Rohrschneider et al. 2021, response letter

The depth scales of the AMOC on a decadal timescale

With the authors' response we re-initiate the publication process and soon submission of the revised manuscript. We are grateful for the many comments which have been posted by the anonymous referees. In the following we outline the future changes on the present manuscript. Our study is based on two sets of experiments only but we believe it will be a major contribution to the scientific community because the local wind forcing dependence of the AMOC has not been explored yet.

We changed the manuscript substantially. We focus on the wind experiments only and neglect the global warming experiment. The content of the paper is now well summarized by the abstract.

We use wind sensitivity experiments to understand the wind forcing dependencies of the level of no motion as the depth of maximum overturning and the e-folding pycnocline scale as well as their relationship to northward transport of the mid-depth Atlantic meridional overturning circulation (AMOC). In contrast to previous studies, we investigate the interplay of nonlocal and local wind effects on a decadal timescale. We use 30-year simulations with a high-resolution ocean general circulation model (OGCM) which is an eddy-resolving version of the Max Planck Institute Ocean Model (MPIOM). Our findings deviate from the common perspective that the AMOC is a nonlocal phenomenon only, because northward transport and its depth scales depend on both nonlocal Southern Ocean wind effects and local wind effects in the northern hemisphere downwelling region where Ekman pumping takes place. Southern Ocean wind forcing predominantly determines the magnitude of the pycnocline scale throughout the basin, whereas northern hemisphere winds additionally influence the level of no motion locally. In that respect, the level of no motion is a better proxy for northward transport and mid-depth velocity profiles than the pycnocline scale, since the wind forcing dependencies of the level of no motion and maximum overturning are equal. The changes in maximum overturning with wind forcing are explained by the changes in the level of no motion only. This is because wind-driven Ekman compensation is baroclinic and occurs above the level of no motion, and the internal vertical velocity shear that is not influenced by the external Ekman cells stays approximately constant. The analysis of the wind experiments suggests a hemisphere-dependent scaling of the strength of AMOC. We put forward the idea that the ability of numerical models to capture the spatial and temporal variations of the level of no motion is crucial to reproduce the mid-depth cell in an appropriate way both quantitatively and dynamically. (line 1-17)

We answer explicitly the following research questions.

This paper presents an analysis of wind sensitivity experiments in order to provide insight into the wind forcing dependence of the inter-hemispheric circulation by understanding the behavior of the depth scale(s) of the AMOC. (line 28-30)

…

Understanding the wind forcing dependence of the AMOC by understanding its depth scales makes the underlying research question twofold, in the sense that we discuss the wind forcing dependence of the AMOC using the depth scales and we discuss whether the depth scales are proxies for northward transport to understand the wind forcing dependence. We hypothesize that the level of no motion is a proxy for northward transport in the inter-hemispheric cell because the background vertical velocity shear of the meridional velocity may stay constant under changing wind forcing. (line 62-67)

…

We focus on the inter-hemispheric region 30S-30N and analyze the interplay of nonlocal wind forcing over the Southern Ocean and local wind forcing in the northern hemisphere downwelling region where Ekman pumping takes place. (line 70-71)

….

We address the question how changes in both nonlocal and local wind forcing influence the AMOC. We hypothesize that the influence of northern hemisphere winds on the AMOC is substantial. (line 85-86)

We changed the discussion accordingly and discuss the wind forcing dependence of the AMOC, the mechanism of the wind forcing dependence, and the depth scales as proxies for meridional flow. Theses topics are not limited to the discussion but the major focus of the present manuscript. The paper is much more tailored regarding the focus of the present manuscript. In this connection, we did rewrite the introduction, say why our high-resolution simulation is necessary, elaborate on the mechanism of the wind forcing dependence with a new figure, inter alia.

We improved the writing considerably throughout the manuscript. In this response letter, we cannot state all changes made, but we made changes in every section in order to improve the flow of the paper.

Please review both response letters as they are entangled due to the sheer amount of the changes made.

