Response to Reviewers Comments (essd-2019-249)

REVIEWER #1

Turney et al. 2020 present an updated version of the Turney and Jones 2010 data compilation. As such, there is nothing too exciting about it but the inclusion of many new records, the effort to quantify ocean drift for all sites, and the resulting thermal expansion contribution to sea level are useful contributions and merit publication. There are similar data compilations (especially Hoffman et al. 2017) already to be found in the literature, with the main additional contribution of this work is the inclusion of more records and the quantification of ocean drift. Still, it is useful to see slightly different approaches yielding generally similar results. The discussion of LIG sea surface temperatures is thus justifiably short, but the thermal expansion section could be fleshed out a bit more.

As Reviewer #SC1 highlights, there are several major innovations in this study. In contrast to other studies, this study makes several contributions including a study into the potential role of ocean drift in reconstructing Last Interglacial temperatures, the development of a robust reconstruction of mean temperatures, the largest yet published network of quantified sea surface temperatures, and an analysis of published seasonal SSTs. As Reviewer #1 acknowledges, it is valuable that different approaches for reconstructing LIG temperatures show broadly consistent results, providing increased confidence in our understanding of the sensitivity of the Earth system to high temperatures.

Specific comments
Turney et al. 2020 note that there are issues with previous approaches with regards to the reference period for all reported data, and they go on to express their anomalies as relative to modern instrumental observations. This seems like a reasonable thing to do, but it is difficult to estimate the effect of this change in referencing on the final data. It would be helpful and I would recommend to try to quantify the difference that arises from different referencing approaches, i.e. modern instrumental, preindustrial, or 20th century. This would allow closer comparison of this compilation to the works of Hoffman et al. 2017 and Capron et al. 2014.

The use of different time periods to represent 'present day' has somewhat confused the literature. Whilst we appreciate the sentiment of the reviewer, there are major problems with using earlier periods (e.g. pre-industrial) to express relative temperature differences given the long known and continuing paucity of observations further back in time, particularly in remote locations e.g. Brohan et al., 2006. Such a study would need to fully quantify the uncertainties in the limited network of 'observations' prior to the satellite era, only increasing the uncertainties further, and would be a separate study in itself. As a result we are concerned this may further confuse the literature and are hesitant to undertake comparisons as suggested by the reviewer. We hope the Editor approves. Reference: Brohan, P., Kennedy, J.J., Harris, I., Tett, S.F.B., Jones, P.D., 2006. Uncertainty estimates in regional and global observed temperature changes: A new data set from 1850. Journal of Geophysical Research 111, D12106.

As noted above, section 3.5 on thermal expansion could be substantially improved in my opinion. As already mentioned by Paolo Scussolini, the recent work of Shackleton et al. 2020 should be taken into account. Further, the methodology for computing the thermosteric contribution from sea surface data could be more detailed. It is stated that the top 700m of each grid cell is assumed to have changed according to the SST change. This seems like a fairly arbitrary depth that stems from the IPCC estimate for modern ocean warming (McKay et al. 2011). With the temperature anomaly estimates being very close to zero the volume used to calculate the thermisteric component is fairly irrelevant. Still, I would appreciate more justification or some sort of sensitivity of the final sea level numbers to the assumed ocean volume. Probably it's insignificant given the temperature dependence of the expansion coefficient, but would be interesting to see the thermosteric component if e.g. half the ocean volume warmed by the stated amount.

We thank the reviewer for their suggestion. We have expanded the discussion on the thermosteric sea level rise as Reviewer #SC1 suggested. And following on from the recommendation of this review we have included the analysis of the greater ocean depths (2000 m and 3500 m). We derived the following results:

2000 m depth of warming: GMSL of 0.36 ± 0.10 m (uncorrected) and 0.39 ± 0.10 m (drift corrected).

3500 m depth of warming: GMSL of 0.67 ± 0.10 m (uncorrected) and 0.72 ± 0.10 m (drift corrected).

We thank the reviewer for the suggestion. We have now also expanded the discussion to include Shackleton et al. (2020) paper which was published after our submission.

Finally, I have some issues with Table 1. The column headings need clarification, e.g. which latitude band does <45◦S refer to? 23.5◦S to 45◦S, 0◦ to 45◦S or something else? Same for <50◦S. I'm not sure what the intention was with the order of the columns, but I would suggest going from the far north to the south and not switching back and forth between N and S. Furthermore, if Mean/uncorrected SST <45◦S is 0.2 and Mean/uncorrected SST <50◦S is 2.7, then the 45◦S to 50◦S latitude band must be very very warm (5+ degrees). Looking at Figure 4 or 5, this is not so. So something is off or I'm not understanding what is being shown in which case it should probably be described more clearly.
We must apologise. Looking at the table again, we realised it was confusing. The four columns in question refer to polewards of either 45˚ or 50˚ in both hemispheres. We have now made this explicit and reordered the columns as the reviewer has recommended.

Technical corrections
Line 19: I recommend spelling out +6-11m as it is done in the main text to avoid confusion.
Done.

Line 58: Buizert et al. did not measure LIG CO_2 concentrations, I would suggest removing said citation.
This study did report CO_2 concentrations from Taylor Glacier but we are happy to remove the citation.

Line 231: Should it say Figures 4 and 5? Line 250: delete ’enable’.
Done.

Line 292: The NEEM community paper is a pure data paper, I don’t see how that reference supports the preceding sentence.
Done.

Line 297: Buizert et al. also did not measure LIG sea level, hence that citation is inappropriate.
We apologise. This has been removed.

Line 410+: The bibliography also needs a bit of work. There are lots of links to nature.com supplementary information that should be removed and inconsistent usage of DOIs, some as full links, some as the number only.
We have edited the references to tidy them up. Sorry about this.