Earth Syst. Sci. Data Discuss.,
https://doi.org/10.5194/essd-2019-249-RC2, 2020

Open Access Earth System Science Data Discussions

Interactive comment on "A global mean sea-surface temperature dataset for the Last Interglacial (129—116 kyr) and contribution of thermal expansion to sea-level change" *by* Chris S. M. Turney et al.

Anonymous Referee #2

Received and published: 21 April 2020

General comments:

The paper by Turney and others presents a new database of sea surface temperature (SST) data covering the Last Interglacial (129-116 kyr, LIG). The database presents 189 mean annual SST records, 99 DJF records, and 92 JJA records including faunal/ floral assemblages, Mg/Ca and Sr/Ca ratios, and alkenone Uk'37 proxies. The data are corrected for ocean drift using a model, a correction that has previously not been applied to SST data from the LIG. The authors do not present SST time series through the LIG or make an attempt to align SST records to a common chronological framework;

rather, they average the SST data across the period defined by the local minimum or "plateau" in benthic δ18O, which they deem sufficiently representative of the LIG. During the LIG, mean global annual SST was +0.2 $\pm$ 0.1 °C warmer than modern. The compiled SST data are then used to quantify the thermosteric contribution to global mean sea level rise, which was +0.01 $\pm$ 0.1 m. The authors also examine the first 5 kyr during the LIG (129-124 kyr) to estimate the maximum SST and the thermosteric contribution to global mean sea level, found to be +0.9 $\pm$ 0.2 °C and +0.13 $\pm$ 0.1 m, respectively, relative to modern.

This dataset will potentially be valuable to other paleoclimate researchers and is well suited to be published in ESSD. However, I think the database would greatly benefit from more thorough presentation of the data in terms of their quality and their limitations (i.e. uncertainties therein and potential biases). For example, the data density (temporal resolution) for each record is not given or accounted for (n=? in each average SST value), the influence of outlier SST values on the LIG averages is not adequately addressed, and the spatial biases due to latitudinal/ longitudinal binning and/or lack of spatial resolution are not explored. The criteria for including records in the database need to be more rigorously and explicitly defined (were datasets rejected? how different is this compilation from the recent Hoffman 2017 compilation?).

Furthermore, the uncertainties acquired by applying the ocean drift correction are not addressed, nor are other models explored or tested to demonstrate model sensitivity.

Additionally, the authors attempted to avoid complications arising from chronological alignment of proxy records by averaging over the entire LIG period; however, there is zero discussion of how the δ18O minimum was defined in each record, how well this minimum was expressed in their 203 different sites, or to what degree errors were inherited due to local variations in benthic δ18O (even though the authors admit that such variations may temporally offset marine records by up to several millennia). In some cases, the SST records relied on proxies other than benthic δ18O to define the LIG time period, but it is nowhere explained what alternative proxies were used, how

many records for which this was the case, or to what extent it might have influenced the results. The authors also do not address to what extent aligning the δ18O minima (because that is effectively what they are doing) warps the original age scales in the 203 records, except to show a very limited number of datasets (4) in Figure 2 – and there it is evident that the differences from the original age scales are substantial in some cases. Put another way, the authors need to address to what extent local variations in benthic δ18O might cause them to falsely identify the LIG time period and ultimately bias their LIG average temperature.

Finally, the manuscript would benefit from a comparison to other published LIG SST compilations (and estimates of thermosteric sea level rise) so that the reader either has some context for whether the new LIG reconstructions are reasonable, and/or why the new data are novel or represent an improvement on preexisting work. The authors also need to clarify what portion of the ocean volume their thermosteric sea level rise applies to (only surface 700 m?). It is confusing in the text as most of the authors' statements make it sound like whole ocean thermosteric sea level rise was calculated (I am still not 100 % certain).

If these comments can be sufficiently addressed, I see no reason not to publish this useful database. Please see specific comments below.

Specific comments (main text):

Line 109-110 – I cannot grasp how reliable this method was for selecting the LIG time period from the various proxy records based on what is presented in the manuscript. Were there any objective criteria for selecting δ18O minima? The authors must describe what they mean by "other complimentary proxy values," and state for how many records in the database this applies. The authors also must state what they mean by "such a δ18O plateau is not obvious." Were there objective criteria for electing to use alternative proxies rather than δ18O? The authors seem to think spatial variations in δ18O are not an important source of error in their approach, though they admit below

that local variations can cause offsets of several millennia. Please provide more convincing arguments for this method and demonstrate to what extent these local δ18O variations are important for your analyses.

