Clim. Past Discuss., 10, C2500–C2514, 2015
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Climate of the Past
Discussions

Interactive comment on "ENSO flavors in a tree-ring δ^{18}O record of *Tectona grandis* from Indonesia" *by* K. Schollaen et al.

K. Schollaen et al.

karina.schollaen@gfz-potsdam.de

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We would like to thank the editor and the reviewer for their constructive comments on our manuscript. We have revised the manuscript, and we believe that all issues raised by the reviewers have been addressed. Please find detailed answers to reviewers' comments below.

Anonymous Referee #2

General comments: In this manuscript, Schollaen and coauthors compare an existing (and published) d18O tree ring record from teak trees in Java with various ENSO indices. They find a significant –albeit weak – correlation with a Central Pacific ENSO time series, but not with an Eastern Pacific ENSO time series and argue that the d18O time series can thus be used for reconstruction of ENSO flavors. They justify their study based on a presumed influence of ENSO –and it's two flavors – on precipitation on Java and thus on (precipitation sensitive) tree-ring series. However, I believe this is the weak spot of this manuscript. For justification of the ENSO-precipitation relationship, the authors only show one map (Fig. 1) that shows the regression of precipitation against the 2 ENSO flavor time series. They claim that these two maps show a clear difference in the influence of the 2 ENSO flavors on Java precipitation, but frankly I don't see that difference: the regression coefficients in Java seem to be equally weak to me. Moreover, the authors do not specify which months (of precipitation and of ENSO indices) are used for the calculation of this map. This turns out to be problematic at the very end of the discussion, where they discuss that the influence of ENSO on precipitation on Java is (1) very weak and (2) strongest during the dry season, whereas tree rings primarily record wet season precipitation. They then use these arguments to explain the weak correlations they find between d18OTR and ENSO. As a reader, I felt deceived at this point (at the very end of the discussion) in the paper: the authors do a lot of armwaving in the introduction and Fig. 1 about the influence of ENSO flavors on precipitation in Java, but lack to come up with concrete evidence. It is not until the very end of the paper that they explain that actually there is not really any influence between ENSO and Java precip and one would thus not expect to find a strong ENSO- d18OTR connection. I believe this is the largest caveat of the paper: why write a paper about the connection between d18OTR and various ENSO flavors, when you would not expect there to be a connection based on what we know? I think this problem could be greatly helped if the authors included an analysis in their paper that concretely (not just in a vague map) showed the link between the various ENSO flavors and Java precipitation. Use the Java precip time series as you use the ENSO time series. As a result, it is not surprising that the correlations the authors find between d18OTR are significant but weak and definitely too weak to use for reconstruction purposes. This should be made much more explicit in the text, particularly in the conclusion where they state that 'These results indicate the significant potential for generating reconstructions of different ENSO flavors from the δ18OTR records in Indonesian teak.' In my opinion, these results show the exact opposite and the weak relationship as a caveat for reconstruction should be discussed. Finally, I find the description of the different indices used for the ENSO flavors confusing and incomplete. The authors do not describe how the La Niña indices are calculated (only how the extreme LN years are calculated, but these are not used in most analyses). Also, they are inconsistent in their use of CT and WP ENSO vs. El Niño, which is very confusing to the reader. If CT and WP El Niño indices are based on SSTas from certain regions, don't they then reflect El Niño as well as La Niña conditions? How are they an index of El Niños alone? I can see how the extreme (>1 stdev) in these time series indicate a certain flavor of El Niño, but in your analysis you use the entire time series, not just the extreme years. Similarly for the La Niña time series, if this is based on SSTas from region NIÑO4, how is this not an ENSO time series, rather than a La Niña time series alone? Also, given that your La Niña time series and your WP El Niño time series are both based on NIÑO4 and only differ in years when NIÑO3NIÑO4>0 (sic; equation 1), it does not come as a surprise that they are both correlated to δ18OTR. Also, to make a statement as you do in the conclusion (P11 L18-20) that 'the conclusions of our study call for caution when doing model-proxy comparisons using ENSO indices that are not able to distinguish between the two flavors (e.g. single standard indices such as NIÑO3.4)', it would be helpful to also include a general ENSO index such as NIÑO3.4 in your analysis in addition to the ENSO flavor indices. All in all, how the different ENSO time series are calculated is crucial to your analysis and discussion and needs to be explained in more detail (see also specific comments).

