Dear Reviewer,

We highly appreciate your time and effort spent on reviewing our manuscript. We have prepared a new version of the manuscript with your comments taken into account. Below we include a point-by-point reply to each comment.

Comment:

It is stated that both stable isotopes H218O and HDO have been implemented into MITgcm. However, neither simulated HDO nor Deuterium excess values are discussed anywhere in the manuscript. Even if the number of available δD in seawater observations (e.g. GISS database, Schmidt et al., 1999) or comparable model results (e.g. Xu et al., 2012) are limited, a first-order comparison would still be valuable and of high interest for on-going SWI modelling efforts within the scientific community. Alternatively, the authors might justify in more detail why they have included HDO in the MITgcm, but don't present any of the results in their paper.

Response:

For our main goal in the near future we want to use the stable water isotope package for paleoclimatic reconstructions and compare those simulations with available $\delta^{18}O_c$ of mainly benthic foraminifera. Thus, it made more sense to us to expound our model validation on the $\delta^{18}O_w$ distribution, since this is one of the main factors influencing the oxygen isotopic composition of foraminiferal shells. We slightly rephrased the beginning of part 3.2 to indicate the latter. Nevertheless, we wanted to implement HDO as a passive tracer as well to simplify the comparison with other models during investigations in the future either by the authors or other researchers who would like to use the newly developed package.

Comment:

In the manuscript, the printed equation for the equilibrium fraction factor $\alpha_{l\text{-}v}$ for HDO is wrong. In Eq. 7 of the manuscript, $\alpha_{l\text{-}v}$ is calculated as:

$$\alpha_{l\text{-}v} = \exp(28.844/SST^2 * 10^3 – 76.248/SST – 5.2612*10^{-2})$$

The correct equation (see Majoube, 1971, Eq. 2) reads:

$$\alpha_{l\text{-}v} = \exp(28.844/SST^2 * 10^3 – 76.248/SST + 5.2612*10^{-2})$$

As no HDO results are shown in this study, I cannot say if this error is simply a (double) typo in the manuscript or if the authors have indeed used a wrong HDO equilibrium factor αl-v in their simulations. In any case, this severe error has to be checked and corrected before publication.

Response:

Unfortunately, this was a typo in the manuscript. We corrected Eq. 2.

Comment:

Title: I suggest dropping the information " (checkpoint 64w) " from the title. It is sufficient mentioning the specific MITgcm model release in the Methods section.

Response:

Done.

Comment:

P2, L4/5 (=page 2, line 4/5): I recommend adding some more key references about the application of SWI in ice cores and speleothems. Just citing the studies by Johnsen et al., 2001, and Fleitmann et al., 2003, seems odd and arbitrary.

Response:

Done.

Comment:

P2, L9: correct “form” => “from”

Response:

Done.

Comment:

P2, L16: please explain in more detail why a non-linear free-surface is essential to simulate the δ-salinity relationship properly.

Response:

The salinity in the global ocean changes due to freshwater fluxes through the air-sea interface altering the ocean volume. Many ocean as well as coupled atmosphere-ocean models still use a virtual salt flux to mimic this effect. In this case, the conservation of salt requires a constant reference salinity (usually taken to be the global annual-mean surface salinity) to estimate the virtual salt flux. This way, regions that differ significantly from the reference value might be biased and the SSS cannot evolve freely.

Furthermore, a restoring formulation is often employed by adding a relaxation term to the virtual salt flux to reproduce an SSS distribution that fits the observations (Huang, 1993; Roullet and Madec, 2000). This approach gives no insight in the dynamical explanation of the SSS pattern and is particularly problematic for past or future climate conditions, for which the SSS is unknown.

Using the real freshwater flux boundary conditions, the concentration and/or dilution effect is accurately simulated, whereby ocean volume changes and the SSS evolves freely. In the case of an ocean model with a free surface (as opposed to a rigid lid), a fully non-linear formulation of the free surface is required to conserve global ocean salinity.

The same reasoning applies to global $\delta^{18}O_w$ as well. Due to the real freshwater flux boundary conditions in conjunction with the non-linear free surface the salinity and $\delta^{18}O_w$ are dynamically more accurately simulated and thus the resulting $\delta^{18}O$-salinity relationship is expected to be much more realistic.

We added this information in condensed from to the introduction.

Comment:

P2, L26: the chosen vertical model resolution (15 levels) appears to be rather coarse. Please briefly discuss how this might affect the SWI simulation and model-data comparison.

