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Interactive comment on "A chironomid-based mean July temperature inference model from the south-east margin of the Tibetan Plateau, China" *by* Enlou Zhang et al.

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Chironomids have been used successfully in paleoclimate research in many parts world as quantitative indicators of past summer temperatures. In recent years there has been a large increase in paleoenvironmental research published in China. However, despite the advantages of chironomids in this role, until now, chironomid-based temperature inference models have not been available in China. This has been an impediment to paleoclimate research in this region and to understanding past global climate dynamics. Therefore the development of the inference model described in this paper will be of wide interest and has the potential to enhance and further invigorate paleoclimate research in China. The manuscript is generally well-written and develops

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Discussion paper

the arguments in a logical sequence. The English language is quite good but careful reading by a native English speaker would improve the flow considerably. The title reflects the contents of the paper and the abstract summaries the paper accurately. I think it would be useful to quote the CCA $\lambda 1 : \lambda 2$ ratio alongside the other performance statistics of the inference model in the abstract.

Response: we thank the reviewer for these comments. We will quote the CCA $\lambda 1 : \lambda 2$ ratio alongside the other performance statistics of the inference model in the abstract in the revised version.

The authors present results from two chironomid-based summer air temperature inference models they have developed. One model is based on 100 lakes from southwest China and the other comprises a subset of 47 lakes from Yunnan Province. The performance of these two models is assessed using standard numerical methods and the models are then used to infer summer air temperature from the top 28 cm of a core from Lake Taincai, in Yunnan. The reliability of this reconstruction is assessed using standard numerical methods and in comparison to an instrumental record from nearby Lijiang weather station. Although the performance statistics of the 47-lake model are better than the 100-lake model, the authors argue that the 100-lake model is superior and recommend the use of this larger model for future reconstructions. To me this begs the question of why the authors have chosen to present results from the 47-lake model in this paper at all. It would greatly simplify the paper and, to my mind, would not reduce the impact or weaken the arguments presented if the 47-lake model was removed from the paper altogether and the authors focussed on results from the 100-lake model alone.

Response: We agreed with the reviewer for these comments. The original idea was to compare the performance and the reconstruction results on the same site by applying a local vs. regional transfer functions. However, we agreed that the readability of the paper would be improved if we focus only on the large calibration set. We plan to modify our manuscript by reducing the sections related to the 47 calibration set.

Specific comments Line 97. It would be useful to provide references to papers which suggest that reduced local models may be more effective in reconstructions than large models encompassing long temperature gradients (e.g. Velle et al 2011 Holocene).

Response: we will include Velle et al., 2011 in our reference in the related sentence.

Line 140. It is important that the authors also quote the present day MJT (rather than MAT) of Tincai Lake since MJT is what they are reconstructing.

Response: we will cite the value of MJT of Tiancai Lake in this sentence.

Line 198. The correct reference here is Wiederholm 1983 Ent Scand Suppl. (not Wiederholm 1984).

Response: we will correct these references accordingly.

Line 205 and elsewhere. Insert 'and abundance' as it is the influence on chironomid abundance as well as distribution that is determined by these numerical methods.

Response: we will correct this accordingly.

Line 206. Table 1 includes 18 variables not 15 as stated in the text.

Response: we will correct this accordingly.

Line 233. It would be useful to plot a PCA of all 18 variables, before elimination of variables following forward selection, so we can easily assess which variables co-vary and which variables might be influencing chironomid distribution and abundance (see Juggins, 2013).

Response: we agree with this comment and we will include a PCA plot of all 18 variables.

Line 304. These taxa may have more cosmopolitan distributions than other taxa in the dataset but they nevertheless must have estimated temperature optima that are used in the model so it would be useful if they were quoted. I would suggest adding the

estimated temperature optima after the taxon names in Fig 2. The taxa can then be ranked in that figure left to right in ascending estimated temperature optima.

Response: we will modify Figure 2 by ranking the taxa along the mean July temperature gradient. We will also add the estimated temperature optima next to the taxon names.

Lines 311-320. I am concerned at the large proportion of lotic taxa in this training set which may have poorly estimated temperature optima. The authors do comment on this potential problem however.

Response: we acknowledge this issue and we will highlight this statement.

Fig 3a and Fig 3c are hard to interpret because the authors do not tell us to which species the code numbers refer. These code numbers either should be explained in the figure caption or preferably should be added after the species names in Fig 2.

Response: We will add the species code after the species names in Fig 2.

Line 337 and Line 369. Table 2a, 2b and Fig 3a, 3b do not show these correlation results

Response: Table 2a and 2b shows the correlation significance discussed in the respective sentences based on the t-values. We will correct/clarify that the significance of each of the correlation is determined based on the t-values.

Line 413, 414. What are RMSEP s1 + s2 and RMSE s1 and RMSE s2?

Response: we will delete s1+s2 and RMSE s1 and RMSE s2 here and only present the RMSEP as the standard approach.

Lines 446-459. The problems in modelling modern precipitation and temperature for the calibration set may explain the relatively poor performance of the inference model.

Response: we agree that this is potentially one of the reasons. We will add a statement about this after line 459.

Line 534. All chironomids are benthic, in fact T. gracilentus is sometimes found in temperate shallow eutrophic ponds.

Response: we will add a comment about these taxa.

