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Interactive comment on "Treeline dynamics with climate change at Central Nepal Himalaya" *by* N. P. Gaire et al.

Anonymous Referee #3

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This manuscript documents regeneration and treeline dynamics over the past 200 years in the Manaslu Conservation Area in central Nepal Himalaya. The aim of the manuscript is to study the dynamic impact of climate change at a treeline dominated by Abies spectabilis and Betula utilis. The authors present very interesting results using a well-documented data set for a unique study site. Nevertheless, the manuscript presents a number of limitations:

- While the area studied in the manuscript is of great importance for its uniqueness and implications under the future climate scenarios, the manuscript presents results only for one study site at a mountain slope with only 2 plots (pseudo replications). For the aim of this study, this must have been replicated otherwise it mainly remains descriptive of one particular site. Treeline dynamics and forest dynamics in general, highly depend

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on several factors such disturbance history and site conditions. As reported in other studies, treeline position and dynamics could vary depending on the aspect (south vs north), elevation, slope, etc. Therefore, the results presented here will just represent the local conditions and history and cannot be used to make broader generalizations or to study the effect of climate variation on the tree growth regeneration of the Manaslu Conservation Area. With this, the study only represents the dynamics of the forests and treeline at that mountain slope, remaining very local and limiting broader implications (such climate change); I found this out of the scope of the Journal. I think this is a great study and the authors must consider submitting it to a more local or ecological journal.

- The authors used the two transect plots "with the hope to learn about lateral migration". I do not see how this sampling design (just 2 plots) can help with that. Furthermore, there is no information on how far apart the plots are (it cannot just be determined from the map). I think this issue needs to be removed from the methods and results as it remains vague.

- My other big concern is the climate reconstruction presented in the Results and later discussed in the Discussion. First, this issue is not addressed in the Materials and Methods section as it should be. Second, the authors analyzed a 229 year long period (1782-2010) and this is not correct for several reasons. At glance, it can be seen that before 1850 the ring width index exhibits high variability and depicts the growth of only 5 radii which most likely correspond to 2-3 trees. This means this part of the chronology is no reliable and cannot be used for a reconstruction; you should guarantee at least a sample size of 10 (not before than 1860). Along with this, and perhaps more important, the authors need to assess the quality of the chronologies by looking at the variation of the RBAR and the EPS using a moving window. The running RBAR and EPS must be checked and then it can be decided the point at which the chronology becomes "stable" and can be used for a reconstruction; I would guess the earliest portion of the chronology runs below the 0.85 EPS threshold. Also, the thirty years of climate records available might not be the best dataset for pursuing a climate reconstruction

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as they just provide a limited period of time. The available records encompass a period where climate changed dramatically compared to the previous 2-3 decades and therefore using such short period would not represent the climatic conditions of the past century. Lastly, and specifically for the model presented, I consider the R value and the Adjusted R square value modest to make a climate interpretation of the reconstruction. Given the relatively short precipitation record for calibration and verification (15 years), I suggest to develop the reconstruction model using the "leave-one-out" cross-validation procedure (Michaelsen, 1987; Meko, 1997). In this approach each observation is successively withheld; a model is estimated on the remaining observations, and a prediction is made for the omitted observation. At the end of this procedure, the time series of predicted values assembled from the deleted observations is compared with the observed predictors to compute the validation statistics of model accuracy and error. The goodness of fit between observed and predicted precipitation values should be tested based on the proportion of variance explained by the regression (R2adj), the F-value of the regression, the linear trend and the normality of the regression residuals, and the autocorrelation in the residuals measured by the significance of the linear trend and the Durbin-Watson test (Draper and Smith, 1981). As additional measures of regression accuracy, authors can computed the Reduction of Error (RE) statistic over the verification period, as well as the root-mean-square error (RMSE) statistic as a measure of inherent uncertainties in the reconstruction. I strongly recommend discarding the reconstruction from the manuscript and focusing on the growth pattern provided by the tree ring chronology. Warm-cool periods for the studied site could be interpreted from it and then be compared with the regeneration and treeline dynamics.

- The entire Discussion section needs to undergo a deep revision. As it stands, it contains very general and vague statements, and some of them are wrong interpretations of the results (P5955 L13 (one site only), P5956 L27-29, P5957 L11-15, P5957 L27-28, P5959 L2-5, P5959 L27-29, P5961 L16-19, etc). This section does not explore in detail the findings of the manuscript at all. This section, in particular, is also poorly written.

- There are also an important number of references on the subject missing. I strongly encourage the authors to explore the "treeline dynamics" literature as I find the manuscript discusses/compares only a limited number of studies.

- Overall, the English could do with substantial editing.

Thus, while I find value in the findings reported here, I suggest that greater attention needs to be given to the analysis/interpretation of the specific findings related to the treeline dynamics and that it may be more appropriate for a more specialized journal. I think the work would have greatest value for researchers and ecologists in the geographic region, or for those working primarily in treeline ecosystems, rather than for a global audience.

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