A global climatology of the ocean surface during the Last Glacial Maximum mapped on a regular grid (GLOMAP) – Final response to referee comments

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Response to Referee #2 (Jessica Tierney)

We are thankful to the referee for her insightful comments which we feel will substantially improve our manuscript.

General comments

Paul et al. present a spatial reconstruction of LGM SST anomalies and sea-ice ex- tent using a new interpolation method, DIVA. Since the underlying data is mostly the MARGO data, the authors get generally the same results as MARGO (2009), with perhaps even smaller anomalies for glacial cooling. I have a couple of general comments here about the data and method used:

1) Underlying data. The sea-ice reconstruction appears to be based on both faunal assemblages and biomarker evidence like IP25, but the SST reconstruction is based only on faunal data from the MARGO collection. Why is this? I'm not sure why the authors would not use the geochemical data in the MARGO collection (?) If there is a reason, then it should be made clear. There is arguably value in a single-proxy field reconstruction, but it should be justified. Also, I think some of the faunal data may have no-analog issues. Were these dealt with in any way?

We agree that we should make clear why we base the gridded SST on the faunal data only. In fact, there are a number of reasons:

- Our interest is in a monthly climatology of the SST anomaly and sea-ice extent during the LGM, and while faunal assemblages are not without issues, they are the only sedimentary proxy that can provide a seasonal reconstruction.

- In addition, a single-proxy reconstruction has the advantage of internal consistency between the different sediment core sites, thereby reducing the noise.

- As for the MARGO reconstruction of the northern North Atlantic Ocean, all four proxies (planktonic foraminifer assemblages, dinocyst assemblages, alkenone coccolithophorid biomarkers and Mg/Ca ratios in planktonic forminifers) support the same features of sea-ice cover (de Vernal et al., 2006; see also Sarnthein et al., 2003). The IP25/PIP25 data by Xiao et al. (2015, Fig. 7a) and Méheust et al. (2018) add information for the Barents Sea and the North Pacific Ocean, respectively. However, in the Nordic Seas, there are large discrepancies between the different SST reconstructions, well above their level of uncertainties (de Vernal et al., 2006). We suspect that the apparently warm signal recorded by dinocyst assemblages, coccolithophores and alkenones may be due long-distance lateral transport (de Vernal et al., 2006; Rühlemann and Butzin, 2006), whereas foraminifera-based proxies have the advantage that they drop very quickly to the sediment (Takahashi and Be, 1984). Further possible sources of errors are the overwintering of dinoflagellates in a cyst phase as well as their broad tolerances for temperature (Dale, 2001). Using alkenones for SST reconstructions might be problematic in high latitudes because of the low sensitivity of the calibration of alkenones at low temperature (Conte et al., 2006), the possibility of redeposition of old and warm signal carrying alkenones with particulate matter originating from the glaciated continental margins and once more the influence of alkenones transported by currents from warmer areas into the polar regions (e.g., Bendle and Rosell-Melé, 2004; Filippova et al., 2016).

- Furthermore, foraminiferal assemblages are usually dominated by species adapted to the environment of the overlying water column (Morey et al., 2005). We therefore consider the temperature estimation to be more robust against the expatriation of single shells that can affect proxies measured on monospecific samples.

- Finally, proxies based on the chemistry of shells of living organisms suffer from the inherent problem that the environmental sensitivity of that organism biases the recording of the proxy (Mix, 1987; Fraile et al., 2009). The transfer function method does not have this problem since it actually uses the environmental sensitivity of foraminifera.

The MARGO project carefully dealt with the no-analog problem in a number of ways. For example, Gersonde et al. (2005) discard all samples with no analogs (dissimilarity > 0.25) and when the majority of the samples in the LGM interval has no anlogs, the estimated quality level is downgraded to 3. Kucera et al. (2005) combine three methods (Artificial Neural Networks – ANN, Revised Analog Method – RAM, Maximum Similarity Technique – SIMMAX) in a multi-technique approach that facilitates a test of the robustness of SST estimates and provides a means to identify potential no-analog conditions or faunas.

A bigger problem with the choice of data is that MARGO is over a decade old now, and surely there have been more faunal datasets published since then (Certainly, there is far more geochemical data available now). Since the authors just use the MARGO data, they get results that nearly the same as MARGO. This doesn't seem like an advance in our understanding of the LGM. If the purpose of this paper is provide new insights into the LGM, I would suggest that the authors consider updating their dataset. If the purpose of this paper is to demonstrate a method (DIVA) then the cooling and ECS results should be downplayed.

An important purpose of our manuscript is indeed to demonstrate the applicability of DIVA to sparse and irregularly spaced paleoceanographic data. Although an update of MARGO is certainly important, it is not clear whether this would change the results significantly, and it is beyond the scope of this manuscript.

We agree that we cannot expect to find truly new results and that we need to make that clearer in the revised manuscript, but we actually see a number of advantages using the well-established MARGO database: To date, it still provides the most comprehensive dataset of LGM SST anomalies at the sediment core locations including their error estimates. Without creating a spatially complete, gridded climatology the MARGO project has not been able to provide area- and uncertainty-weighted global and regional averages or boundary conditions. The main motivation of our manuscript is to provide a spatially complete, continuous gridded climatology for such purposes. By using MARGO, we can directly compare the gridded and non-gridded data and assess the effect of proper weighting in calculating global and regional averages, and we indeed find small but noticeable differences.

