

Manuscript:

Stable water isotopes in the MITgcm (checkpoint 64w)

Authors:

Rike Völpel, Andre Paul, Annegret Krandick, Stefan Mulitza and Michael Schulz

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Review

Völpel et al. present in their manuscript first results of a newly implemented stable water isotope (SWI) diagnostics within the ocean GCM MITgcm. Their evaluation of this model enhancement focuses on modelling results of  $\text{H}_2^{18}\text{O}$  in a simulation under pre-industrial climate conditions. This evaluation contains both a model-data comparison using measurements of  $\delta^{18}\text{O}$  in seawater and in planktic foraminifera as well as a brief analysis of the simulated  $\delta^{18}\text{O}$ -salinity relationship in different water bodies.

The manuscript is well outlaid and written in a clear and concise manner. The implementation of SWI into the MITgcm does not contain any new methodological approaches or intellectual merit. It follows more or less directly previous isotope implementations done in other ocean GCM. However, given the few number of existing ocean GCM with SWI diagnostics so far, I still rate this work as highly valuable and well suited for publication in GMD.

Two important issues should be addressed by the authors before publication can be warranted:

(i) It is stated that both stable isotopes  $\text{H}_2^{18}\text{O}$  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  $\delta\text{D}$  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.

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

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

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

$$\alpha_{1-v} = \exp (24.844/SST^2 * 10^3 - 76.248/SST + 5.2612*10^{-2})$$

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  $\alpha_{1-v}$  in their simulations. In any case, this severe error has to be checked and corrected before publication.

Further comments and corrections:

- Title: I suggest dropping the information “(checkpoint 64w)” from the title. It is sufficient mentioning the specific MITgcm model release in the Methods section.
- 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.
- P2, L9: correct “form” => “from”
- P2, L16: please explain in more detail why a non-linear free-surface is essential to simulate the  $\delta$ -salinity relationship properly.
- 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.
- 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?
- 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?
- P4, Eq 11: why is river runoff  $R^i$  subtracted in this equation? Conventionally, it is added to  $(P^i-E^i)$  to calculate the total isotopic surface flux.
- 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?
- P7, L20/Fig. 4b: if the authors rate the  $\delta^{18}O_w$  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.
- P7, L24-26: please specify the sample number N for the different correlation calculations.
- P8, L7: omit “nicely”
- P8, L10: correct(?) “Simulated surface waters” => “Simulated calcite values”

- P9, L2: replace “is overestimated” by “is too depleted”
- P9, L2: please add “all other three Russian rivers”
- 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  $\delta$ -values and total water amount are of importance.
- P10, 27-29: I don’t fully understand this argument. Please explain in some more detail the linkage between salinity restoring and SWI modelling.
- P11, Section 4.3: as a non-expert on planktonic foraminiferal  $\delta^{18}\text{O}_c$  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  $\delta^{18}\text{O}_c$  due to gametogenic calcification, why have these data been compared to the simulated planktonic  $\delta^{18}\text{O}_c$  values at all? And is the better agreement of sediment  $\delta^{18}\text{O}_c$  data with modelled  $\delta^{18}\text{O}_c$  calculated with Shackleton’s equation just by chance, then? Which procedure/equation do the authors suggest for future SWI modelling studies, if modelled  $\delta^{18}\text{O}$  values shall be compared to the manifold of available planktonic  $\delta^{18}\text{O}_c$  values from marine sediments?
- P12, L15: “using real freshwater and isotopic flux boundary conditions” => omit “real”
- P12, L25: omit “remarkably”
- P19, Table 1: please specify in more detail how the different water masses (AAIW, NADW, AABW) have been defined and how the related  $\delta^{18}\text{O}$  values have been calculated.
- P21, Fig. 2: add the unit “[psu]” to the colour bar title.
- P23, Fig. 4: please specify in more detail how the zonally averaged cross sections of  $\delta^{18}\text{O}$  have been calculated.
- P23, Fig. 4: why do the plots stop at  $50^\circ\text{S}$  and  $50^\circ\text{N}$ , 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.
- P25, Fig. 6: do the plots show salinity and  $\delta^{18}\text{O}$  values at a depth of 50m or at a depth range 0-50m? Please clarify this in the figure caption.