REVIEW 1

Nonlocal and local wind forcing dependence of the Altantic meridional overturning circulation and its depth scale

COMMENT
REPLY
CITITATION

Dear reviewer, first of all we would like to thank you for your effort. We were happy to address the major comments in an extended way. We are afraid that we neglected a wide range of the minor comments because we believe these comments are driven by personal belief with respect to an own paper. We hope the paper is publishable according to your opinion which is important to us.

MAJOR COMMENTS

Overview

Revised manuscript is much more coherent, though still not easy to read. It argues that in a global ocean model, the Southern Ocean wind stress controls the Atlantic pycnocline depth, but that the Northern Hemisphere wind stress also influences the thickness and magnitude of the upper limb of AMOC in the northern hemisphere. I recommend one more set of modifications before publication, but I now believe it should be eventually publishable after one more revision.

Main Comments

A. Time Dependence. One essential revision is the need to characterize the time-dependence of their results. While extending the model runs may be prohibitively expensive due to the resolution, the paper should at least talk about the time-dependence for the 30 years of the run. Is there any evidence from the behavior of the overturning or stratification that the 20-year averages taken here would be similar if the next 20 years were used? Is there any evidence that the model is converging so that results may be similar if the run was extended another few centuries? While the paper explicitly says it is only talking about a particular time period, the theoretical framework of the paper is based on steady-state behavior, so its incomplete to not comment further on whether the results are relevant to the steady-state. And if the results are to be relevant to the transient behavior, then the time evolution has to be discussed.

We added a proper section on the robustness of the results (please review the content below) line 407-456

B. Longer low-resolution runs. The paper would be better if an additional set of long runs were done with a non-eddy-resolving grid. This would tell us if the eddy resolution is important for getting the correct sensitivity to wind, and would give further insight into whether the results are indicative of long-term means. This is plausibly beyond the scope of the current paper, so I won’t insist on it, but at least a second paper to check this would be worth considering.

We added a proper section on the robustness of the results (please review the content below) line 407-456

```latex
\section{Robustness of the wind forcing dependence}
In this section we would like to elaborate on the robustness of the results
considering the wind forcing dependence of the AMOC. The question arises whether
the wind forcing dependence of the AMOC found in the short-term integrations of
TP6ML80 (1980-2010) is robust.  In the study we use the time window (1991-2010) in
order to allow for major adjustments at an initial stage. We state that the wind
forcing dependence found in the time window would reflect a quasi-steady response.
This is a strong assumption given that it is actually a transient response within a
short integration time. The adjustment in the density field (Fig. 3) support the
perspective that major adjustments in ocean dynamics to forcing are realized.
However, the wind forcing dependence of the AMOC may still be time-dependent, and
low-resolution model outcome may differ from high-resolution model outcome.
\newline

We first show the full time series (1980-2010) of maximum overturning and the level
of no motion in TP6ML80 (30S-10S,10N-30N) (Fig. 10 a,b,c,d). There is a strong
adjustment and time-dependence in both variables at an initial stage on a decadal
time scale. During the course of the study we have neglected this initial
adjustment by focusing on the time window 1991-2010 only. After the initial
adjustment on a decadal timescale (1980-1990), the wind forcing dependence of
maximum overturning and the level of no motion is robust. Nevertheless, there are
oscillations at low frequency which put into question whether the wind forcing
dependence of the AMOC found in the short-term integration of TP6ML80 is quasi-
steady. We cannot investigate the steady response of the AMOC in TP6ML80 due to the
high computational costs. The temporal changes in the level of no motion, however,
coincide with the temporal changes in maximum overturning in the sense that the
vertical velocity shear of the meridional velocity stays approximately constant
over time.
\newline

\begin{figure*}
\includegraphics[width=0.9\textwidth]{time_image}
\caption{The TP6ML80 time evolution (30S-10S,10N-30N) of (a,b) maximum overturning
and (c,d) the level of no motion after the forcing is switched on. The MPIESM1.2-LR
time evolution (30S-10S,10N-30N)of (e,f) maximum overturning and (g,h) the level of
no motion after the forcing is switched on.}
\label{fig:41}
\end{figure*}

\begin{figure*}
\includegraphics[width=0.9\textwidth]{warming_image}
\caption{The time evolution (30S-10S,10N-30N) of (a,c) maximum overturning and
(b,d) the level of no motion after the forcing is switched on in the global warming
experiments with altered surface wind stress using MPIES1.2-LR. Atmospheric CO$_2$
is quadrupled.}
\label{fig:41}
\end{figure*}

As a next step, we use a AGCM-OGCM coupled low-resolution model to simulate the
wind forcing dependence in a low-resolution counterpart and on a longer timescale
(50 yr). The coupled model better simulates the salinity balance in the OGCM to
which ocean dynamics are sensitive. The coupled model is MPIESM1.2-LR, with the
low-resolution configuration of MPIOM being the OGCM component. The ocean model
(GR15L40) has a horizontal resolution of 1.5 degrees and 40 vertical levels only.
We have a set of four experiments: the 2X experiment in which the zonal and
meridional surface wind stress is doubled throughout the hemispheres; the 2XSH
experiment in which the wind stress is doubled over the Southern Ocean only; the 1X
experiment which is forced under no changes; and the 0.5X experiment in which the
```