I)-V) : major comments

VI) : minor comments

:*comment*

:response

I) REFEREE 1

The paper is very poorly motivated. In the Introduction, the 1st paragraph is okay, the 2nd paragraph is a little dense, and the 3rd and final paragraph is very hard to read and does not do a good job of explaining why these particular experiments were done. The 3rd paragraph of the Intro skips back and forth between mentioning subtropical gyre theory (Luyten et al), thermocline behavior due to diffusion and Southern Ocean wind stress (Vallis 2000), and northern high latitudes (Cessi, 2018). Three depth scales are mentioned, but the "advective depth" isn't defined. Wind stress curl (Cabanes et al, 2008) is mentioned, but again without much explanation. Reference is made to "local" versus "nonlocal" wind influence but its unclear if "local" refers to wind and thermocline thickness in the same hemisphere, wind and thermocline both in low/mid latitudes, or wind and thermocline in the same latitude band. For this reason the paper does not clearly state what question is being asked. It doesn't give motivation for the particular wind patterns used.

We spent much effort into the motivation and writing style of to improve the new paper. In fact, these two issues are entangled and the writing style strongly influences the perception of the reader about the motivation and definitions and arguments. The author's writing style is to write in a condensed way. With the third paragraph we now try to synthesize the current research stage of the nonlocal and local wind forcing dependence of the AMOC, and we try to guide the reader through the research questions. Throughout the present study nonlocal wind forcing dependence means the dependence of the basin-wide AMOC on winds over the Southern Ocean and local wind forcing dependence means the dependence of the AMOC in the northern hemisphere on the surface winds over the northern hemisphere downwelling region where Ekman pumping takes place. Considering the wind forcing, we change the meridional and zonal velocities because we are especially interested into the local wind forcing dependence of the AMOC in the northern hemisphere downwelling region. The latter is related to the wind stress curl. We are adding explicit explanations. Based on our changes we state the research questions on the wind forcing dependence more clearly, and we state why we analyze the depth scales of the AMOC.

We propose to change the 2nd, 3rd, and 4th paragraph of the introduction in the following way:

review line 33-86

Intro also does not explain why an eddy-resolving model is used.

The OGCM used in the present study is a high-resolution eddy resolving MPIOM version (TP6ML80). From an overarching perspective, we simply expect a more realistic simulation compared to low-resolution MPIOM versions. In section *Experiments and Methods* (2) we mention that we assume better model physics in terms of the resolution of mesoscale eddies as well as wave propagation, compared to low resolution models. In this connection, a published study is lacking, but Gutjahr et al. (2019) already indicate that the high-resolution MPIOM configuration reveals the most realistic simulation compared to low-resolution MPIOM versions in terms of the mean state of the ocean. For instance, a cold bias in sea surface temperature over the Southern Ocean is strongly reduced, because the resolved eddies better influence the flattening and cropping of isopycnals compared to the GM thickness diffusivity parameterization. The latter is switched off. The authors realized a preliminary analysis of the effect of model resolution on the wind forcing dependence of the AMOC. They basically compared TP6ML80 with GR15L80 (1.5 degrees horizonal resolution and 80 vertical layers) and find that qualitatively the wind forcing dependence of the level of no motion is the same (the dependence on nonlocal Southern Ocean wind forcing and local wind forcing over the northern hemisphere downwelling region). However, the horizontal transmission of density signals is sensitive to the horizonal model grid and the accumulation of vertical shear is sensitive to the vertical model grid, which inter alia changes quantitatively the relationship between the depth scales and meridional velocity profiles. Concerning the current state of model development, these details matter and are a source of uncertainty. We now outline in more detail the need of the high-resolution MPIOM version for our scientific study. We explain the advantage of the high resolution MPIOM version already in *Introduction* (1) now and then extend it in *Experiments and Methods* (2). Beyond the scope of the paper, we do not integrate a model inter-comparison.

We propose to change the 5th paragraph *Introduction* in the following way:

review line 88-96

We propose to change the 1st paragraph of *Experiments and Methods* in the following way.

review line 109-120

The computational expense of an eddy-resolving model forces the experiments to be very short compared to the adjustment timescale of the large-scale thermocline to wind.