Response:

Since the upper ~500 m are presented by only 4 layers in the ocean model, the thermocline might not be as pronounced as in the real ocean. Observational data that corresponds to depths within this transitional layer might reflect a different signal than resolved by the ocean model. More generally, a coarse vertical resolution makes a realistic representation of water mass boundaries and the comparison to observations (which involves vertical interpolation) more difficult. We added this information to section 4.2.

Comment:

P3, L16-18: what has been the exact criteria to determine if the PI simulation has reached "quasi steady-state"? Do SWI trends in deep ocean waters still exist at the end of the final 3000 simulation years?

Response:

We determined the quasi stead-state based on the salinity, temperature and Atlantic Meridional Overturning Circulation (AMOC). There were no critical trends visible, so that the global salinity, temperature and AMOC were approximately steady at 34.73 psu, 2.86° C and 18.24 Sv after 3000 model years, respectively. We added these criteria to the manuscript.

Regarding the stable water isotopes in the global ocean, there are no observable trends visible after the final 3000 years (for more details see response to comment to "correction factor for stable water isotope precipitation"), thus the global tracer concentration in the ocean was conserved (cf. P5, L21-22).

Comment:

P3, L25-27: why have different PI atmospheric forcing fields and isotopic fluxes been used for this simulation setup? Wouldn't it have been much more consistent to take all necessary forcing fields from the Tharammal et al., 2013, IsoCAM simulation?

Response:

Yes, it would have been, but unfortunately, we were not able to directly force the MITgcm with these fields because they led to a collapse of the overturning circulation. Therefore, we decided to use the forcing fields by Kurahashi-Nakamura et al., (2017), which were optimized to produce proper hydrographic conditions in our configuration of the MITgcm.

Comment:

P4, Eq 11: why is river runoff Ri subtracted in this equation? Conventionally, it is added to (P^i-E^i) to calculate the total isotopic surface flux.

Response:

Thank you for pointing that out. In Eq. 11 a bracket is missing. We changed Eq. 11 to:

$$F^i = -((E^i - P^i) \cdot (1 - A_{ice}) - R^i)$$

Comment:

P5, L17/18: please quantify the applied correction factor for SWI precipitation. How fast and how much would the global SWI concentration in the ocean change without this correction factor?

Response:

Without the correction factor, the stable water isotopes would not have reached a "quasi steady-state". Even after 3000 model years a continuous trend in the concentration of stable water isotopes would exist, resulting in a global $\delta^{18}O_w$ value of -0.17 ‰ (the ocean was initialized with 0 ‰). With the correction factor the stable water isotopes are steady with a global $\delta^{18}O_w$ value of -0.0003 ‰. For the quantification of the applied correction factor we modified Fig. A1 and added some additional information to the appendix A.

Comment:

P7, L20/Fig. 4b: if the authors rate the δ^{18}Ow measurements from the Okhotsk Sea as not representative for the North Pacific, these data points should be omitted in the analyses as well as Fig 4b.

Response:

The $\delta^{18}O_w$ measurements from the Okhotsk Sea are not representative for a zonally-averaged cross section of the North Pacific. To point this out we rephrased the respective sentence. However, since we show them in Fig. 4 and also use them in the model-data comparison in Fig. 6 (where we use inverse distance weighting to interpolate the GISS data to our model grid) we would like to keep them in Fig. 5b as well.

Comment:

P7, L24-26: please specify the sample number N for the different correlation calculations.

Response:

Done.

Comment:

P8, L7: omit "nicely"

Response:

Done.

Comment:

P8, L10: correct(?) "Simulated surface waters" => "Simulated calcite values"

Response:

Done.

Comment:

P9, L2: replace "is overestimated" by "is too depleted"

Response:

Done.

Comment:

P9, L2: please add "all other three Russian rivers"

Response:

Done.

Comment:

P9, 10-12: how well do the simulated annual discharge amounts agree with the observational data given in Cooper et al. (2008)? For a correct simulation of river runoff SWI into the ocean, both δ-values and total water amount are of importance.

Response:

It is difficult to determine the annual discharge amount in the MITgcm, because determining the grid cells that belong to the respective river is based on visually assigning them according to the location of the river mouth. This may lead to deviations compared to observational data. Nevertheless, when doing so, we get good agreement for the Yenisey, Lena, Yukon and Mackenzie rivers, while the Ob' and Kolyma rivers differ substantially. But if we compare the total modeled discharge received by the Arctic basin (> 60° N) it fits quite well the observational estimate. We added this information to section 4.2 and further included a Table (Table 2) to improve the comparison.

