Answer to RC1:

Dear Marlos Goes,
thank you for your helpful comments and constructive feedback. Please find our answers in line and in green.

This is my review of the paper "Evaluating Transport Observations of the Atlantic Meridional Overturning Circulation at 11°S Using an Ocean Model" submitted to Ocean Science. This paper uses a high resolution ocean model to test the observational techniques used to derive the AMOC at 11S from the TRACOS ocean observation array. The paper tests various methodologies by simulating real bottom pressure (PIES) and hydrographic (Tall mooring) data in the model, and divides the results into two categories: low (>5 years) and high (<2 years) frequency variability. Some of the tests performed here are the vertical interpolation, vertical resolution of the profiles, bottom pressure drifts and gaps, and reference depths for velocity/transport. In the end, all data from tall moorings and bottom pressure are used to simulate an optimal setting of the 11S array, and recommendations on how to improve the AMOC observations are given.

The paper is well written and design. It is a bit too long to be able to read it in a at once, and the tables are sometimes difficult to understand. I would suggest to leave part of the analysis for an appendix, and just give a summary in the text, as for example the part of the vertical interpolation, such as consolidating sections 3.13, 3.2, and 3.2.1 into one or leaving part of it in the appendix. I leave this to the discretion of the authors.

We agree that the paper is quite long and benefits from being more condensed. Thank you for your suggestions how to shorten the paper. We will move some paragraphs to the appendix and slightly re-structure the remaining sections. Please find a list of the respective changes we will make at the end of the document.

In general the paper is very informative, and builds on previous papers on the inclusion hydrographic and bottom pressure data analysis and the simulations of an enhanced method using all information available. The paper should be published after minor suggestions listed point-by-point below:

Point-by-point Comments:

L. 43 Strictly speaking, this is not an OSSE experiment since it does not test data assimilated in the model nature run. I would put a disclaimer in the abstract, introduction or methodology this work, just for accuracy. Maybe just call it a data impact study.

From our understanding, it is not required for an OSSE (observing system simulation experiment) to assimilate data. For example, the glossary of meteorology defines an OSSE as an experiment in which model results are just subsampled (https://glossary.ametsoc.org/wiki/observing-systems-simulation-experiment/). For a method test of the OSNAP array, Li et al. (2017) define that in “observing system simulation experiments (OSSEs), […] the deployed observing system is recreated within the context of an ocean circulation model.” The OceanOSSE tool developed by the EPOC project to test

AMOC arrays also only subsamples instruments and does not assimilate their data (Blaker, A. and Tooth, O. (2026). Specification of AMOC metrics for OSSEs. EPOC. https://zenodo.org/records/23499945). Hence, we think that the current sentence in the introduction “In an OSSE, observing systems are mimicked in a model and compared with the full-field solution (hereinafter referred to as model truth).” should be sufficient to clarify how we use the term OSSE.

Abstract: L 10-13 The items (1) and (2) are not really "potential improvements of current approaches" but rather "misrepresentations of the current approach"

The limited usability of self-calibrating BPRs and IESs are not really a misrepresentation of the current approach as they are not used for the current AMOC estimates at 11°S. Instead, we just tested their potential and limitations in this study to assess whether the AMOC at 11°S could be calculated more reliably when including alternative data sets compared to the current approach. We rewrote the sentence to “Regarding alternative approaches, we find:…” instead of “Regarding potential improvements of the current approaches, we find:…”. Furthermore, we will slightly re-structure the results section by dividing it into current approaches and alternative approaches and hope that this makes it more obvious.

L. 133: "global mean SSH, which is derived from the local divergence of the velocity field and the balance of all local surface freshwater fluxes". Maybe you want to say "the component of the global SSH that is derived from the local ...."

The simulated SSH is directly calculated from the divergence of the velocity field and the balance of surface fluxes. Hence, this describes the full SSH in the model and not just a component of it. We revised the sentence and it now reads as “Consequently, the modelled SSH, which is locally derived from the divergence of the velocity field and the balance of all surface freshwater fluxes, is prevented from drifting in its global mean.”

L.140-154 This paragraph is long to not have any figure associated to it. Is there a way to include some model validation in the supplementary?

We will shorten the paragraph slightly. Still, we think it is important to mention both the ability and weaknesses of the used model simulation. As we do not carry out an own model validation, but only refer to previous studies, there is no figure associated with this paragraph.

L.189 Since there are many authors, it would be appropriate to write the text in the third person.

Thank you for spotting this. We corrected the pronoun.