As outlined in *Experiments and Methods* (2), the experimental burden of an eddy-resolving simulation is large and we therefore use 30-year wind experiments only. The forcing is switched-on at year 1980, and we analyze the time window 1991-2010 after the realization of major adjustments on a decadal timescale. The wind experiments are justified by the robust adjustment of the AMOC and density field on the timescale considered here. The experiments are not fully equilibrated but major adjustments are realized on a decadal timescale. That is, the wind experiments reflect the wind forcing dependence of the AMOC. For instance, the basin-wide transmission of density signals by wave propagation (e.g. Rossby waves) occurs on a pentadal and decadal timescale. The actual adjustment timescale of the pycnocline is longer and may depend sensitively on the experimental set-up. We now discuss the adequacy of the 30-year simulations in *Experiments and Methods* (2).

We propose to change the 2nd paragraph of *Experiments and Methods* in the following way.

review line 121-135

II) REFEREE 1

Figure 3 shows the wind effects on isopycnlas, as stated in para 2 of the section. However, paragraph states "displacement is scaled by ηw*". How do we know this from the figure or from the experiments? Why do "We expect considerable changes in the level of no motion"? After stating that its easy to see why changing the stratification changes the Level of No Motion, the paragraph gives an argument I can't follow. "displacement of the isopycnals... is small" because "change in deep vertical shear is presumably small". I don't see how one is related to the other, or why the change in deep shear is presumed small. And why do density differences at the advective depth imply changes in velocity at mid-depth? What is mid-depth – 2 km? The paragraph mentions that zonal pressure gradients are related to the Level of No Motion, but I don't see how that fact helps us since the paper doesn't present any information about the zonal pressure gradients.*

The maximum change in density between the wind experiments coincides with the advective depth scale. The latter is proportional to Ekman pumping and thereby to the wind stress curl which displaces isopycnals downward. We now state this explicitly as it is a matter of writing. Furthermore, we now provide some theoretical background such as thermal wind balance. In general, the mechanism how the level of the motion changes with wind forcing and how we understand the wind forcing dependence prevails throughout the new manuscript now. It is a major research question which is reflected in the abstract and introduction. We did rewrite the section on density stratification in order to meet the comments made by the referee. Our findings suggest that changes in zonal pressure gradients at the advective depth are related to significant changes in the level of no motion of the AMOC, because the vertical velocity shear stays approximately constant with wind forcing (formally a matter of mathematical integration). Section (4) closes the gap and explains explicitly how much of the changes in volume transport can be explained by the level of no motion in the case that we hold the vertical velocity shear constant. To illustrate, synthesize and state this more explicitly, we added a new paragraph at the end of section (4) and neglect theoretical arguments in the section on density stratification, section (3). We try to make better connections.

Besides changing the abstract and introduction as stated above, we propose to change section (4) in the following way. We neglect the mechanism is section (3).

Section (4):
In this connection, we analyze whether there is a direct relationship between the vertical velocity profiles described by $\frac{\partial {\psi}}{\partial {z}}$ and the changes in the level of no motion $\eta_\mathrm{\psi}$, by computing the meridional averages 30S-10S and 10N-30N (Fig. 8a,b). We analyze how much of the velocity profiles can be predicted by the level of no motion only. For this purpose, we assume vertical velocity shear that is constant with changing wind forcing. (line 341-344)

…

Integrating the velocity profiles vertically, however, the Ekman cells should cancel out such that the level of no motion is a proxy for northward transport.

