Page 21 line 17: I cannot see these results. Page 23, line 7: Tree physiognomy is generally controlled by climatic and mostly geomorphic factors. Page 27, line 10-17: this sentence is too long! Page 28, line 18: Schickhoff et al., in review (cite the ms in review) In conclusions, section between 17-27. I think here you need also to highlight the some of the conclusion you are suggestion are based on evidence from existing documents/studies. For instance tree growth-climate relationship. You did not such as analysis, but conclude those trees are high sensitivity??? It is honest indicating that your conclusions are based on previous works, at some stage.

2) Author's response

Author's response to anonymous referee #1
Received and published: 22 January 2015

We agree with the reviewer's assessment that altitudinal location and physiognomy of Himalayan treelines are modified to a large extent by human impact, and that a climatic change signal as driver of treeline dynamics is hard to detect when studying

anthropogenic treelines. We therefore carefully selected study sites in Langtang and Rolwaling Himal where human impact is low (Langtang National Park) or negligible (Rolwaling, here mainly due to low population density and religious reasons) in order to rule out land use effects on treeline dynamics as far as possible. We will include instructive photographs in the revised version.

Author's response to anonymous referee #2
Received and published: 22 January 2015

We appreciate this thorough review and the reviewer's assessment that the topic of the paper does fit properly into the scope of the journal. We comment on the reviewer's comments and suggestions as follows: We will check chapters 2 and 3 once more for potentially summarizing and condensing the information. Basically, we consider the content of chapters 2 and 3 as necessary background information to understand the statements in subsequent chapters, in particular addressing those readers who are not familiar with treelines and climate change in the Himalaya. The quantitative values referring to the occurrence of various treeline types (p. 14) represent best estimates based on extensive field experience in the Himalaya and Karakoram for the past 30 years (first author) or even longer (fifth author). Collectively, the team of authors has acquired intimate regional knowledge along the Himalayan arc, from E Hindu Kush to SE Tibet, during extensive field trips and excursions. According to our observations, topographic settings or morphodynamic processes which potentially affect treeline formation are mainly concentrated in the upper alpine and nival belts, way above the alpine treeline. We rarely found treelines in the field which are prevented from reaching distinctly higher elevations by steep rock walls, talus cones, slope debris and the like. According to our best knowledge, the percentage of orographic treelines will not be substantially higher than c. 15 %. As for the seed-based regeneration, our conclusions of fairly high levels of tree recruitment are not only based on our data sampling and evaluation in Langtang and Rolwaling, but also on the cited studies from the W, Central, and E Himalaya which reflect more or less consistent results. We explicitly express that we consider these results as preliminary evidence regarding the use of recruitment as an indicator of treeline sensitivity (p. 18). As for tree growth-climate relationships, the conclusion of growth patterns in W and Central Himalaya being particularly responsive to pre-monsoon temperature and humidity conditions is indeed robust since it is based on consistent results from a considerable number of studies. The manuscript is currently being revised. During this process we will go thoroughly through the reviewer's specific comments on single pages and lines, and conduct necessary corrections and modifications wherever appropriate.

3) Author's changes in the manuscript

- we added 6 photos, and rearranged figure captions accordingly
- we rewrote the sentence on p 3, l 1-5
- we deleted one word on p 3, l 8
- we added references on p 3, l 24-28
- p 4, l 13: no change necessary
- we rewrote the sentence on p 6, l 8-12
- we rewrote the sentence on p 6, l 16
- p 10, l 2, 4: we prefer to use 'junipers' here for stylistic reasons

- we added a reference on p 10, l 11
- p 10, l 11-30: statements refer to Fig. 2 which has a reference
- p 14, l 18: see author's response
- p 15, l 4: statement is based on the references given
- we changed some words on p 15, l 18-20; the statement results from the preceding paragraphs
- p 16, l 29, p 17, l 2: no change necessary
- p 20, l 10: no change necessary; the scientific name is mentioned in line 6
- we added 'unpubl. data' on p 21, l 12-17
- we corrected the measuring depth on p 21, l 21
- we detected an error in soil moisture correlations in Fig. 4: high field capacity values which actually show low soil moisture had been mistaken as indicating high soil moisture. We corrected Fig. 4 and changed the interpretation accordingly on p 21, l 22ff
- p 23, l 7: no change necessary since we speak of varied site factors
- we pointed to unpubl. data on p 27, l 9, p 28, l 18
- we splitted the long sentence on p 27, l 10-17
- p 30, l 7: we deleted (D.Don) since we have not cited taxonomists throughout the text
- p 31, l 17-27: no change necessary since conclusions are based on both literature review and author's own studies
- we corrected page numbers on p 44, l 6
- we also incorporated all corrections of the proof reading process into the word file of the manuscript