Re: We thank the reviewer for illustrating the confusion that certain parts of our previous manuscript caused. To address the reviewer's concerns, we have revised these parts (also see our detailed answers to the reviewers' comments), and offer below a general comment on the above review: We agree with the reviewer that -in its previous version- Fig. 1 failed to properly show the significant differences between WP and CP ENSO influence on Java precipitation. Indeed, the colorscale was overwhelmed by the

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central Pacific rainfall, which caused the difference over Java to not show properly, as the reviewer points out. In the revised manuscript we have updated Fig. 1, which now clearly shows the differences over Java, since the central Pacific response does not overwhelm the colorscale. We also report in the text the average regression coefficient over Java for the two flavors: -0.83 for WP and -0.03 for CT (page 5 L1-3). *Note that the regression maps are computed using monthly values. We believe that Fig. 1 (especially after its update) clearly shows that the two flavors have distinct influence on Java precipitation. Still, without an analysis like the one that we present in this study, one cannot take for granted that δ18OTR is capable of recording these distinct influences. Our quantitative analysis, and Fig. 2 and 3 show that indeed the proxy is able to show the clear differences in ENSO-induced precipitation shown in Fig. 1. We believe that this is the main contribution of our study, i.e. showcasing that a tree-ring proxy clearly captures the distinct teleconnections of Java precipitation and ENSO flavors. We also understand that the discussion of the indices caused some confusion, and appreciate the reviewer's comment on this issue. Of course, the two indices for ENSO flavors -by their construction as anomaly indices- record variants of La Niña events as well. The point of using the Ren & Jin indices is that by their construction they are able to clearly separate the two El Niño flavors. In other words, Equation 1 is a transformation of the phase space of two correlated indices (NINO3 and NINO4) into two new indices (Nct and Nwp) which are orthogonal and uncorrelated. During a WP event, NINO4 generally dominates NINO3, therefore NCT will be very small, in contrast to NWP. Consequently, Equation 1 will classify this event as WP. During a CT event, the opposite will occur, and NCT will capture it. The parameter alpha of this transformation is determined by a minimization procedure to better separate the classification of events (for details the reviewer is referred to Ren and Jin 2011). However, the separation for La Niña variants is not significant, and taken that the La Niña anomalies generally propagate westward, a single index like NINO4 is sufficient to capture them. We agree with the reviewer that Nwp and NINO4 are very closely related, and this is reflected in the correlations reported in Fig.2. Note that the third panel shows NIÑO4*(-1), in order to highlight the

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La Niña events (we have corrected the label here as well). The reason why we also present an analysis using NINO4 here and plot it in the third panel is to indicate the La Niña events that are selected by our methods and used in Figure 3 without overwhelming the first panel of Figure 2 (Nwp) with La Niña shading. Otherwise, the reviewer is correct, and the third panel is only -to an extent- complementary to the first panel; yet we feel it is useful because it presents a clear identification of La Niña events used in the subsequent Figure 3. In conclusion, indeed, the two indices record both warm and cold anomalies. We have corrected the y-axes labels in Fig.2 to avoid the confusion that was caused by our previous labels. With respect to our discussions and conclusions, we believe that this study illustrates that proxies from specific regions (like Java) are ideally located to distinguish between ENSO flavors and help elucidate some of the open questions regarding their past variability. Our conclusion about the caution required for interpreting proxies was not specifically referring to this region (due to its strong teleconnection with La Niña; the latter indeed can be captured using a single ENSO index– see our answers above and in the comments below). Even in this case of generally low correlations between the flavors and d18O, we show that the correlations with NINO3.4 (which is an index that mixes the signals of the two flavors) are even lower and non-significant statistically (Table 2). Thus, especially in multi-proxy reconstructions, where proxies from different regions are synthesized to reconstruct past ENSO variability it is important to take notice of the distinct (and often of opposite sign) correlations with the two flavors and not convolute their influence by using a single ENSO index. We have updated the conclusions to clarify this point.

Specific comments: 1) P3 L8: I am confused as to whether these are El Niño flavors or ENSO flavors. You only discuss El Niño flavors (positive ENSO phases), yet you keep calling them ENSO flavors. If they were ENSO flavors, shouldn't there be a La Niña equivalent?

Re: "ENSO flavors" typically refers to the two variants of the positive ENSO phase (El Niño); this is the manner in which we use the term in this paper. For our comment on the existence of La Niña flavors, we refer the reviewer to page 5, L27- page 6 L2. The reviewer is correct to point out that when referring to Nwp as “WP El Niño index” and not WP ENSO index one might think that it does not capture cold events. We have updated the manuscript throughout to avoid such confusions. We changed the wording WP El Niño/ CT El Niño index into WP/CT ENSO index, accordingly.

2) P3 L10-11: Fig. 2 does not show an increased frequency in WP ENSO events. How do you explain this?