We therefore show the changes in maximum overturning that are associated with the level of no motion only by analyzing the vertically integrated transport. Fig. 9 shows the total maximum overturning streamfunction ψ_t and the maximum overturning streamfunction $\psi^\mathrm{*}$ in the case that we hold the vertical velocity shear constant. We find that the changes in total maximum overturning ψ_t are explained by the changes in the level of no motion to a very large degree. The maximum overturning streamfunctions ψ_t and $\psi^\mathrm{*}$ are approximately congruent, and the vertical velocity shear $\frac{\partial^2 {\psi}}{\partial {z}^2}$ stays approximately constant with wind forcing on the timescale considered here. There is deviation at the equator due to systematic errors that arise from perturbations in equatorial upwelling. Away from the equator, however, the differences between the 2XSH and 2X experiments arise solely from the differences in the level of no motion. The mechanism how the changes in wind forcing translate into changes in the level of no motion $\eta_\mathrm{\psi}$ is thus easy to comprehend from a generic point of view. Small changes in the zonal pressure gradients at the depth range scaled by advective depth scale η_w are related to substantial differences in the level of no motion $\eta_\mathrm{\psi}$, because the internal velocity shear that is not influenced by the external Ekman cells hardly changes between the wind experiments. (line 358.-373)

III) REFEREE 1

Sec 4 shows that the subtropical cells are largely confined to the top few hundred meters, which has long been known (see for instance McCreary & Lu, 1994, JPO). Para 2 asserts that this suggests that "$\eta\psi$ is a proxy for ψt rather than... ψg". Doesn't it show the opposite? If the Ekman cells are above $\eta\psi$, shouldn't their transport be independent of $\eta\psi$? The last paragraph of the section seems to say that if we assume that the vertical shear is constant (why should it be?), than changes in $\eta\psi$ produce changes in volume transport. However, it is unclear to me if the paper actually shows that. Again, the difficult writing style makes it harder to tell.

Our study supports the perspective that the interior return flow of the surface Ekman flux is baroclinic. McCreary and Lu (1994) already show theoretically that the subtropical cells are baroclinic; i.e. the interior return flow of the surface Ekman flux is baroclinic. However, in the current literature on the AMOC it is not well established to assume a baroclinic interior return flow of the surface Ekman flux on the timescale considered here. For instance, as outlined in the paper, a range of studies assumes that the interior return flow of the surface Ekman flux is barotropic even on longer than monthly timescales. Using an idealized OGCM experiment, Williams and Roussenov (2014) show that the return flow is indeed baroclinic on an interannual timescale. Our results support this finding but it is not the main focus of our research. However, it is important to understand the geostrophic flow of the AMOC and disentangle different contributions of the meridional velocity field in order analyze whether the level of no motion is a proxy for northward transport. The level of no motion is a proxy for the total maximum overturning streamfunction despite the Ekman cells because the surface Ekman flux is compensated baroclinically above the level of no motion. Also commented and a matter of writing, in Fig. 8 we assume that the vertical velocity shear is constant because we would like to show how much of the changes in volume transport between the wind experiments is attributed to the displacement of the level of no motion only. We make better connections, which is outlined in the response to comment II.

IV) REFEREE 1

In general, almost every sentence is difficult to read. The authors add words to the sentences that do not give any information, and they omit words that would specify what they are talking about. Often the connection between one sentence and the next is not clear. Below, I give some examples of difficult paragraphs. Unfortunately, many paragraphs have similar problems. It would help if paragraphs were not so long. For instance in the 2nd paragraph of the Intro, "The depth scale itself is determined..." can start a new paragraph, "Different assumptions like..." can start a new paragraph, and "The present study addresses" can start a new paragraph. Similarly, paragraph 3 can be divided into 3 or 4 paragraphs, as can many other paragraphs throughout the paper. Ideally each paragraph gives one main idea, otherwise its easy for the reader to get lost in overlong blocks of text.4

As stated above, we spent much effort into the writing style of the new paper. As stated above, the author's writing style is to write in a condensed way but having transitions between the sentences in order to facilitate comprehension. We meet the comments on the writing style made by the referee and now write in a more simple way and add definitions such as defining *wind stress curl* or *nonlocal and local*. Focusing more on the wind forcing dependence of the AMOC now, we state more clearly the questions that are being asked and why we need the experiments or wind patterns. Finally, the author's style is to have paragraphs that describe qualitatively proper sections. We reviewed the sections in order to ensure that the reader can follow the arguments more easily. We tried to improve the writing throughout the paper. We do not comment on the writing examples given by the referee but meet them during our revision.