Line 159-164 – The wording in this section is a bit too sleight of hand in my opinion. I disagree that the strategy is better than aligning records to a common temporal framework, or that it somehow circumvents the problem of generating time series data. While I agree that the authors do not interpret temporal trends (though they do distinguish the first 5 kyr from the rest of the LIG), by averaging over the selected periods with minimum δ18O the authors are in essence still aligning records to a common chronology because their analysis assumes the periods were coeval. I also disagree that this strategy is better than the example of aligning North Pacific data with EDC δD (which they state could be off by 1-2 millennia) because Figure 2 shows even larger temporal offsets of up to $\sim$ 6 kyr (for example the end of the LIG in MD06-2986). The authors still need to present a convincing argument that aligning benthic δ18O is robust against the spatio-temporal variability between sediment cores, and then please state some estimate of the uncertainty and inherited SST error.

Line 188-197 – Could you show some sensitivity analysis by running the model with different circulation? Just bracketing a plausible range would be enough to demonstrate the sensitivity. Also, I am very keen to see how the core top calibrations may change due to the ocean drift. I know the full analysis is beyond the scope of this paper, but perhaps selecting only a few core top measurements and examining how impacted they are by ocean drift would be useful for demonstrating the concept?

Line 203 – How is the uncertainty determined? If most proxies have uncertainties of 1-2 °C, it seems like the uncertainty on the mean should be larger than 0.1 °C.

Line 213 – So far I did not realize that you were just calculating the thermal expansion of the upper 700 m of the ocean. I highly recommend saying this in the text prior when stating your results (e.g. in the abstract and also in the introduction when discussing

previous sea level work). Otherwise, the reader may think you mean thermosteric sea level due to whole ocean thermal expansion (deep-water and surface).

Line 296-305 – Please specify here that the authors mean thermal expansion of the top 700 m of the ocean (which I think is what they mean, though it needs to be clarified more explicitly in the text). The authors should compare their result to other estimates of the thermosteric component of LIG sea level in addition to the McKay result (Hoffman et al., 2017;Shackleton et al., 2020).

Line 303-305 – This statement is too strong without explicitly stating that the deep ocean was not considered. Readers will misinterpret it to mean whole ocean thermosteric. Or, if the deep ocean was considered (I am still unclear about whether the authors did this or not), it must be justified why SST estimates alone were used to estimate whole ocean thermosteric sea level rise and why the estimates were so low compared to other work (e.g. Shackleton 2020).

Figure 2 – Showing the alignment of only four marine cores is much too limited to give readers any sense for how much the 203 chronologies were distorted when the authors picked δ18O minima to delineate the LIG time period, over which they averaged the SST results. Figure 2 demonstrates that for none of the four cores shown did the LIG actually occur during the period 129-116 kyr (on their respective age models), and in core MD06-2986 the LIG notably occurred during a span of only about 5 kyr. Can you say with confidence (or even better, demonstrate for readers) that the cores in Figure 2 represent the full range of chronological differences in the δ18O minima between all of the records? Additionally, please improve the figure resolution so that the text and traces are not blurry.

Figure 3 – This is confusing. It looks like only the modern data were run through the drift correction. I thought the correction was applied to each LIG average.

Figure 4 – I recommend plotting a third panel showing the residual between the original SST and the drift-corrected SST.

Table 1 – It strikes me as odd that the DJF and JJA global SST values are both negative, whereas the mean global SST value is positive. What delineated a DJF and JJA record from the other 189 records? How much overlap is there between the 92 + 99 seasonal records and the 189 annual records?

Table 2 – Similar comment as above.

Specific comments (regarding the Excel file):

Sheet 1 – The spatial delineations are confusing. Why do you average > 45° and then also > 50° with only 5° difference? Please justify.

Column H - By “Jan-Dec” do you mean annual? Just say “annual” so as not to be confused with “DJF.”

Technical corrections:

Line 42 – “The timing and impacts. . . remain. . .” instead of “remains.”