zonal and meridional wind stress is halved. We only change the ocean wind stress factor that multiplies the surface wind stress in the coupled model because I am interested in the OGCM dynamics only. It is an online multiplication of each wind stress value at each timestep.
\newline

We find that in the 50 years integrations of the low-resolution model the response is apparently quasi-steady on this timescale (Fig. 10 e,f,g,h). On longer timescales, internal, low-frequent variability may take place. We find that the wind forcing dependence of maximum overturning is similar to TP6ML80 and robust. However, there are major deviations in the level of no motion which does not reflect the wind forcing dependence in the high-resolution model outcome. The general finding that the level of no motion deepens with stronger wind forcing is confirmed, but the details between the 1X and 2XSH experiments are not well simulated. This may be due to model drift in the coupled model, or low-frequency oscillations, or the low vertical model resolution. The level of no motion is sensitive to small variations in the velocity field which may still adjust and oscillate. It seems that the nonlocal wind forcing dependence of the AMOC is less strong and the local wind forcing dependence is much stronger. The vertical velocity shear of the meridional velocity is not constant.
\newline

Disappointed from the finding that the level of no motion may not be well represented in the low-resolution MPIOM configuration, we looked for an alternative way to make sure that the wind forcing dependence of the AMOC is robust. We computed the wind forcing dependence of the AMOC in 100-year global warming experiments with altered surface wind stress, using also MPIESM1.2-LR. We quadrupled atmospheric CO$_2$ and applied the wind stress factor during the forward integration. We initialized with the control experiments with altered surface wind stress at year 30, after having explored that the initialization plays a minor role for the evolution of the AMOC in the global warming experiments. We believe the system is more strongly forced so that the forced underlying dynamics overcome internal oscillations and model drift. Fig. 11 shows the wind forcing dependence (30S-10S,10N-30N) of maximum overturning and the level of no motion in the global warming experiments with altered surface wind stress. Now the wind forcing dependence of maximum overturning and the level of no motion is the same as in the wind sensitivity experiments with TP6ML80.

C. More on Dynamics. I simple way of looking at the effect of Southern Ocean wind stress is that it's Ekman transport pushes water northward, and the resulting current joins the upper limb of the AMOC and returns with NADW. Shouldn't the northern hemisphere wind do the opposite? Isn't that what happens in previous studies? Ekman transport from strengthened westerlies goes southward, which would weaken the upper limb rather than strengthening it. Similarly, that same stronger wind in subpolar gyre would increase upwelling, which might counteract the downwelling associated with deep water formation. It would be helpful if the paper addressed this point. In addition, a key result is that we can think of a change in NH wind moving the depth of maximum streamfunction vertically, which moves a fixed vertical velocity shear vertically, which then determines the change in the overturning. But why should each of these facts (change in depth, constancy of velocity shear) be true? Answers to these questions would enhance the paper though they are not required for publication. line 498-512

We added a section on dynamical components considering our study to the end of the discussion section. Note that the focus of the paper is the new way how to decompose the AMOC and explain the wind forcing dependence. AMOC components away from the inter-hemispheric region play a minor role. Please review the content below.

We would like to use this discussion section to refer briefly to the wind forcing dependence of the AMOC in terms of dynamic components. An outcome of our experimental study is that northward overturning is well approximated by the level of no motion which reflects the wind forcing dependence of the AMOC. We demonstrate that, using the level of no motion, the flow can be subdivided into internal flow and external flow, because the external baroclinic Ekman cells that are directly forced by the surface winds cancel out by vertical integration. Our findings support baroclinic Ekman compensation which makes the level of no motion a proxy for northward overturning. That is to say, meridional Ekman transport in the southern hemisphere as well as in the northern hemisphere do not change the relationship between overturning and its depth. Thus, it does not change the wind forcing dependence of the AMOC because the surface Ekman flux is compensated above the level of no motion. Ekman pumping in the southern hemisphere and in the northern hemisphere do change the relationship between overturning and its depth. The explanation for the changes in maximum overturning and the level of no motion differs between the southern hemisphere and the northern hemisphere. In the southern hemisphere north of the ACC Ekman pumping displaces isopycnals downward that span the basin meridionally. In the northern hemisphere the increase in transport and depth can be explained by continuity and isopycnals are displaced downward only locally. The wind forced change in Ekman puming gives a new advective balance. It forces the flow thus horizontally upstream, and a new dynamical balance establishes downstream. We speculate that in this way maximum overturning and the level of no motion are altered.
\newline