Re: Given the short length of the record, whether there is indeed increased frequency of CP events in the recent decades and whether it can be attributed to GHG forcing or natural variability is still an open question. We have altered the phrasing of this sentence to better highlight this. Our analysis and our Fig. 2 only depict the two flavor indices up to 2007. It can be seen that after the big El Niño event of 1997, there are only WP events, and this state continues up to 2014, with a possibility of having another CT event only emerging in 2015 (and yet to be confirmed).

3) P3 L17: dampened instead of damped?

Re: The correct word is damped.

4) P3-L25: I don’t think Fig. 1 demonstrates this well: Fig. 1 shows that the precipitation in Java (red square) is approximately equally strongly regressed against WP El Niño as it is against CT El Niño. In general, you are going to have to make a much stronger case for differential influences of WP vs. CT ENSOs on Java’s precipitation to make the case you want to make.

Re: As noted in our general comments above, the problem with Fig. 1 was that the colorscale was overwhelmed by rainfall in the central Pacific, which is irrelevant for our study, and thus the difference in regression coefficients over Java was not clear. We have updated this figure to better illustrate the difference, and we also report the 2x3 grid-point average regression coefficient for the two indices, which are -0.03 for CT and

-0.83 for WP (order of magnitude different) (page 5 L1-3).

5) P5 L10-11: it would be good to mention what time period is covered by the TRW chronology, so that the reader has an idea of how far a potential ENSO reconstruction based on d18O could extend back in time.

Re: We added the following sentence: The according TRW chronology goes back in time until AD 1714.

6) P6 L9: it would be nice to see this demonstrated in a more convincing way: what is the concrete correlation between Java precip and ENSO in general and WP and CT ENSO in particular? Again, I find Fig. 1 not very convincing in that respect and I don't see the 'nodal line of influence' that you are talking about.

Re: As noted above, the colorscale was overwhelmed in the previous version of this figure. Please see updated version. We also report the regression coefficients for the two flavors, as requested.

7) P6 L20: - Please give a reference for the 'alternative indices' that you are talking about

Re: We added the following references: No significant differences were found when using alternative indices (e.g. Ashok et al., 2007; Takahashi et al., 2011) for calculating ENSO flavors. . ..

8) It is unclear to me what you mean by 'since the NIÑO3-NIÑO4 SST anomalies are so closely associated with rainfall anomalies in the Java region': - It is exactly these correlations that you are failing to show in this paper. Writing that they are closely correlated is not sufficient. -I also don't understand how this presumed close correlation influences the calculation of CT and WP ENSO indices?

Re: This sentence was re-phrased, and the whole section was edited for clarity.

9) P6 equation 1: - What are NCT and NWP and N3 and N4? I assume SSTas in zone

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NIÑO3 and NIÑO4, but this has not been defined.

Re: We have added the necessary clarifications.

10) What do you mean by N3N4>0? I'm assuming this should be NIÑO3 – NIÑO4 >0? - If NCT and NWP are based on SSTas of NIÑO3 and NIÑO4 regions, do they not reflect both positive (El Niño) and negative (La Niña) conditions? How come this is considered an index of El Niño alone? Please explain.

Re: As noted above, we understand the confusion caused by the phrasing, and we thank the reviewer for pointing this out. As anomaly indices, the indices are for both positive and negative anomalies, however Nct mainly captures the CT events, while Nwp the WP events. We have updated the discussion in this session to clarify (page 5 L14- 27). Please note that N3N4>0 refers to the product of the two indices and is used in order to distinguish between the cases when SST anomalies in these two regions are of the same or opposite sign, i.e. to describe whether the anomalies are basin-wide or not. In the latter case, the NINO3 and NINO4 indices are adequate to capture the different ENSO flavors and thus Nct and Nwp are equal to them without any transformation. Also, in this case, the anomalies are mostly small and the conditions are neutral (see Figure 1 in Ren and Jin 2011). Equation 1 and the parameter alpha of this transformation are determined by a minimization procedure to better separate the classification of events (also see Ren and Jin 2011 for details). We have updated the equation symbols to clarify the above points.

11) P6 L25-26: This is not the case for Table 2, which shows correlation with indices over various combinations of months.

Re: The Ren & Jin indices belong to the ENSO indices. We modified the sentence for clarification: For subsequent analyses we use the January (Jann+1) indices for the ENSO flavors, for the timespan 1900–2007.

12) P7 L3-5: please also mention what La Niña index you used to calculate the La Niña

time series you used in Figs. 2, 3, 4 and Table 2.

