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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.

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This is a welcome work that tackles a key question that is presently still insufficiently resolved: understanding global and regional temperatures during a key instance of past warm climate. It is ideal that independent groups of researchers address the same problem with different approaches and producing comparable results, something that also addresses the hotly discussed issue of reproducibility in the sciences at large. This study parallels a number of previous efforts, and most closely the recent work of Hoffman et al (2017). The main differences with that study are, in subjective order of

importance: ocean drift correction is applied; SSTs are integrated across the whole LIG; a larger sample of SST proxy records; much larger sampling of seasonal SSTs.

The accounting of the oceanographic footprint of the proxy records seems to me the clearest novelty introduced in this work. This is very timely, and the importance of the drift is clear as seen in the biases in Fig. 1, although I expected this to also impact the global SST estimate. The authors provide some sensitivity test on the choice of the lifespan parameter of the virtual particles, but I find this aspect somewhat incomplete, as it focused only on parameters appropriate for foraminifera. In a sensitivity test, only lifespans longer than the 30-day value adopted in the database are tested, while shorter lifespans seem plausible for coccolith-based reconstructions, which make up much of the database; the sinking speed of 200 m/day and the 30 m depth for the lifecycle may not be adequate to simulate the situation with coccoliths and other organisms smaller than foraminifera, and with phytoplankton that is confined to the photic zone. I am not expert in these organisms, but it should have been relatively easy to apply different parameters to the main type of organisms relevant to the database (that is, if the literature suggests that these are substantially different from those used), and at least test the effect of taking unique values for the whole database when a differentiation could have been possible. Also, while this probably exceeds the scopes of this study, would it be possible to mention why a simulation of OFES with LIG boundary conditions is not contemplated, e.g., initiated with data from the coarser grid of an ocean model from a PMIP4 GCM? Maybe an idea for future work.

The integration of SSTs across the whole period has both advantages and pitfalls: on the one hand it makes results independent from the delicate set of choices that necessarily come with assessing age models and aligning them within and across basins on a coherent chronology; on the other hand it dismisses the millennial scale variability that is critical to understand notable climatic variability within the LIG. The authors recognize this, but I suggest that a more convincing explanation could be provided of the choice of working from the hypothesis (as in Turney and Jones 2010) of global synchronicity of peak SSTs: why is it superior to other solutions that make some use of the each record's explicit age models, what are the implications of the assumption for the results?

Last, it is important that the results are discussed in the light of the new results on mean LIG ocean temperature based on Antarctic noble gas, in the paper by Shackleton et al. just out in January (2020; doi: 10.1038/s41561-019-0498-0). It is encouraging that the global average anomaly from the present is indistinguishable in the two studies, although one has to consider that the Shackleton et al estimate refers to the temperature of the whole ocean and not to its surface as here. What is the relationship between these two metrics at these timescales? This should be a fine opportunity to pick up the discussion on this in Shackleton et al, and see what else can be learned from the new global compilation, especially from the fact that, unlike from Hoffman et al., mean ocean temperatures don't seem here to much exceed global (or hemispheric?) SSTs. Also, it seems very important to understand how come the thermosteric implications for global sea levels are so much lower than obtained by both Shackleton et al and Hoffman et al? The latter use a relationship of 0.42-0.64 m $^{\circ}C-1$ to infer a thermosteric contribution of 0.08-0.51 m. it is not clear how the authors obtained their thermosteric estimates.

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