Clim. Past Discuss., 7, C1176–C1182, 2011
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Climate of the Past Discussions

Interactive comment on "Reconstruction of southeast Tibetan Plateau summer cloud cover over the past two centuries using tree ring $delta^{18}$O" *by* C. Shi et al.

Anonymous Referee #1

Received and published: 4 August 2011

This is an interesting manuscript based on a well prepared data set which I believe may be suitable for publication in COTP with revisions.

My main concern is with what is actually being reconstructed using d18o from tree-ring cellulose. In my opinion to reconstruct palaeoclimate with confidence a climate proxy should have both a strong theoretical and statistical relationship with the climate parameter being reconstructed. I unfortunately have doubts over both of these relationships here.

1. Theoretically I can see no direct link between sunshine/cloud cover and d18o from tree-rings and while, as the authors point out, some previous work has show a link it is,

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I believe an indirect one. Stable carbon isotopes from tree-rings (d13c) have been used to reconstruct sunshine/cloud cover by Gagen et al (2011) and Young et al (2010), and while these look convincing the jury is still out on how valid these reconstructions are. At least, however, there is a direct theoretical link between the sunshine or PAR and d13c in certain environments. How can we be sure that the autocorrelation between circulation and the CRU cloud cover data set which persists over the ca 50 year calibration co-exists further back in time? I think the authors need to be very clear that they are not directly reconstructing cloud but something that may be strongly related to it over the calibration period. To this extent a change of title would be appropriate and also changes in the text where cloud cover is mentioned.

2. The calibration with gridded cloud cover is a very impressive one numerically, and while I don't fully understand the leave one out verification method, I am sure that the calibration is a valid one, although I am not in favour of removing the outlier(s). But is the calibration data set being used really cloud cover? I doubt it. As far as I can discern from this manuscript and Shi et al (2011) the data being used are CRU gridded cloud cover. New et al (1999 & 2000) discuss these data at some length and as far as I can tell where no appropriate data are available they have used other climate parameters to estimate cloud cover (Tmax-Tmin probably). It is telling that in Shi et al (2011) no statistically significant relationship is found between d18o and the most local cloud data (Borni) which is very close to the site but the relationship is very strong with gridded data. If New et al (2000) had used actual observed cloud data for their gridded cloud network they would almost certainly have included the Borni data set for that grid square and the regional and station cloud should then be closely related. If they did not use station data what did they use? It seems likely to me that they have used Tmax-Tmin or something similar. This however is not cloud cover. I would also like to see a figure comparing the gridded cloud and the Borni cloud data (it could be in the supplemental data). If I am correct about what the CRU gridded data set represents here, then references to cloud cover in the manuscript should be changed, rather than saying "cloud cover" say "cloud cover estimated from. . ...(whatever it is)".

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When the above two points are taken into consideration can this manuscript really justify its title or major conclusions on cloud cover changes in this area? Certainly not the way it is currently presented. The authors should discuss at some length the calibration data set used and define exactly what it is and why it only matches the local Borni cloud data to R2 = 0.25 (Chi et al, 2011). They should then reconstruct this parameter(s). They can discuss the strength of the relationship between this and cloud cover and any possible utility in using tree-ring d18o to reconstruct cloud cover. There is obviously a very strong climate relationship in these d18o data (which is good news) but the authors need to be much more explicit about what it actually is they are reconstructing, as reconstructing the wrong thing can be highly misleading.

Unless the authors can demonstrate that it actually is actually observed cloud cover they are calibrating against rather than cloud cover estimated from other climate parameters I would suggest a rewrite of the manuscript using a more appropriate climate variable, or at the very least making it very clear at all stages of the manuscript that they are reconstructing cloud cover estimated from other climate parameters rather than actual cloud cover. They should also show how strong the relationship between observed and estimated cloud is (it may be possible to do this with the Borni station data and the gridded cloud cover). This I imagine would then affect the confidence intervals surrounding their cloud cover reconstruction.

Page 1827, Line 1: add “the” before TP

Page 1827, Line 6-7: “Oxygen stable isotopes archived in tree-ring cellulose have been reported to be proxies of cloud cover variations (Hilasvuori and Berninger, 2010; Kress et al., 2010)”. Need to add some more detail here as to why a relationship between ïĄď18O and cloud cover might be expected.

Page 1827, Line 11: “28–31_ N 90–95_ E, June–August, R2 = 0.40, p < 0.01, 1956–2005” The site does not actually lie in this grid square (Page 1828, Line 3-4 “95.55_ E, 29.87_ N, ca. 2760 m”), should mention this. What is the correlation for the grid square

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it actually lies in?

Page 1827, Line 16: Two recent papers discussing age trends in d18o should be mentioned here Esper et al (2010) find age trends and Young et al (2011) find none.

Page 1828, Lines 23-25: Some more detail on the pooling strategy would be helpful here rather than having to go to Shi et al (2011) for details (This happens a number of times in this manuscript for important methodological and calibration steps I would like to see the details in this manuscript)

Page 1829, Line 11: Replace “two times” with twice

Page 1829, Line 19-24: I am not in favour of removing outlier years unless there is a very good reason, especially from such a short calibration, +/- 0.5 is not an especially large uncertainty for multiple measurements of the same cellulose when the uncertainty on one measurement is ca +/- 0.3. A mean of 6 measurements should give a reasonably accurate mean value for that year. Some discussion about what is unusual about that year climatically might be more helpful. I see no good reason to leave out either 1978 or 1991 from the calibration.

Page 1830, Line 4: I’m afraid I don’t really understand “leave one out cross validation” as applied to RE and CE statistics. Details, equations and reference(s) please.

