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Interactive comment on "Can sparse proxy data constrain the strength of the Atlantic meridional overturning circulation?" *by* T. Kurahashi-Nakamura et al.

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We appreciate the reviewer's constructive review and insightful discussion. In the following, first we reply to the general questions raised by the reviewer.

Q) additional temperature observations at depth and the addition of salinity observations would help .... it is unclear which experiments actually buttress this conclusion. ..... Unless some of the surprising results can be explained more completely, there is the remaining possibility that ...

A) In the revised manuscript we will add some analyses and explanation to give a more consistent interpretation and more insight to the experiments. The outline will be:

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- In all experiments for both Target 1 and 2, very strong vertical mixing occurs in the N-ATL at the beginning of the optimization runs, because there is an adjustment of the model SST to the much colder target SST (i.e., denser water).

- For Target 1 the point is how to recover from the "initial shock". Without deep-ocean data the lighter (warmer) deep water of the reference ocean tends to remain, causing continued mixing.

- For Target 2 the warmer deep water of the reference helps to maintain the strong mixing. However, without any salinity constraints, the surface water becomes too light (too fresh) because the colder SST there leads to less evaporation.

This analysis suggests that the sampling locations would matter irrespective of the data uncertainty with respect to predicting (at least) the direction of AMOC change. Also we will add one column to Table 1 (also see below) to present the mean cost (i.e., cost function divided by the number of model-data comparisons), which shows that the data-fitting itself is successful.

Q) an expression of AMOC strength in SST, e.g., R. Zhang, GRL, 2008

A) If we compare our study with Zhang (2008), the large difference of the time scale of phenomena would matter. Zhang (2008) dealt with decadal variabilities. On the other hand, we are trying to capture the targets that are in a quasi-steady state including the deep ocean after 1000-yr run. Therefore, information from the deep ocean will be required to constrain the model instantly. Otherwise a very long time period of assimilation will be required so that the information at the surface can penetrate the entire ocean. This is the reason why one does not find the coherent fingerprint as suggested by Zhang (2008). We will explain this citing Zhang (2008) in the revised manuscript.

Q) salinity observations will only come at great cost or as an indirect result of other observations. .... stable isotope measurements .... do have some (perhaps indirect) rate information.

A) We have already mentioned the difficulty with salinity proxy data in the text. Even if it is a demanding task, we would like to discuss salinity data because we believe that it can be an important message for the paleoceanography (proxy) community. However, we will add some comments on the potential usefulness of other proxies such as δ^{13}C for water masses, radiocarbon and ^{231}Pa/^{230}Th for circulation rates, and δ^{18}O (possibly in combination with a temperature proxy such as Mg/Ca) for density itself as additional constraints. We will cite Huybers et al. (2007) as well (see also the last Q and A).

[Hereafter we comment on the point-by-point analysis by the reviewer.]

Q) P4420, L3: No paragraph break appears necessary.

A) We will remove the paragraph break.

Q) P4420, L5-14: This paragraph sounds like it will recap all previous LGM state estimates, but it does not.

A) We rephrased this paragraph to make clear that we focus on model estimates based on inverse methods and data assimilation. We added a few more references.

Q) P4423, L3, and throughout: The maximum AMOC streamfunction should not be confused with the NADW formation rate.

A) What we are paying attention to is the maximum AMOC streamfunction. We will use that term consistently in the revised manuscript accordingly.

Q) P4425, L10: ... experiments with the colder ocean state (Targets 1 and 2) were "very inconsistent." ... nonlinearity due to the sea-ice model is the cause of the inconsistency in finding a solution?

A) We meant "incoherent", i.e. more data did not necessarily mean a better AMOC. With respect to sea-ice model, it turned out that the reconstructed sea-ice distribution (not shown) was indeed improving with more (or better) data even if the AMOC was

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not. This implies that the sea ice can be reconstructed more straightforwardly than the AMOC and can be interpreted more easily. Instead, the incoherence can be explained in a different way as shown in the first Q and A in this reply. We will change "inconsistent" to "incoherent" and explain this term and the sea-ice behaviours as outlined here in the revised manuscript.

Q) P4425, L24: Is it surprising that complete coverage of SST is insufficient to reconstruct the AMOC?

A) Not really, but we think it is still better to keep those experiments to emphasise the significance of deep ocean data even when there are many surface data available. We will rephrase the sentences concerned in a more descriptive way.

Q) P4426, L11: .... if "their sources were known." What does that mean?

A) We meant that, if we know that the targets are made by changing the viscosity alone, we are able to retrieve the modified viscosity by using it as a control variable. We will remove the sub-clause in the revised manuscript.

Q) P4426, L14: Do all experiments fit the data equally well, as determined by a chi-squared statistical test?

