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## Comment on cp-2022-35

Anonymous Referee #1

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Referee comment on "Unraveling the mechanisms and implications of a stronger mid-Pliocene Atlantic Meridional Overturning Circulation (AMOC) in PlioMIP2" by Julia E. Weiffenbach et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2022-35-RC1>, 2022

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This is a review of the submission "Unraveling the mechanisms and implications of a stronger mid-Pliocene AMOC in PlioMIP2" of Weiffenbach et al. to Climate of the Past. The paper is a welcome contribution that utilize CMIP/PMIP output beyond reporting, to make progress in narrowing the yet substantial gap in our understanding of the drivers and impacts of the AMOC in the present and the past. The paper is in general well-written in terms of language and readability. The papers has some interesting conclusions. It shows how the closing of the Bering Strait results in lower freshwater input to the Atlantic from the Arctic and how that results in a region of higher salinity in the subpolar North Atlantic, which could very well be the driver of the stronger AMOC changes. And it shows that the stronger overturning leads to higher heat transport, but that a decreased zonal temperature gradient means the gyre heat transport is decreased. The resulting impact on North Atlantic SST is complex and model dependent. Despite these and other insights, I have some concerns about the validity of some of these analysis and conclusions that may require a substantial revision.

### General comments:

#### 1) Splitting of Overturning and Gyre transports:

The overturning heat transport (OHT) and wind-driven gyre heat transport (GHT) calculations are approximated by the heat transport from the zonal mean flow (ZHT) and a-zonal flow (AZHT), respectively. The difference between the GHT and the AZHT and the OHT and ZHT are not discussed even though it could be substantial. In fact, they are presented as the same thing (e.g., L329 and elsewhere). At 20-40N the ocean interior (outside the Western Boundary, WB) is very close to Sverdrup balance. The anomalous meridional transport due to the overturning occurs in the WB. This is well known from theory, models, and also clear in Figure S6. The ZHT uses the zonal mean temperature and not the WB temperate and thus likely under-estimates OHT. Similarly, in the deep

ocean we know there is no wind-driven gyre at these latitudes, so there should be no GHT. Yet, the mean WB flow is subtracted everywhere in the deep ocean to give a resultant AZHT, that can obviously not be wind-driven and should not be presented as GHT, at least not without an analysis and discussion of the error made in this. Similarly, the deep eastern interior temperature is probably not relevant to OHT in reality, but for ZHT it dominates since most of the basin is away from WB. The same would apply for freshwater transports. Given that the Miocene-PI temperature changes do not have the same spatial patterns in the models, with some warming more in the east or west than others, this error made in the mPWP-PI calculations by equating ZHT to OHT and AZHT to GHT would be different in the different models.

A more relevant way to calculate the GHT and OHT at these sub-tropical gyre latitudes may be as follows: Determine an estimate of the level-of-no-motion (where the interior velocity nears zero). Next, at each latitude, determine a longitude for the western boundary of the wind-driven gyre by integrating meridional transport from the east to the west, until the longitude where the total meridional transport from east to west is zero. Now calculate the actual HT from  $v$  and  $T$  in this region above the level-of-no-motion and east of the western boundary of the gyre and this should more accurately represent the heat transport from the wind-driven circulation than the AZHT estimate. The OHT is then the actual HT in the rest of the cross-section. The same goes for the freshwater transport. This calculation is not as easily transferrable to other latitudes. However, most of the conclusions from the overturning and gyre heat and freshwater transports are drawn from 20-40N so this should work well. It is still an approximation of course, but should be much closer to the actual OHT and GHT.

The major conclusion in the paper that the GHT is weaker during the Pliocene while the OHT is stronger, should still hold, since it can be deduced directly from the fact that the wind-driven volume transports are constant (Fig S6) and the zonal temperature gradient is decreased.

## 2) Cross-model correlations between quantities..

The figures Fig 4, Fig 8, Fig 10, Fig 11, Fig 12 are scatter plots involving all models to show the relationship of one process against another. The correlation coefficients and confidence levels are only given for Figure 8, 10, 12. It would be good to have in all these figures, rather than just a few. It is actually unlikely that one would find great correlations in these kinds of plots, even when processes are well correlated within one model. The constant of proportionality is usually highly model dependent. However, when two processes A and B both increase in (almost) all models at the Pliocene or both decreases, it is reasonable to assume that they are positively correlated somehow. A weak correlation coefficient between A and B across models, does not necessarily negate this. Of course, when the correlation is very strong, it does support the robustness of such a relationship within and among models. In this paper, the same standard for when processes are assumed to be related or to drive each other should be applied throughout for all these figures. At the moment this is not always the case. For instance, Fig 4c and 4i is said to show that the AMOC is driven by meridional and in particular salinity gradients. But the correlation coefficients are not given. Similarly, when all models show enhanced Arctic freshwater input to the Atlantic and a stronger AMOC, the freshwater decrease is taken to

be the proven driver of the AMOC, even without correlation analysis. But in Figure 8 the near-zero correlation between PmE and the AMOC is taken to mean that PmE does not drive the AMOC directly. However, 14 of 15 models show more evaporation and a stronger AMOC.

### 3) Structure

The results at the moment jump from 3.1 AMOC SST impact to 3.2 AMOC salinity drivers and then back to 3.3 AMOC heat transport impact. It seems more logical to keep the impact on SST and heat transport together. Perhaps start with the exploration of the salinity changes driving the AMOC changes, then continue to impacts.

#### **Specific comments:**

L68: Missing "is".