Line 550 and 568. Juggins (2013) argues that the CCA $\lambda 1 : \lambda 2$ ratio, when Axis 1 is constrained against the variable of interest, should be greater than 1.0 if the variable is to be modelled reliably. In your 100-lake model the ratio is only 0.47. This reflects the fact that MJT is not the main driver of chironomid distribution and abundance in your model. This point needs to be stated as one of the caveats you list from line 569.

Response: we will state this as a caveat in line 569. However, it should be noted that the CCA $\lambda 1 : \lambda 2$ ratio in most of the training sets in the mid-latitudes and the Southern Hemisphere (e.g. Rees et al., 2008; Chang et al., 2015) is less than 1 this is because minimising other environmental gradients and only extracting the temperature gradient is difficult to achieve when we are away from the NH high latitudes.

Line 586. Why is it important that the RMSEP is around 15% of the total temperature gradient covered by the calibration set? Comparing the new Chinese inference models with other chironomid-based inference models that also have relatively low r2 does not mean that the Chinese model has an acceptable performance. You should also compare the performance of the Chinese model with the chironomid-based inference models of Heiri et al (2011) and Barley et al (2006) that you refer to earlier.

Response: we will compare the scalar length of the new Chinese transfer function with Heiri et al (2011) and Barley et al (2006) here in line 586 also, state the differences and comment on the possible causes.

Line 605. I could not immediately find the results showing the statistical correlation between the inferred records and the instrumental record. In fact this result is presented in the discussion at line 648. It should be moved to the results section.

Response: we will move the statistic P value up in the result instead of the discussion.

Line 608 and Fig. 6b. You do not discuss what might be driving the poor fit to temperature of the most recent fossil samples. This result suggests a variable other than temperature might be influencing the chironomids in the most recent period of your sequence. I think it would be informative to plot the fossil samples passively in CCA space of the calibration set to see which environmental variables the taxa were responding to.

Response: we will plot the fossil samples passively fit in CCA space and discuss what might be driving the poor fit to temperature of the most recent fossil samples.

Lines 612-615. This sentence should be deleted. It describes methods.

Response: we will delete this sentence.

Line 621. Although you adjusted the MJT from Lijiang for altitudinal lapse rate you do not present these results. Instead you plot deviation from the mean in order to compare your results. This shows a close similarity in trends but it would be of interest to see whether the chironomid-inferred temperature estimates were similar to the instrumental records adjusted for altitude. This would be another useful test of model performance.

Response: we will add the plot of the lapse rate corrected mean July temperature curve in Figure 6e along with the temperature deviation and the instrumental record.

Line 630. The 47-lake and 100-lake inferred temperature estimates are similar, especially in the most recent part of the record. However the gap between the estimates is greater at the beginning of the record than at the end. Do you have any thoughts on what might be the explanation for this difference?

Response: we will remove the plot for the 47-lake based results. A possibility for the gap between the estimates is greater at the beginning of the record is that the larger training set covered a few more sites between 10-12 °C and this may have elevated the curve slightly at relatively warmer period while the 47-lake based results are more flattened.

Line 631 and Fig 6e. It is not apparent from the plot in Fig 6e that the cool and warm periods are amplified by the 100-lake model in comparison with the 47-lake model.

Response: we will remove the plot based on the 47-lake model and focus only on the larger data set.

Line 632. While I agree that the trends in the instrumental record are well-reflected by the chironomid-inferred record it would also be useful to compare the chironomid-inferred estimates with the lapse rate adjusted instrumental data.

Response: we will also add the plot of the lapse rate corrected mean July temperature reconstructed using the chironomid transfer function in Figure 6e, together with the instrumental record and the temperature anomalies.

Lines 633-638. I could not understand the meaning of these sentences.

Response: These sentences will be simplified as we will remove the 47-lake model related discussion. We will simply apply the lapse rate to the 100-lake model reconstructed results and compare with the instrumental record.

Line 648. Results should not be presented in the discussion.

Response: we will move this sentence to the 'result' section.

Line 650. The authors' conclusion that the 100-lake model performs better than the 47-lake model makes me conclude that there is no point in presenting the results of the 47-lake model. I think reference to the smaller model should be deleted from the paper. This would make the paper shorter and easier to follow.

Response: we agreed with this comment and we will modify our manuscript by removing the separate discussion on the 47-lake model.

Conclusion. There is no need to present the performance statistic results again in the conclusion.

Response: we will remove the performance of statistics from the conclusion in Lines 659-664.

Fig 1 caption. Insert '(b)' after '(square box)'. Delete'(b)' and replace with '(c)'

Response: we will do these corrections accordingly.

Fig 2a caption. Why are only a few taxa grouped by their thermal preferences? How did you decide that some were better temperature indicators than others? What do you mean by 'optical observation'? The lakes should be listed in descending order of altitude or MJT. This needs to be stated in the caption. I don't understand how T. gracilentus and M. radialis can both be cold indicators when between them they appear to be found in complementary lakes. The estimated temperature optimum of each taxon should be presented after its name in the figure. The CCA sample score is not informative in terms of ranking the taxa when MJT is not the main driver of the taxon distribution and abundance. It would be more useful to rank them by order of temperature optimum. The code number of each taxon used in Fig 3 should also appear next the taxon name in this figure too.

Response: we will correct these accordingly.

Table 3. The caption is difficult to understand. I suggest replacing the word 'retained' with 'maintained' as I think this would better reflects the results.

Response: we will correct this accordingly.

Table 4. The results should be quoted to two-decimal places.

Response: we will correct these accordingly.

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