Another advantage of the MARGO database is the consistent use of the modern SST (WOA, 1998) as the calibration as well as the reference dataset. Most methods used for temperature reconstruction are calibrated to absolute temperatures, not temperature differences or changes. By using the calibration dataset as a reference dataset, any error is only incurred once in calculating the LGM anomaly. If using any other time slice (e.g., the Late Holocene) as a reference dataset, the error would double.

With respect to downplaying the cooling and ECS results, please see our response to the major point no. 4.

2) DIVA method. This method is new to me, but seems appropriate for the problem at hand. However some more description of the method is needed here for non-specialists. I'm also wondering, given that DIVA was designed to work with more dense modern oceanographic data, how well it does with the sparse data of the LGM? Can the authors do some validation tests to assess this? E.g., withhold 10-25% of the data, fit the field using DIVA, then validate on the withheld data? This would provide some sense of performance.

Thank you for suggesting this useful test, which we will provide in the revised manuscript to give an idea of the performance, together with some more explanation of the method for non-specialists.

3) Comparisons to other field reconstructions of the LGM. The authors discuss how their result is fairly similar to MARGO, which is not surprising since the underlying data are similar. What about other products? There are some data assimilation prod- ucts to compare with (Annan and Hargreaves, 2013) - Paul is a co-author on one of them (Kurahaski-Nakamura et al., 2017 https://doi.org/10.1002/2016PA003001) and see also Amrhein et al., 2018 (https://doi.org/10.1175/JCLI-D-17-0769.1). We have a new data assimilation product available as well (in review, but a preprint is available here: https://eartharxiv.org/me5uj/) based on an updated database of geochemical proxies.

Thank you for this valuable suggestion, too. In the revised manuscript, we will follow up on it and compare our gridded climatology with other products and discuss the advantages and disadvantages of the various methods. For example, while DIVA is a purely statistical method, the adjoint method used by Kurahashi-Nakamura et al. (2017) and Amrhein et al. (2018) makes use of the physics and paramaterizations of an ocean general circulation model (MITgcm), however, it also inherits the biases of this model (similar to the enhanced cooling in the eastern equatorial Pacific that reflects the CESM prior in Tierney et al., 2019). In contrast, Annan and Hargreaves (2013) employ an ensemble of models, but with partly inconsistent physics and parameterizations.

4) Estimates of glacial cooling and climate sensitivity. In keeping with the MARGO results, these are arguably unrealistically low (global SST change of -1.7, ECS of 1.5). The MARGO-based results of ECS (Schmittner et al. 2011) have faced a lot of criticism. Multiple studies have suggested a global SST change closer to -3C (Ballantyne et al., 2005, Lea et al., 2000) and a corresponding global air temperature change closer to 5-6C (e.g. Snyder, 2016, Nature; Schneider von Deimling et al., 2006

GRL, Holden et al., 2010 Climate Dynamics, Bereiter et al., 2018, Nature, and our new estimate in our preprint noted above). There needs to be a critical discussion in light of these other results.

According to Bindoff et al. (2013), the equilibrium climate sensitivity is likely in the range of 1.5 °C to 4.5 °C ("high confidence"), extremely unlikely less than 1°C ("high confidence"), and very unlikely greater than 6°C ("medium confidence"). The PALAEOSENS Project Members (2012) estimate based on a comprehensive set of reconstructions of past temperatures and radiative forcing yields a range of 2.2 °C to 4.8°C for the actual climate sensitivity. While neither a value of the equilibrium climate sensitivity at the low end (about 1.5 °C) nor at the high end (about 4.5 °C) can be ruled out for purely scientific reasons, we agree that a critical discussion of our result in the light of these and other results is needed. In fact, it is our area- and uncertainty-weighted gridded climatology that enables us to reassess the MARGO results.

The study by Schmittner et al. (2011) was mainly criticized because of some methodological issues (e.g., missing atmospheric feedbacks, a definition of climate sensitivity that includes feedbacks associated with vegetation, an insufficiently steep lapse rate, underprediction of land temperatures and misfit of ocean data in mid- and high latitudes – see the discussion in RealClimate, 2011). These resulted in a global cooling during the LGM in the range of 1.7 °C to 3.7 °C, which appears to be too small and possibly led to an underestimate of the climate sensitivity with a surprisingly low uncertainty range of 1.4 °C to 2.8 °C. Indeed, Annan and Hargreaves (2013), who also used MARGO as ocean data, arrived at a larger global cooling during the LGM of 3.1 °C to 4.7 °C, supposedly implying a larger climate sensitivity.