L. 236 When pressure "profiles" ... - to be more specific.

We specified “When the pressure at both sides of the basin is known, T_g can be directly calculated from Eq. (3).” to “If the pressure at a given depth is known at both sidewalls of the basin, T_g can be directly calculated for that depth level from Eq. (3)”. We used pressure

at given depths instead of pressure profiles as we only compute profiles after we derived zonal pressure gradients. Profiles are, for this method, only calculated for T_g and not for pressure.

L.249-250 - By choosing SSH right at the coasts, wouldn't it be detrimental to the signal-to-noise ratio due to coastal waves and costal wind variability? Maybe a disclaimer should be added about this negative side as well, at least in the observational sense.

Thank you for pointing this out. The choice of the SSH location is ultimately a trade-off between having distortion of the signal right at the coast and neglecting parts of the transport close to the coast. We chose SSH right at the coast following the procedure in Herrford et al. (2021) who also show the difference in terms of annual harmonics between SSH directly at the coast and further away from it (their section 2.2 and Figure 2). We will add the uncertainty by choosing SSH right at the coast to the limitations in the discussion. In this section, however, we will refrain from mentioning it to not mix the method test (in the model environment) with observational constraints.

Equation 7: Tg was previously defined as the zonal integration of velocity, calculated as the pressure difference, but here it is defines as the depth integration (Compare Eq. 7 with Eq 3). Should this be AMOC_g instead. This definition should be consistent.

We agree that the definition should be consistent, however, we are convinced that this is currently the case. Equation 7 represents the vertical integration of the thermal wind equation and thus still refers to the meridional transport at a specific depth (relative to a level of no motion) and is not $AMOC_g$. Equation 7 is just another formulation from Equation 3, in which the pressure is replaced by Equation 5, that means the pressure is expressed by density according to the hydrostatic balance ($\frac{dp}{dz} = -\rho \cdot g$). Hence, Equation 7 only provides $\int v_g dx$ and another integration from the surface to a level of no motion / maximum of the overturning streamfunction is needed to obtain $AMOC_g$.

Section 2.4.3 - I am not sure I understood from the text how Tau_ies is derived in the model. Since this is calculated from the sound speed, and this is not an output variable of the model, how is the time travel calculated?

Tau_ies is derived from temperature and salinity profiles which are an output of the model. In a first step, sound velocity profiles are calculated from the temperature and salinity profiles. This is done using the seawater library based on the EOS80 equations of state. In a second step, the sound velocity c is integrated to obtain the travel time, $tau_{ies} = 2 \cdot \int_z^0 \frac{1}{c(z')} dz'$. We rewrote the respective sentence for clarification. It now reads as follows:
"As tau_ies is not a model output variable, we calculate profiles of the sound velocity c from temperature and salinity following the EOS80 equations of state and then derive tau_ies from $tau_{ies} = 2 \cdot \int_z^0 \frac{1}{c(z')} dz'$."

L.359 The comparison between the rmse and standard deviation of the AMOC is using the same filtering, or it compares the rmse of the filtered data with the standard deviation of the full spectrum of the AMOC_g?

The standard deviation of the model truth, to which the rmse is compared, is calculated also for the high-pass filtered and low-pass filtered time series. So, the comparison of the rmse and standard deviation is based on the same filtering. We added this to the description: “For a sufficient performance, we expect the rmse to be smaller than the standard deviation of the model-true **(equally filtered)** AMOC_g'.”

L.403-405 Before the trend removal every few years, was any trend added to the BPR data similar to what was described in section 3.1.2? If not, please explain there the difference in AMOC_g would come from?

For the simulation of the de-drifting of the standard BPRs, no drifts were added beforehand. As we would add and remove drifts according to the same drift model ($a_1 \cdot (1 - e^{-a_2 \cdot x}) + a_3 \cdot x + a_4$), we do not expect any advantage from doing so. Put a bit differently, we do not want to assess how good the sensor drift is captured with the de-drifting procedure but instead we want to assess how much of the oceanic signal (and thus AMOC variability) is removed in the process. When we fit an exponential plus a linear function to a part of the pressure time series, there will be non-zero function coefficients corresponding to longer term pressure variability. Therefore, the difference in AMOCg with and without ‘de-drifting’ of the pressure time series originates from oceanic variability that is wrongly removed in the first case.

In section 3.1.2, we added the expected drift rates of self-calibrating BPRs since our aim is assessing whether with such a drift rate an AMOC trend could be recovered. So essentially, section 3.1.1 is about the side effects of de-drifting while section 3.1.2 is about estimating for which drift rates no de-drifting was necessary.

