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Interactive comment on "Forward modeling analysis of regional scale tree-ring patterns around the northeastern Tibetan Plateau, Northwest China" *by* X. Gou et al.

Anonymous Referee #5

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GENERAL COMMENTS

The article under review studies the performance of VS model at simulating 5 Tibetian Qilian Juniper tree-ring width chronologies, closely following the regional PCA approach of Anchukaitis et al 2006. I consider this investigation is worth to be published in BG, as it analyses several important aspects of the regional tree-ring growth patterns in the study zone; however, there are some considerable issues which must be clarified first.

1. The authors claim to use the complete VS model, however its cambial module (a very distinctive feature of VS model, see Evans et al 2006, sec. 2.4) is hardly

described in the paper and none of their parameters is reported in table 2. Related to this issue, the sentence "VS model integrates scaled point-wise minimum of two functions to determine the overall annual growth rates." (P9975,L23) makes me wonder whether the authors are using the complete VS model or some "VS-Lite-like" variation where the growth rate function is time integrated instead of used to drive a cambium model.

2. The model tuning approach used in the paper is neither systematic nor explained: "we adjusted a few parameters to guarantee good agreement of hypothetical and observed tree-ring chronologies through repeated trial manually."(P9974,L2). I am aware that the high number of parameters in VS model strongly hampers the possibilities of using more exhaustive and mathematically sound tuning strategies (see Gaucherel et al 2008 and Tolwinsky et al 2011), however I deemed necessary that the authors discuss in detail the tuning procedure and perform sensitivity studies on the most influential parameters.

3. The length of the time series used for calibration/validation is rather small (22 points). This feature together with the typical non-negligible autocorrelation of tree-ring width chronologies can considerably reduce the effective number of degrees of freedom and render the correlations insignificant. Could the authors recalculate the correlation significance taking into account this fact (this also applies to the correlations shown in fig 3). Effective degrees of freedom correction and the Ebisuzaki test (Ebisuzaki 1997) are possible options for that.

SPECIFIC COMMENTS

1. The authors mentioned accordance between VS model output and statistical analysis between chronologies and climate factors (P9977,L18-19), however the paper does not present such statistical information. In order to understand the additional value of using the process-based VS model, I suggest to add a table (or plot) showing the chronology-climate factor correlations, both for the individual chronologies as well as

for the principal components.

2. Related to specific comment 1, the authors mention documented diverging growth trends in zones nearby the study region (P9976,L26-29). Could the authors check if this phenomenon is present in the studied chronologies?

3. P9970,L11-14: "The non-stationary and non-linear response of tree growth to climate variability has important implications for calibration of tree-ring records for paleoclimate reconstructions and prediction for forest carbon sequestration." Could the authors elaborate further on the implications?

4. P9971,L6: "Although in most cases these assumptions are shown to be broadly met". This statement is not supported. Could the authors provide backing references?

5. P9971,L28: "The mechanistic model generally performs best in dry and cold regions, where variations in both temperature and rainfall influence tree growth dramatically." This statement is not supported. Could the authors provide backing references?

6. P9972,L23: As pointed out by the other referee, there is an error in the expression for the insolation response. Is this mistake present in the calculations?

7. P9973,L26: "Since physiological characteristics and water-use efficiency of conifer trees were different from one to another in dry environments." This statement is not supported and rather unclear. Could the authors rephrase and provide backing references?

8. P9974,L12: Can the authors provide information about the next principal components?

9. P9974,L19-22: "Since the temperature remains relatively stable during this phase, the arrival of abundant rainfall in June may correspond with wide ring formation of Qilian Juniper over the northeastern TP. On the contrary, the failure of early summer precipitation is expected to give rise to the development of narrow rings". This hypothesis can be tested statistically. Could the authors provide quantitative arguments?

10. P9988, Fig.5(b): Could the authors explain how this radial growth was calculated? This question is strongly linked to the general comment 1.

TECHNICAL CORRECTIONS

1. P9970,L5: Replace "principle" by "principal".

2. P9973,L15: "George et al., 2008" reference is not present in the reference section.

3. P9975,L3 and L5: "makes a contributing effect" sounds incorrect, please rephrase.

4. P9975,L5: "the partial similarity of two correlation fields reflects that early summer moisture makes a rather critical effect in determination of variations in the annual ring-width from year to year in the hypothetical chronologies as well". This sentence is rather unclear to me. Could the authors rephrase?

5. P9975,L9-10: "Here, the wide (narrow) ring is defined as the mean ring width plus (minus) one standard deviation". I find this definition too imprecise, could the authors elaborate further?

6. P9975,L23: "tree growth at these sites is moisture-limited at the onset but temporarily temperature limited at the end dates of the growing season". This sentence is rather unclear to me. Could the authors rephrase?

7. P9975,L23: "since the VS model integrates scaled point-wise minimum of two functions to determine the overall annual growth rates." I fail to see the connection of this sentence with the rest of the paragraph.

8. P9975,L7 - P9976,L5: This paragraph is excessively long and rather confusing to me. I would encourage the authors to split and rephrase it.

9. P9987, Figure 4 caption: Change "Shared" by "Shaded".

10. P9988, Fig.5(b) caption: The term "average monthly dynamics of radial increment" seems not to be appropriate for the plot shown.

FINAL REMARK

In order to guarantee the traceability of the results, which is one of the aims of BG, it would be very convenient for the reader to have access to the climate data, chronologies, as well as to the code used in the paper. In particular, was VS model coded by the authors or some already existing implementation was used?

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