Line 47 – Better references exist for “multi-millennial duration shifts in the Earth system took place in the past.” The ones used here appear to mostly be about Anthropocene/ future tipping points.

Line 51 – Can you provide a reference for 129,000-116,000 years ago, if it is elsewhere defined? Otherwise state it is the authors’ definition.

Line 56 – Global Mean Sea Level should not be capitalized.

Line 57 – There are better references for the observation of abrupt shifts in regional hydroclimate during the last interglacial than Thomas et al. 2015. Why not just cite cave record papers (Wang et al., 2008;Cheng et al., 2016), for example?

Line 58 – Buizert 2014 is not about CO2. Kohler 2017 is partly, but why not cite the original data? (Petit et al., 1999;Barnola et al., 1987) or (Bereiter et al., 2015) for the most recent compilation of CO2 ice core data.

Line 61 – Provide references for “considerable debate” about the contribution of sources to sea level rise.

Line 74 – Cite also (Hoffman et al., 2017).

Line 80 – Sea-Surface Temperature should not be capitalized.

Line 83 – Can you move the Mercer 1978 reference to somewhere in the middle of the sentence? At the end of the sentence it looks like it is a reference for the Paris Climate Agreement.

Lilne 117 – Does “maximum” refer to the average of the first 5kyr? I recommend changing the wording because “maximum” can be interpreted here that your means are upper limits.

Line 121-123 – I don’t think Figure 3 should be referenced here, as it doesn’t really relate to what is said in the sentence.

Line 125-129 – Again the use of the word “maximum” could be misunderstood to mean you only used the highest values in the datasets, especially on line 126.

References:

Barnola, J. M., Raynaud, D., Korotkevich, Y. S., and Lorius, C.: Vostok ice core provides 160,000-year record of atmospheric CO2, Nature, 329, 408-414, 10.1038/329408a0, 1987.

Bereiter, B., Eggleston, S., Schmitt, J., Nehrbass-Ahles, C., Stocker, T. F., Fischer, H., Kipfstuhl, S., and Chappellaz, J.: Revision of the EPICA Dome C CO2 record from 800 to 600kyr before present, Geophysical Research Letters, 42, 542-549, 10.1002/2014gl061957, 2015.

Cheng, H., Edwards, R. L., Sinha, A., Spotl, C., Yi, L., Chen, S. T., Kelly, M., Kathayat, G., Wang, X. F., Li, X. L., Kong, X. G., Wang, Y. J., Ning, Y. F., and Zhang, H. W.: The Asian monsoon over the past 640,000 years and ice age terminations, Nature, 534,

640-+, 10.1038/nature18591, 2016.

Hoffman, J. S., Clark, P. U., Parnell, A. C., and He, F.: Regional and global sea-surface temperatures during the last interglaciation, Science, 355, 276-279, 10.1126/science.aai8464, 2017.

Petit, J. R., Jouzel, J., Raynaud, D., Barkov, N. I., Barnola, J. M., Basile, I., Bender, M., Chappellaz, J., Davis, M., Delaygue, G., Delmotte, M., Kotlyakov, V. M., Legrand, M., Lipenkov, V. Y., Lorius, C., Pepin, L., Ritz, C., Saltzman, E., and Stievenard, M.: Climate and atmospheric history of the past 420,000 years from the Vostok ice core, Antarctica, Nature, 399, 429-436, 10.1038/20859, 1999.

Shackleton, S., Baggenstos, D., Menking, J. A., Dyonisius, M. N., Bereiter, B., Bauska, T. K., Rhodes, R. H., Brook, E. J., Petrenko, V. V., McConnell, J. R., Kellerhals, T., Haberli, M., Schmitt, J., Fischer, H., and Severinghaus, J. P.: Global ocean heat content in the Last Interglacial, Nature Geoscience, 13, 7, 10.1038/s41561-019-0498-0, 2020.

Wang, Y. J., Cheng, H., Edwards, R. L., Kong, X. G., Shao, X. H., Chen, S. T., Wu, J. Y., Jiang, X. Y., Wang, X. F., and An, Z. S.: Millennial- and orbital-scale changes in the East Asian monsoon over the past 224,000 years, Nature, 451, 1090-1093, 10.1038/nature06692, 2008.

Interactive comment on Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2019-249, 2020.