Page 1830, Line 5-6: “statistically significant” is the wrong phrase, RE and CE are not tests of significance.

Page 1830, Line 9-11: “We have used a bootstrap method to test the quality. . ..”I do not understand exactly what the authors have done here. More details required on this method, equations and references.

Page 1830, Line 11-14: Again I do not understand exactly what the authors have done here. More details required on this method. The whole of this section (currently lines 7-14) needs to be expanded and explained more clearly and in more detail.

Page 1830, Line 17-19: I am not familiar with the SSA-MTM toolkit. It looks like a method of applying a filter, what is the filter length and methodology behind it? A simple Gaussian filter might be easier and more understandable.

Page 1830, Line 19-21: As above I am not sure what the regime shift detection software does. Please explain what the test involves, significance levels & etc.. A simple statistical test might serve better.

Section 4.1: This whole section is very confusing and needs rewriting, see individual points below.

Page 1831, Line 15: Replace "depicted" with "described" and put an e.g. before Stothers, 1984 (e.g. Stothers, 1984)

Page 1831, Line 22: Delete "massive"

Page 1831, line 25 to Page 1832 line 3: I don't like the logic behind this argument. Is d18o temperature or cloud cover or something else? If it is cloud you can't use it as a temperature proxy as well to support your argument (this is circular reasoning). You need an independent proxy to verify the temperature during this period you can't use d18o twice. Cloud and temperature often co-vary, but not always. This sentence should be removed.

Page 1832, lines 4-5: "Therefore, does the cellulose d18o anomaly of the 1810s reflect changes in large-scale moisture advection, or a regional cloud cover/moisture condition?" Aren't these much the same thing?

Page 1832, lines 7-10: "As precipitation d18o accounts for about 46% of tree-ring d18o variation (Sternberg, 2009), a large-scale shift of precipitation isotopic composition could be recorded in tree-ring d18o and misinterpreted as a local climate signal (Sternberg, 2009)." I have read this paper on a number of occasions and cannot recall him making either of these statements, are you sure you have the correct reference here?

Page 1832, lines 10-16: How far away are these ice cores and what is the dating error? Are they a useful test as you go on to say (page 1833, line 9) that d18O of precipitation from Borni and the region of these cores is not significantly correlated so how can you say that they demonstrate that the anomaly is not precipitation related? This section needs some rethinking and rewriting.

Page 1833, lines 1-6: So you conclude the 1807-1817 period is nothing to do with the volcanoes but changes in monsoon. You should therefore remove references to the Tambora eruption etc from the abstract (Page 1826, line 9-10). If you want to make a link between volcanic eruptions and changes in monsoon this should be discussed and explained at some length with reference to the literature.

Page 1833, line 6: Young et al (2010) is mis-cited here this paper has nothing to do with d18O and precipitation; it is a paper about d13C and cloud if you want to cite it should be in the introduction as an example of using tree-ring isotopes to reconstruct cloud cover.

Page 1833, 8-10: In the previous section you used data from these ice cores to say that the anomaly 1807-1817 was nothing to do with precipitation d18o changes, now you say there is no relationship between the two regions; I find this confusing and contradictory.

Page 1834, Section 5 (conclusions): Will need rewriting based on changes suggested above. Is it really a cloud cover reconstruction?

Page 1834, line 17-18: You concluded (Page 1833, 1st paragraph) that the d18o anomaly was due to changes in monsoon activity rather than volcanoes, change you conclusion to reflect this. If you wish to link changes in monsoon with volcanoes this needs to be done in detail with reference to the literature in the discussion.

Page 1842, Fig 2: Line 3 of caption, a 95% confidence interval would be more appropriate (why 85%?). It would be more normal (although not ideal) to base the uncertainty

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of 2 standard errors of the prediction.

Pages 1843 & 1844, figures 3 & 4: Figure captions need to be expanded. Figures should understandable from their captions without extensive reference to the main text

Esper, J., D. C. Frank, G. Battipalagia, U. Büntgen, C. Holert, K. S. Treydte, R. T. W. Siegwolf, and M. Saurer (2010), Low-frequency noise in d13C and d18O tree ring data: A case study of Pinus uncinata in the Spanish Pyrenees, Global Biogeochemical Cycles, 24, doi:10.1029/2010GB003772.

Gagen M., Zorita E., McCarroll D., Young G. H. F., Grudd H., Jalkanen R., Loader N. J., Robertson I. and Kirchhefer A. J. (2011) Cloud response to summer temperatures in Fennoscandia over the last thousand years. Geophysical Research Letters, doi:10.1029/2010GL046216.

New M, Hulme M, Jones P. D. (1999) Representing Twentieth-Century space-time climate variability. Part I: Development of a 1901-90 mean monthly terrestrial climatology. Journal of Climate 12: 829-856.

New M, Hulme M, Jones P. D. (2000) Representing Twentieth-Century space-time climate variability. Part II: Development of a 1901-96 monthly grids of terrestrial surface climate. Journal of Climate 13: 2217-2238.

Young G. H. F., McCarroll D., Loader N. J. and Kirchhefer A. J. (2010) A 500-year record of summer near-ground solar radiation from tree-ring stable carbon isotopes. Holocene 20, 315-324.

Young G. H. F., Demmler J. C., Gunnarson B. E., Kirchhefer A. J., Loader N. J. and McCarroll D. (2011) Age trends in tree-ring growth and isotopic archives: a case study of Pinus sylvestris L. from north-western Norway. Global Biogeochem. Cycles, GB2020, doi:10.1029/2010GB003913.

Interactive comment on Clim. Past Discuss., 7, 1825, 2011.