Comment:

P10, 27-29: I don't fully understand this argument. Please explain in some more detail the linkage between salinity restoring and SWI modelling.

Response:

As described in one of the previous responses, salinity restoring gives no insights in the dynamical explanation of the SSS pattern and is problematic for past climate conditions, where SSS is. Investigating the $\delta^{18}O$-salinity requires a free simulation of salinity and $\delta^{18}O_w$, which is possible by combining the nonlinear free surface and a real freshwater flux boundary condition. Thus, our results are an improvement compared to ocean models using virtual salt fluxes and salinity restoring, where a freely evolving salinity is not ensured.

Comment:

P11, Section 4.3: as a non-expert on planktonic foraminiferal $\delta^{18}O_c$ data, I am a bit confused by this paragraph and the given recommendations. If it is well known that core-top sediments are enriched in $\delta^{18}O_c$ due to gametogenic calcification, why have these data been compared to the simulated planktonic $\delta^{18}O_c$ values at all?

And is the better agreement of sediment δ18Oc data with modelled $\delta^{18}O_c$ calculated with Shackleton's equation just by chance, then? Which procedure/equation do the authors suggest for future SWI modelling studies, if modelled $\delta^{18}O$ values shall be compared to the manifold of available planktonic $\delta^{18}O_c$ values from marine sediments?

Response:

We agree and changed our comparison of modeled $\delta^{18}O_c$ with measurements by using only plankton-tow data. Since $\delta^{18}O_c$ values measured on planktonic foraminifera from sediment cores might not reflect equilibrium with the surface ocean water due to mechanisms such as vital effects, vertical migration and modifications after death, it seemed to be more appropriate testing modeled $\delta^{18}O_c$ against living foraminifera from plankton-tows. This way any deviations due to seasonality, depth habitat, gametogenic calcification or modifications after death should be negligible.

The better agreement using the Shackleton equation was indeed due to the circumstance that it is based on the benthic foraminifera *Uvigerina spp*, which is relatively enriched in ^{18}O and produces a similar offset from equilibrium calcite as the gametogenic calcification.

The use of ecosystem models including foraminifera might ultimately provide a better understanding of the factors that determine the recording of oxygen isotopes in foraminiferal shells (Fraile et al. (2008), Lombard et al. (2009), Kretschmer et al. (2016)).

Comment:

P12, L15: "using real freshwater and isotopic flux boundary conditions" => omit "real"

Response:

We would like to keep it that way, because the term "real freshwater flux boundary conditions" is based on Huang, 1993.

Comment:

P12, L25: omit "remarkably"

Response:

Done.

Comment:

P19, Table 1: please specify in more detail how the different water masses (AAIW, NADW, AABW) have been defined and how the related $\delta^{18}O$ values have been calculated.

Response:

Done.

Comment:

P21, Fig. 2: add the unit "[psu]" to the colour bar title.

Response:

Done.

Comment:

P23, Fig. 4: please specify in more detail how the zonally averaged cross sections of $\delta^{18}O$ have been calculated.

Response:

For the calculation of the zonally-averaged cross sections we used a mask for the respective basin provided by the WOA09 and divided it into equally spaced latitudinal bands. Along those latitudinal bands a weighted zonal mean was calculated. We added those information to the figure caption of the figure (now Fig. 5).

Comment:

P23, Fig. 4: why do the plots stop at 50°S and 50°N, respectively? GISS data from higher latitudes exist and it would be valuable to compare model results and observational data in these regions of the Atlantic and Pacific, too.

Response:

Our plots in the figure (now Fig. 5) are based on the basin masks provided by the WOA09. According to the WOA09 the Atlantic and Pacific basin extend from 50° S to 60° N, thus we went along with that.

Comment:

P25, Fig. 6: do the plots show salinity and $\delta^{18}O$ values at a depth of 50m or at a depth range 0-50m? Please clarify this in the figure caption.

Response:

The figure (now Fig. 7) shows all the GISS data from a depth range of 0-50 m, either located in the tropics or mid-latitudes, while the data simulated by the MITgcm correspond to the first level of the vertical grid, thus the upper 50 m. We clarified this in the figure caption.