L.416 Can you state in parenthesis the value of the correlation and rms.

Yes, we added the correlation and rmse. It now reads “the correlation between the reconstructed and model-true short-term AMOC_g' variability decreases by 0.15 **to 0.76**, while the rmse increases by 70 % **to 1.80 Sv**”.

L. 618 I do not understand why this statement was made that IES are not suitable for the AMOC_g if in the next sentence it will be used for it. It seems contradictory.

We are unsure what gave the impression that we were using IES measurements, that would indeed be contradictory. We assume that the use of the instrument name ‘IES’ is misleading as we are using the pressure measurements from PIES but not the acoustic travel time measurements. To clarify, we modified the respective sentence to: “**The available acoustic round-trip travel time measurements** do not seem suitable to be incorporated in the AMOCg reconstruction at 11°S.”

L. 698: "refined to enhanced" vertical resolution?

We rewrote to “refined to the enhanced **setup**”.

L. 724-725 These papers are for the North Atlantic. Is there any example for the South Atlantic. As a suggestion, a recent paper (Pita et al., 2024; https://doi.org/10.1029/2023JC020010) shows it in their Figure 11.

As we understand it, Pita et al. (2024) Figure 11 shows that "The AMOC geostrophic transport has a higher correlation with the western (r = 0.69) than with the eastern boundary (r = 0.32)." This conclusion was drawn from monthly data and we cannot infer what this means for the variability contribution on different time scales – which is essentially what we are discussing. However, we mention in the previous line the study from Rühs et al. (2015) which includes results of the South Atlantic.

List of changes to condense the manuscript:

To condense the manuscript, we re-structured it slightly and moved some paragraphs to the appendix. Specifically:

Moved to appendix:

- Parts of the model validation from section 2.1
- Section 3.1.3 on the sensitivity of AMOCg to the vertical structure for the bottom pressure method including Figure 4; accordingly, legend Table 1 is shortened
- Section 3.2.1 about the reconstruction of density profiles from moored temperature and salinity including Figure 5; accordingly, legend Table 2 is shortened
- Figure 6

Further adaptations:

- Figure 3a-c removed
- Figure 2a AMOC added
- Figure 7 replotted for AMOC instead of AMOCg
- Statistic tables (Tables 3,5, and 6) replaced by bar plots for easier understandability

The paper is **re-structured** to more clearly separate between evaluation of currently used methods and alternative methods and to also account for the resulting error in terms of the full AMOC and not just AMOCg. The adapted structure is:

1 Introduction
2 Data and Methods
    2.1 Ocean model configuration VIKING20X
    2.2 TRACOS array setup
    2.3 Calculating AMOC volume transport
    2.4 Deriving geostrophic AMOC transport from the TRACOS array
        2.4.1 Method based on zonal bottom pressure gradients
        2.4.2 Method with interior based on moored temperature and salinity
        2.4.3 Method based on acoustic round-trip travel times
        2.4.4 Combining bottom pressure and moored temperature and salinity data
    2.5 Evaluating the performance of the array reconstruction
3 Results for model-true AMOC components
4 Results for subsampling observational methods to derive geostrophic AMOC transport
    4.1 Methods used so far
        4.1.1 Based on zonal bottom pressure gradients
        4.1.2 Based on boundary currents
    4.2 Alternative methods
        4.2.1 Based on moored temperature and salinity for the interior
        4.2.2 Based on acoustic round-trip travel times
        4.2.3 Potential of including self-calibrating BPRs
        4.2.4 Potential of combining bottom pressure and moored temperature and salinity data
5 Results for deriving AMOC transport and improving methods used so far
6 Summary and Discussion

Appendix A: Additional information on realisation in model
    Appendix A1: Realism of the used model simulation
    Appendix A2: Differences between reality and model in instrument depth of the TRACOS array
Appendix B: Overview of all performed method tests
Appendix C: Full version of statistics for the simulated array-reconstruction of AMOCg compared to the model-true AMOCg
Appendix D: Additional information on reconstructing vertical structures
    Appendix D1: Sensitivity of Tg to the assumed vertical structure for the method based on zonal bottom pressure gradients
    Appendix D2: Vertical structure for reconstructing density profiles for the WB4 and EB1 moorings
Appendix E: Additional information on the potential of acoustic round-trip travel times
Appendix F: Additional information on reconstructing bottom pressure by other variables