We do not list the changes made but we improved the writing throughout the manuscript. We make better connections and transitions.

All minor comments or suggestions on the writing style are met.

Small issues:

Sec 1 para 3: paragraph emphasizes lack of work on effects of northern hemisphere wind on overturning, but Klinger et al. (2003) and (2004) both looked a effect of Westerlies in both hemispheres.

There is a lack in literature regarding the northern hemisphere downwelling region where Ekman pumping takes place. Your propositions are very valuable but would distract from the major flow of the paper.

V) REFEREE 2

But my main difficulty here: which of these is the main focus of the paper, and what is the take-away message? About 3/4 of the paper focuses on the wind experiments, and if the title were changed to reflect this topic, the paper would very much read like this was the primary subject. But the paper also analyzes the global warming experiment, which (given the ms. title) suggests that this study is meant more as a contribution about AMOC depth scaling and theory (?) So which is it?

A main difficulty stated by the referee is the main focus of the paper. The aim of the study is to understand the wind forcing dependence of the AMOC by understanding its depth scales. That is to say, it is twofold in the sense that we discuss the wind forcing dependence of the AMOC and we discuss whether the depth scales are proxies for northward flow to understand the wind forcing dependence. Hitherto, the main focus on the wind forcing dependence of the AMOC suffered in some parts of the wind experiments and in the global warming experiment. We therefore strengthened the main focus on the nonlocal (Southern Ocean) and local (northern hemisphere downwelling region) wind forcing dependence throughout the present paper.

Besides adding more context in each chapter, we do so now by rewriting section (4) on the wind experiments (the relationship between the depth scales and meridional velocity profiles), as well as *Introduction* (1) and *Discussion* (5). Considering section (4), we now focus on the mechanism on the wind forcing dependence of the AMOC and refer to the experimental setup of the present study. Deep velocity shear below the advective depth remains nearly constant, whereas the velocities above the advective depth are altered, which together changes the level of no motion. In summary, section 4 provides the opportunity to understand the wind forcing dependence of the AMOC by analyzing the relationship between the depth scales and meridional velocity profiles in more detail, and disentangle different contributions. We now generate some insights on the maximum overturning streamfunction in the case that the vertical velocity shear is constant with wind forcing, using the 1X reference experiment to compute the constant vertical velocity shear. This maximum overturning streamfunction is the maximum northward transport that arises from the changes in the level of no motion only, and we find that it explains the changes with wind forcing in very large degree by vertical integration. Please refer to the response of comment III. We now focus on the wind experiments only. We neglect the warming experiment because it is not the main focus of the paper. We now discuss wind forcing dependence and global warming in *Discussion* (5). In a future study, we would like to additionally analyze whether local Ekman pumping in the northern hemisphere downwelling region substantially influences the adjustment of the AMOC to warming.

The discussion mirrors the content of the paper, which has been changed accordingly throughout the text. We propose to change *Discussion* (6) in the following way.

line 375-418

In line with the current understanding of the Atlantic circulation, Southern Ocean winds boost the strength of the AMOC and change density stratification throughout the basin \citep[e.g.][]{vallis2000,klinger2003,klinger2004,klinger2009}. Northern hemisphere winds over the downwelling region additionally influence the meridional flow and density stratification locally, which is commonly ignored in the scientific literature on the AMOC. The present study is based on simulations with an eddy-resolving OGCM on a decadal timescale rather than a fully equilibrated experiment. We find a robust adjustment of the AMOC and density field, which demonstrates the realization of major adjustments due to wave propagation, and the 30-year simulations are long enough to analyze the wind forcing dependencies of the depth scales and northward transport. The wind forcing-dependence of the AMOC is reflected by the wind experiments.