Re: This sentence explains the calculation of the La Niña time series: We classify a year as La Niña (LN) when NINO4 is negative by less than one standard deviation of the monthly NINO4 index (Kaplan et al. 1998).

13) P7 L7-10: this would be a much stronger statement if you showed this for Precip in Java or in a Fig. similar to your Fig. 1, rather than just making a general statement based on the literature.

Re: The attached figure (S1) shows the regression coefficients between precipitation and the two ENSO flavor indices for wet (October to May) and dry (June to September) season. It shows that most of the signal in the IMC and surrounding oceanic areas is from WP ENSO in wet and dry season (mostly dry), and CT ENSO shows no signal for either season. The figure shown in the manuscript uses monthly data to perform the regressions, therefore shows the ENSO flavor influence throughout the year. According to our literature review and our previous work on the different isotopic signatures in δ18OTR, the annually-resolved δ18OTR record that is used here encompasses the influence of both seasons, so it seems more appropriate to show in Fig. 1 the regressions based on the monthly values throughout the year (as we do). If the reviewer and the editor wish, we can include the attached figure in an appendix.

14) P7 L20-22: - How were neutral conditions defined here? - It would be nice to also show this for general El Niño conditions (not separate flavors).

Re: We added the following sentence (page 6 L22-24): We classify neutral conditions when CT or WP are not greater than one standard deviation of the respective monthly index and when LN is negative by more than one standard deviation of the monthly NINO4 index.

15) P8 L2-11: all of the reported correlation coefficients are statistically significant, but none of them are strong enough for reconstruction purposes. These are really

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rather low correlation coefficients that weaken the potential of teak d18O for ENSO reconstruction purposes. The results of this study are still of interest, but the weak correlations and what that implies for reconstruction need to at least be discussed in the discussion section.

Re: We agree with the reviewer that it is difficult to make reconstructions based solely on this proxy given the weak correlations. The low correlation is discussed in the last parts of the discussion section.

16) P8 L19: until in stead of till

Re: Corrected.

17) P8 L20 and L28: please define the time period for which this 'overall r' is calculated.

Re: We modified the sentence: The teleconnection with Jann+1 WP ENSO is strong and significantly positive from the 1950s until present, with running correlations reaching 0.6, and an r of 0.45 for AD 1950-2007 ($p<0.001$) (Fig. 2a).

18) P9 L26: what do you mean here by 'in cases with no strong signal'? what signal are you talking about?

Re: We modified the sentence: In contrast, no clear CT El Niño signal is preserved in the δ18OTR record, illustrated by the bimodality.

19) P10 L1: dampened in stead of damped?

Re: The right word is damped.

20) P10 L8-9: this discussion would be greatly helped by showing the seasonality of the influence of WP and CT ENSO on Java precipitation.

Re: We also refer the reviewer to our answer above and the attached figure, which we could include in an appendix if the reviewer and editor believe is need.

21) P10 L16-18: it wouldn't be too hard to test this: add an IOP time series to your

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analysis and see if you find a stronger correlation with d18OTR in the earlier period. Why not do this?

Re: For the benefit of the discussion, we attach here in Figure 2a (S2) the correlation between the δ18OTR record and the (DJF) DMI index (Kaplan et al. 1998). However, we do not feel it is essential information that should be included in the manuscript, and it might be distracting, since studying the IOD influence is outside the scope of this paper.

22) P10 L20: space missing between the d18OTR

Re: Corrected.

23) P10 L23-29: I find it interesting that you don't mention this until the very end of your discussion. This should be mentioned in your introduction! Also, this begs the question for which months Fig. 1 was calculated then? And also, again, this should be supported by showing correlations between the different ENSO indices and Java precipitation.

Re: This paragraph was moved to the site description (page 4 L22-L27). We also refer the reviewer to our answer above regarding the updated Fig. 1 and the attached figure, which we could include in an appendix if the reviewer and editor believe is need.

24) P11 L15-18: given what you describe in the last paragraph of the discussion, the predominantly wet season signal in d18OTR vs. the predominantly dry season teleconnection between Precip and ENSO, and the resulting low correlation coefficients between d18OTR and ENSO, I think this statement is not justified. I don't think, based on your results, that whole-ring d18OTR has potential for ENSO (flavor) reconstructions. I think you've just demonstrated that.

Re: The sentence has been modified (page 9 L16-18): These results indicate the potential for generating reconstructions of different ENSO flavors from high-resolution intra-annual δ18O records from appropriately selected regions, such as Java. We have also edited this discussion to clarify the potential for reconstructions.