A) We will introduce a measure of the goodness of the fit along the following lines: The chi-square test assumes that the scaled model data misfits have an expected value of one each, so that a well balanced problem should be characterized by a cost function values that equals the number of model-data comparisons. Therefore, the mean cost (i.e., cost function divided by the number of model-data comparisons) is of order one or less (Dail, 2012). All of our experiments with MARGO data errors result in a value of less than one, which shows that the data-fitting itself was successful. For the other experiments with much smaller data errors of $\sigma = 0.1$, the values are larger than one. This implies the fit is not successful and the hypothesis that the model is consistent with the data within prior errors has to be rejected. Nevertheless, thanks to the stricter requirements, the model was generally controlled better than with the larger errors. We will add a column showing the mean $(model - data)^2/\sigma^2$ to Table 1.

Q) Figure 1 ... Sinking in the North Atlantic (either south or north of the Greenland-Iceland-Scotland Ridge) would be a better behaved target.

A) We agree with the reviewers on this point. However, because it is suggested that during the LGM the convection location was largely shifted, we devised such a diverse target for a test concerning LGM-like conditions.

Q) P4428, L20: Some elaboration is required .... temporal averaging that makes a difference?

A) As the reviewer speculated, the mixed layer depth shown in the figures is a time average for a longer period than one year and does not represent the depth of convection. If we plot the mixed layer depth for shorter time periods, it surely reaches the convection depth in the winter (and early spring). We will modify the figures.

Q) P4429, L1: What kind of prior knowledge could be used for the LGM?

A) By prior knowledge we mean a better first guess. The reviewer is right in saying that this better first guess may be hard to obtain, but some ideas to ease this problem have been already described in the conclusion section. The point is that one would be able to remove a systematic bias (e.g., globally lower temperature) before the spatial patterns of smaller scale were adjusted according to the local paleoceanographic reconstruction based, for example, on MARGO. It should be noted that the Targets 1 and 2 are more difficult than 3 and 4 because they have not only different spatial pattern (of T and S) but also a much different global mean temperature. After we remove the systematic bias of mean temperature for LGM as much as possible, the adjustment of local patterns would become easier.

For that we would need to provide different boundary conditions. One way is to couple the ocean model to an atmospheric model as already mentioned in the manuscript.

The coupling would enable for us to easily control over the global SST by changing, for example, the CO_2 content of the atmosphere, or the planetary albedo. Note that the CO_2 content and the ice-sheet distribution during LGM are comparatively well known. Another way could be to utilize the results of experiments designed for the LGM with other coupled GCMs (e.g., the PMIP project (Otto-Bliesner et al, 2007)) to modify the initial guess.

We will reinforce the conclusion section by adding some sentences about this.

Q) Table 1: Why are some AMOC estimates worsened by the addition of observations?

A) If you constrain a model only in a few places, it may adjust to these constraints at the cost of unrealistic adjustments in other places, that may then lead to an overall deterioration of the result. This phenomenon reflects the underdetermined nature of the problem. For example, the difference between E1-7 and E1-8 comes from the difference of T, S, and MLD in the ocean at depths shallower than 500 m that has almost no data points (see Fig.3 of the manuscript). This result implies the importance of data at such depths, even though the convection of the target can be roughly reconstructed from surface information.

We will add some explanations to the revised manuscript.

Q) Figure 5: .... To what extent is the small scale data variability fit?

A) One can see the situation of fitting in Fig.5 of the manuscript. When the model is very flexible (column 3), although the scale in the model results after fitting is larger than that of data having high wavenumber variability, it would be still so small due to strong local adjustments of boundary forcings that we can not say whether it is realistic or physically reasonable. When the model is less flexible (column 5), the model seems to keep a more realistic wavenumber variability.

Q) A 2007 paper with a very similar title, ... What are the specific points that distinguish this 2013 paper from the one that is 6 years old?

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A) First, we will refer to Huybers et al. (2007) in the revised manuscript.

Huybers et al. (2007) use a box model that relies on global mass conservation in each layer and geostrophy. The classic "level-of-no-motion" problem persists in this model, and the null space is large so that many data are required. In a GCM the number of physical constraints is larger and in the worst case the null space is restricted to a subset of the initial conditions and surface boundary conditions, but not the circulation itself. This brings us back to the improved first guess. If such a first guess exists, constraining the control variables immediately removes the null space. Such a method would impose a very subjective constraint on a box model in which the reference velocities are the control variables.

Another large difference between Huybers et al. (2007) and our study is that their domain excludes depths shallower than 1 km. Therefore, they did not discuss the effectiveness of surface data that are the main components of the currently available proxy data archive like MARGO. Our results show the importance of surface data to predict the convection properly, which was not obtained by Huybers et al. (2007). The adjoint sensitivity of our model to AMOC strength at $45°$N also shows the significance of temperature and salinity at the surface and at shallow depths in the high-latitude North Atlantic. This is an advantage that distinguishes our work from Huybers et al. (2007).

We will add a discussion on this topic to the manuscript.

Interactive comment on Geosci. Model Dev. Discuss., 6, 4417, 2013.