The findings of the present study support the pycnocline model described in \citet{gnanadesikan1999} in the sense that Southern Ocean wind forcing deepens the pycnocline scale and the level of no motion and strengthens the AMOC. However, local wind forcing over the northern hemisphere downwelling region additionally influences the level of no motion and northward transport locally. In that respect, the level of no motion is more appropriate to scale northward transport than the pycnocline scale. By artificial modification of density gradients in OGCM experiments, \citet{griesel2006} and \citet{deboer2010} indicate that the pycnocline scale does not scale northward transport at all. By contrast, we provide insight on the scaling behavior of the depth scales from a conceptual point of view, and the pycnocline scale fails to scale northward transport in the northern hemisphere.
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Wind stress curl variations at the surface translate into changes in the AMOC. The changes of the AMOC with changing wind forcing in the inter-hemispheric region are explained by the changes in the level of no motion. The internal velocity shear that is not influenced by the external Ekman cells remains constant on the timescale considered here. In contrast to what is stated in \citet{cabanes2008} who analyze interannual variability, the forcing imposed by the wind stress curl at the surface does not substantially change the vertical shear but the reference depth of the AMOC shear component. Our findings also deviate from \citet{levermann2010} who evaluate the pycnocline model using a model of intermediate complexity. They analyze equilibrated experiments which reproduce the response to Southern Ocean wind forcing and focus on meridional density gradients instead of zonal density gradients to represent vertical velocity shear. Using meridional density gradients instead of zonal density gradients is based on the assumption that these gradients are proportional and have the same order of magnitude, and zonal and meridional velocities compare well with one another. According to their findings, both the pycnocline scale and meridional density gradients vary, while according to our study the internal velocity shear remains fixed. We speculate that our high-resolution simulation better simulates velocity shear.

The displacement of the level of no motion in the MPIOM wind experiments approximates the conditions in the interior with the Ekman cells mainly cancelled out. Comparing the wind experiments, the ocean response at the upper levels is much more complex than the response at the deeper levels, which is mostly related to the baroclincity of the interior return flow of the surface Ekman flux. However, integrating vertically, the changes that are associated with the level of no motion give approximately the changes in the total maximum overturning streamfunction with changing wind forcing. As a general contribution and supporting the theoretical considerations made in \citet{mccreary1994}, our findings give baroclinic Ekman compensation which has been demonstrated in an idealized way by \citet{williams2014}. Baroclinic Ekman compensation may depend sensitively on the resolution of an OGCM.

The wind forcing dependence of the AMOC suggests that the temporal adjustment of the AMOC to global warming is not independent of location. Both nonlocal Sothern Ocean wind forcing and local wind forcing in the northern hemisphere downwelling region are likely to influence the adjustment of the level of no motion and northward transport in the inter-hemispheric region.

We are going to keep the experimental strategy which is consistent with the necessary changes.

VI) REFEREE 2

As one example of what seemed underwhelming in regard to building on AMOC theory, while the Bryan 1987 scaling is cited and its general approach explained, the ms. does not explain how this has been used to predict scaling of the AMOC to different parameters, the most explored being diapycnal mixing, but also density gradients. Might this be relevant in the 4XCO2 expt? And although Levang and Schmitt is cited, there is little discussion of how new findings here might relate to their study (note, not sure I understood the sentence l. 421). I would think the large spread of AMOC changes in CMIP-class models might be excellent motivation for the 4xCO2 experiment, If the authors wished to explore this further.

We would have added considerations on advective-diffusive balance (Bryan 1987) in order to add some explanations on the pycnocline scale in the global warming experiment. However, we neglect the global warming experiment in the new manuscript. The connection to Levang and Schmitt (2019) is simple. In the northern hemisphere downwelling region, positive salinity-induced changes in density counterbalance negative temperature-induced changes in density. The latter causes a weakening of the AMOC because the geostrophic shear component of the AMOC is altered. The anomalies in density (salinity and temperature) are advected from the surface to deeper levels due to the forcing imposed by the wind stress curl at the surface. In general, there is less spread in the wind stress curl at the surface among CMIP6 models but the translation of the forcing into density and pressure anomalies at the advective depth remains a major source of uncertainty. The authors look forward to explore the influence of local Ekman pumping on the AMOC in CMIP6 global warming experiments in a subsequent study.