25) P11 L18-20: you would have a stronger argument for this statement if you had actually compared d18OTR to e.g. NIÑO3.4 and found weaker correlations than WP ENSO. - Table 2: also here, it would be good to see results for a general El Niño index (e.g. SOI, Niño3.4) for comparison. Does divvying the El Niño up in flavors increase your correlation coefficients?

Re: As discussed in previous answers, one of the main foci of our study is to illustrate that proxies, such as δ18OTR, do record different flavors in a clear way (as seen in Figure 1 and Figure 3), and can potentially be used in a synthesis with other proxies with higher correlations for reconstruction of past ENSO flavor variability. We have edited our discussion in order to clarify this point. In addition, the conclusions of our study call for caution when doing model-proxy comparisons using ENSO indices that are not able to distinguish between the two flavors (e.g. single standard indices such as NINO3.4). We have also added NINO3.4 in Table 2, which shows insignificant correlations, due to the mixing of the signal of the two flavors when using a single ENSO index.

26) Fig. 1: What months (precipitation and ENSO indices) are these maps calculated for?

Re: The figure shown in the manuscript uses monthly data to perform the regressions, therefore shows the ENSO flavor influence throughout the year.

27) Fig. 2 caption: - 75% confidence level: that seems fairly irrelevant to me, why not show 95% as usual? L6: analysis in stead of anaylsis

Re: The Figure 2 shows the 95% confidence levels. This was a spelling error, and we thank the reviewer for pointing this out.

28) Fig. 4: - Why not also show for CT El Niño? - To me, Fig. 4B and C don't mean/show much. The fact that you don't really interpret the results (P9 L13-16) con-

firms this to me. I suggest leaving these panels out.

Re: We modified Figure 4. Instead of showing La Niña index in Figure 4c we show now the CT ENSO index.

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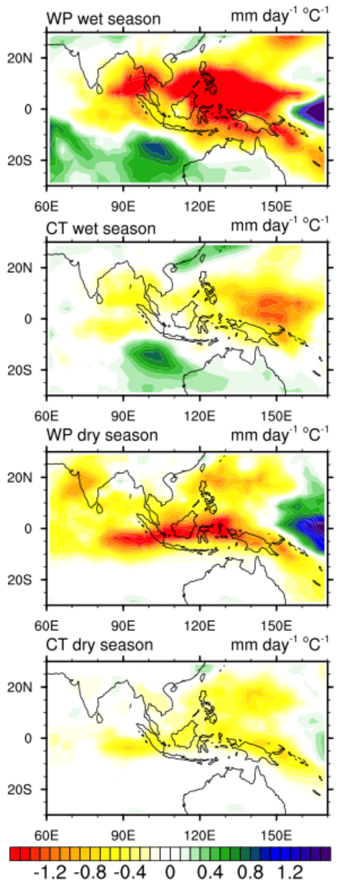
WP wet season
mm day^{-1} °C^{-1}
CT wet season
WP dry season
CT dry season
20N
0
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60E
90E
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Fig. 1. FigS1

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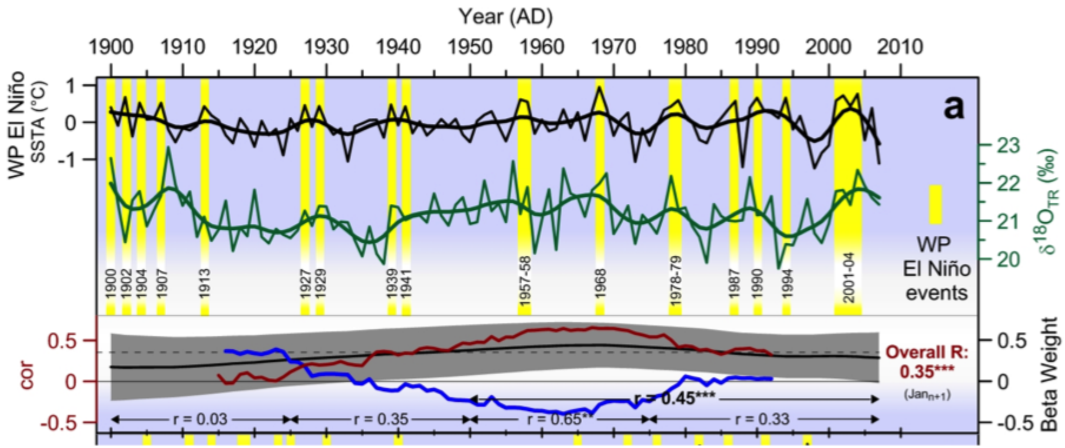

Fig. 2. FigS2

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