L70: You could add to the final sentence "and a potential driving force during the Pliocene".

L94: Please provide some information about the spin-up times. I'm sure this is in earlier papers, but since this paper deals with the deep ocean, it seems appropriate to include it here.

L234: Is this really a Gulf Stream variability issue? The single data point that is outside the model range is from Mg/Ca, and the Mg/Ca is also way out of the model range at the more stable site 609, where other proxies do fall within the model spread. So perhaps it is more of a Mg/Ca proxy issue?

Section 3.1.2: About half of the models (6 of 13) do not show that the correlation between AMOC and SST is stronger at the mPWP, so the statement that the SST is more sensitive to the AMOC during the Pliocene could be misleading as a general statement. I suggest you add the mean (and perhaps STD) of the models' correlation coefficients so one could see if and where the correlations are stronger during the mPWP than the PI and also where they are positive and strong in particular. If nothing obviously pops up, then it illustrates clearly the model-dependency of the correlations which is also important to keep in mind.

Figure 3 Caption: Shouldn't that be "95% confidence level"?

Section 3.2: While the AMOC and meridional density gradients are often correlated, the AMOC is not driven by meridional density gradients. Instead it is pressure, which combines density and the vertical stratification, that drives overturning (De Boer et al., 2010, doi.org/10.1175/2009JPO4200.1). The meridional pressure gradients can be estimated by  $\Delta \rho \cdot H^2$  (meridional density gradient times the square of the depth of the max of overturning streamfunction). It may or may not make a huge difference here, but at least the text should be clear that a correlation with the density gradient is only expected if the depth of the overturning is not affected by processes that are not directly related to the strength of the AMOC (like AABW). Just like if the AMOC correlates often with a meridional salinity gradient we would not make the general statement that the AMOC is driven by meridional salinity gradients.

Figure 4: Could you add a little space between the left and center column and center and right column to make it easier to figure out to which subplot the vertical text belongs? The caption says the density gradient is plotted against the AMOC anomaly. Is it not more conventional to say the y-axis is plotted against the axis? (As in dependent variable against independent variable?).

Figure 4 and L273->: As discussed in the general comment, it would be good to add the correlation coefficient here to compare with assumed forcing by freshwater changes. Also, could you add (in the supplement perhaps) the correlations of AMOC with the northern band of salinity/density and the correlations with the southern band salinity/density band separately to see whether the changes in the gradient originates more from the north or south? It could strengthen the argument that it all comes from the Bering Strait closure or it could force a rethink of that.

L270 with regards to the GISS2.1G, on what evidence is the statement based that the deep water is formed north of 65N? When looking at the streamfunctions in Figure 1 in Zhang et al. (2021) this is not obvious, in fact, in other models the streamfunction stretches further north and the maximum overturning is at a similar latitude in GISS than the other models. From a quick inspection it seems that the GISS is the only model that has a maximum overturning deeper than 1km. Have you tried calculating the meridional gradients over a deeper layer?

L299: Perhaps change "into the Atlantic" here to "into the Arctic", since you are discussing changes in the Arctic salinity.

Figure 7: Could you use the same y-axis scale for these FW transports so they are more easily comparable?

Figure 8: Comparing Figure 4i and 12b suggest a relation between SSS and PmE in the North Atlantic, with models having a strong salinity gradient, like EC-Earth, also having large PmE anomalies and vice versa (like GISS and CESM1.2). The correlation is not great, but see my general point (2) that argue that this does not negate a role for PmE for salinity. On a visual point, the figure would be easier to read with another contour interval level for the salinity anomaly, and a split of the positive and negative contours into solid and dashed lines.

L365: Is the higher PmE attributable to more evaporation from the higher? Presumably a lot if this rains out downwind?

L413: What is the advantage of the pseudo-sverdrup transport analysis in Figure 11 over calculating the actual southward gyre transport, as in Figure S6? The lack of changes in the interior in Figure S6 quite is illuminating and arguably a more direct measure of the wind-driven transport.

L440: In theory, if the gyre volume transports are the same, the heat and freshwater transport must by definition be 100% driven by east-west differences in T and S. The correlations should be stronger if you calculate it according the suggested method in the general comment 1, since you would then not include the superficial gyre contribution below the wind-driven layer into gyre heat and freshwater transport. And playing with the place where the zonal differences are taken might help. But in theory Figs 12a-d together with Figure 11 or Figure S6, should suffice to illustrate that increase gyre H/F transports are due to zonal T/S differences.

L501-508: I suggest leaving out this description of the a-priori expectations. It's difficult to follow and turns out not to be substantiated by the results.

L535: The closing of the Bering Strait is likely the driver of the AMOC changes, but the sentence "is thereby the driver" is arguably too strong, since it has not been shown in a rigorous analysis. For the pliocene runs, the freshwater input from the Arctic was certainly lower and could have driven the higher salinity, but also the PmE was also lower. The PmE did not correlate to the AMOC well, but that was an intermodel correlation. No similar correlation was attempted between the AMOC and the Bering Strait freshwater input for instance, or between PmE versus salinity and Arctic freshwater input versus salinity. Figures S3 for instance shows that in GISS and HadCM3, the Bering Strait freshwater

input was really weak in the PI, yet they both have a stronger than average AMOC increase during the mPWP. So these things are still a bit complicated. Exploring this further may very well be beyond the scope of the current manuscript, in which case one could settle for a reasonable though somewhat more careful "plays a discernable role". Similarly, lines 10-11 can be a bit more carefully stated.

All in all this is a really interesting thought provoking paper.