Motivation for model choices was lacking. If this work was intended as a more conceptually-oriented contribution, could this be accomplished using a coarse, idealized setup? Or maybe, both a (cheap) coarse, idealized setup could be contrasted with the realistic run? While there is some hint in the ms. that the eddying capability is important in particular for accurate wave-propagation (given the decadal adjustment timescale), this is not clear to me. Or, why not run a coarse model to equilibrium? Is the decadal adjustment an element of the story (i.e. as implied in title) ? I'm not saying this study needs to be redone with a different setup, just that there is scant justification for the model setup used.

We make considerations on the model choice and experimental strategy above (I).

On figures, many lines labelled as "black" were more gray to me, and "opaque" vs. "transparent" were better described as red vs. pink, for example. Is dotted blue line missing in 8a?

The dotted blue line and the dotted red line overlap, since the level of no motion between the 2XSH and 2X experiments is approximately equal south of the equator.

l. 40 discusses "diapycnal upwelling in the tropics" after mentioning the Gnanadesikan (1999) model. Although perhaps a bit beyond the scope of this paper, I might argue Gnanadesikan is a single basin model that explicitly assumes the adv-diff balance in its overturning hemisphere, whereas the model here is global and one might assume the important advective-diffusive balance (justifying an e-folding pynocline scaling) might be occurring in the (larger) Pacific basin. Again, if the main focus is as a conceptual AMOC contribution, it is a bit disappointing to not even comment on other possible relevant issues such as this, nor advance the science with new or revised conceptual models, nor use new results to go back and comment more extensively on conceptual understanding in the literature.

We switched the focus of the paper and narrowed the research questions. We do not raise expectations which cannot be met. Commenting other relevant components such as the global pycnocline is beyond the scope of the paper now.

It would seem more could be explored about the relationship between zonal and meridional density gradients. Of course, the pycnocline scaling says nothing directly about zonal density gradients, in contrast with level of no motion (albeit somewhat indirectly).

We strengthened the focus on the wind forcing dependence of the AMOC. Therefore, it is nearly impossible to include an analysis on the relationship between zonal and meridional density contrasts. We focus on the role of zonal density contrasts and vertical velocity shear with respect to the changes that are related to the level of no motion.

l. 116 mentions "monthly climatology of reanalysis wind stress is doubled"; by this I presume the reanalysis wind is available every six hours or thereabouts, and the six-hourly variability is *preserved but the monthly mean wind is doubled? Or please explain. Does high-frequency forcing play any role?*

We only double the monthly mean climatology of the wind stress curl while the anomalies are unchanged. We do not consider high-frequency variability of the AMOC.

l. 153 "surface buoyancy fluxes change continuously": this could be said about any model with a seasonal cycle or interannual forcing. I think what is meant is that the 4xCO2 experiment adjusts slowly to the step change, with the surface forcing changing as a function of ocean-atmosphere state in the coupled setup. In contrast the wind experiments are not coupled, and the adjustment is assumed to occur on a decadal time scale, simplifying the analysis to a comparison of 1991-2010 mean states

The warming experiment is based on step function forcing of the radiative forcing or the atmospheric CO2 concentration. We would rewrite the statement that the surface buoyancy fluxes change continuously in order to explicitly state that they change constant in sign as a function of time. The transient nature of the 100-year global warming experiment would have made it possible to compare it with the mean states of the wind experiments on a decadal timescale.

l. 280 by "inter-hemispheric regions" I presume the authors are referring to 30S-30N?

We consider the region away from the lateral margins; that is, 30S-30N.

l. 452 didn't follow sentence

The changes in the level of no motion between the wind experiments explain a large fraction of the changes in meridional velocities. Near the surface, however, the signal that arises from the interior return flow of the surface Ekman flux overcomes the signal that arises from the displacement of the level of no motion

Literature

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