The best resolved alkenone-based SST estimates from the central Pacific show an SST change between 1.2 °C and 2 °C (Prahl et al., 1989; Lee et al., 2001; de Garidel-Thoron et al., 2007). From a number of studies using Mg/Ca as well as alkenones, Lea (2004) finds a tropical cooling at the LGM by 2.8 °C $\pm$ 0.7 °C. Leduc et al. (2017) summarize the results of the Sensitivity of the Tropics (SENSETROP) working group, which after the incorporation of high-quality records and a thorough quality control obtains a cooling of the low latitudes during the LGM of -2.3 °C $\pm$ 0.8 °C and -2.4 °C $\pm$ 0.8°C for alkenone- and Mg/Ca-based SST estimates, respectively. Tierney et al. (2019) obtain a very similar mean tropical cooling of -2.5 °C (-2.8 °C to -2.2 °C, 95 % CI) from the SST proxies on their own. These values are indeed larger than the estimates by CLIMAP, MARGO and Annan and Hargreaves (2013) by up to a degree, but not as large as the early estimate from corals by Guilderson et al. (1994) of about 5 °C, which made Crowley (2000) wonder whether it would be possible for corals to survive in the tropical ocean at such low temperatures. The assimilated mean tropical cooling by Tierney et al. (2019) is -3.9 °C (-4.2 °C to -3.7 °C, 95 % CI), which is larger than the data-only estimates that may suffer from incomplete and uneven sampling.

Regarding faunal assemblages, Ravelo et al. (1990) demonstrate that in the equatorial Atlantic they do not respond primarily to SST, but rather to thermocline and seasonality changes. Along similar lines, Telford et al. (2013) provide evidence that planktonic foraminifera assemblages can be more sensitive to subsurface temperatures than the 10-m SST that they are usually calibrated against, e.g., as in MARGO. They conclude that reconstructions of 10-m SST are likely to be biased, with the sign and magnitude of the bias varying regionally, probably causing a warm bias in the tropical North Atlantic, but foraminifera-based reconstructions for other ocean basins still remaining to be assessed

For many studies, the question is to what degree they included the Pacific Ocean in assessing the tropical cooling, or the high latitudes in assessing the global cooling; SST changes in the Arctic Ocean and the Southern Ocean are very small where permanent sea ice prevails. In our revised manuscript we will take up the above discussion and comment on why our estimate of the global SST decrease based on the MARGO faunal assemblages may be at the low end of the currently accepted range.

Also: how was ECS calculated? There must be a scaling assumption to translate to global mean surface temperature, and then there has to be estimation of the forcing as well (the denominator). Please describe this.

We used the same relationship as MARGO Project Members (2009), which is shown in Fig. 6 by Schneider von Deimling et al. (2006).

My overall take of this paper is: It's really interesting to see the application of DIVA to paleoclimate information, and this could use some more discussion and exploration (perhaps comparison to optimal interpolation). However in terms of providing new scientific insights into the LGM, the paper is limited here by use of the MARGO faunal dataset, which ultimately shapes the results. No matter what the method used, the MARGO data, particularly the assemblage data, provide an estimate of glacial cooling that is very small. This result has been challenged a lot over the years and there is a sense that perhaps no-analog problems still plague the faunal data.

I think the best solution here would be to update the underlying dataset with new studies - either new faunal data or new faunal data + geochemical data. Otherwise, the conclusions of the paper re: glacial cooling and climate sensitivity are just the same as MARGO.

Alternatively, the authors could treat this paper as a methods paper. If the goal is to just demonstrate application of DIVA, then it's OK to stick with MARGO. But in that case comparisons should be made with other field estimation methods (OI, data as- similation) and the scientific results (LGM cooling and ECS) should be downplayed and presented critically since they are ultimately tied to the underlying data.

We agree to elaborate more on the DIVA method and include a test of this method on a subset of the data. Updating the MARGO data with new data sets is unfortunately beyond the scope of this manuscript, however, we will make clear that truly new results may not be expected and put the values of the LGM cooling and ECS, now based on a properly area- and uncertainty-weighted gridded climatology, in perspective, comparing them to values obtained from other data and different methods.

Specific comments:

Abstract: Clarify that GLOMAP is based on only faunal transfer function data (except for the use of IP25 for sea ice).

Agreed

Section 2.1: Please clarify here what each reconstruction is based on (transfer functions, IP25, etc).

Agreed

Section 2.2: Why did you only use the faunal data from the MARGO collection? Also MARGO is now 11 years old. I imagine that more data have been published since then. Certainly for the geochemical proxies this is true. I think it's worth updating the data with newly published results.

See our response the major point no. 1

Section 2.3: This section could benefit from a little more explanation of how DIVA works since most readers will not be familiar with Troupin et al. (2012). In particular, it would be useful to describe how DIVA is distinct from pure interpolation (no information about spatial relationships) vs. optimal interpolation (covariance structure is set). Is DIVA essentially isotropic away from the coastlines?

Thank you for bringing up this point. We will add to the manuscript that DIVA indeed makes use of a covariance structure. In our application, it is essentially isotropic away from the coastlines, however, in principle, if current velocities are known, an advective constraint may be imposed, too.

Section 3.1: The use of past tense here is a little confusing. Use present tense for describing the results.

Agreed, we will use present tense when describing the contents of Fig. 2 and Figs. A1 to A8.

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