MINOR COMMENTS:

1. Improve Table 1. Make table less wordy and leave out information common to all 3 experiments, so that table looks something like this:

   Abbreviation Name Description
   1X Reference Observed wind stress
   2XSH Double Southern Wind Double wind stress south of 30S
   2X Double Wind Double wind stress at all latitudes

We do not believe that is necessary to change Table 1 and prefer it the way it is.

2. Improve Table 2. Separate and organize variables into groups, make separate “parameter name” and “parameter definition” columns. At the authors’ discretion, I suggest using lower case and upper case symbols to differentiate between variables and parameters, using a capital Z or H rather than lower-case η for depths, referring to “level of no motion” as “Streamfunction depth” or “upper-limb depth”, and referring to “pycnocline scale” as “pycnocline depth” (because it refers to a measurement of model behavior, unlike the advective depth scale which is an estimate calculated from forcing parameters.” I don’t see the need for defining derivatives of ψ in the table because (for instance) $\partial\psi\/\partial z$ obviously means the “vertical derivative of the overturning Streamfunction”.

We believe it is straightforward to capture the content of the Table. We have our own definitions which we use throughout the paper in a consistent way. It is technical.

3. Clarify model spin-up (Sec 2.1). Paper says “we focus on the time-window 1991 to year 2010”. How long before that (if at all) was the model spun up? How close to steady state was it at this point? What does “change the monthly-mean climatology of the surface wind stress only” mean? Does run 1X use daily wind stress, and other runs use daily wind stress multiplied by a factor, or is there some more complicated procedure involving taking monthly means? Or are monthly mean wind stress used for all runs?

Thank you, we did clarify this in the paper.

4. “Mid-depth” is confusing. In Abstract and elsewhere, I suggest replacing “mid-depth AMOC” with “upper limb of the upper AMOC cell” or “upper limb of the North Atlantic Deep Water (NADW) cell of AMOC”.

Done

5. Density Difference Figure. I don’t understand what is gained by looking at density differences in Fig 3ab. Since the reason for examining density is connected to measurements of isopycnals depth, why not just look at isopycnals? Also, rather than using the normalized density (black curves in Fig 3ab), it would be better to use a measure that is closer to the one used to calculate integral depth scale
r(y,z)=(∫_z^0▒(ρ-ρ_r)dz)/(∫_(z_T)^0▒(ρ-ρ_r)dz)
Which by definition gives r=0 at the surface and r=1 at z=z_T. Can then show separate panels for r contours for each experiment.

We do not agree with you. It is straightforward to capture the difference in zonal-mean density or stratification in our study. Furthermore, we define the term stratification and use it consistently throughout the paper.

6. Focus on latitude-band averages for depth. Figure 6 shows averages over latitudes 10-30o in both hemispheres. Extend this to several quantities. Instead of Fig 3c and Fig 4 showing latitude dependence, just show averages for each of the 3 depth scales in each hemisphere. Each panel would contain depths (y axis of panel) for 3 runs (x axis of panel) for northern (upward-pointing triangle) and southern hemisphere (downward-pointing triangle) for a single quantity (streamfunction depth, pycnocline depth, advective depth scale). This would emphasize how each quantity depends on the wind, rather than current version which emphasizes complicated latitude dependence which text does not comment on much. Also Fig. 4 currently is very busy with 6 different curves and one has to concentrate to see the point about the wind different wind dependence in NH and SH. The latitude dependence of η_w is not an appropriate value to plot, since η_w is a scale quantity representing the pycnocline depth for a given gyre, not the detailed geographical variation of pycnocline depth within the gyre.

We do not agree. We think showing a latitudinal dependence of the quantities is the most tangible way to illustrate the wind forcing dependence of the AMOC.

7. Why is advective depth scale included? Currently fig 3. Plots η_w (y) with different values of g'. The values seem arbitrary, and I don't understand why these alternate calculations are graphed. The only significance I can see of η_w is that it depends on √τ. Therefore, maybe just compare η_ρ variations to √τ in the plot I suggest in (6) above.

`We do not agree. The advective depth scale is directly related to local Ekman pumping or the wind stress curl at the surface. Furthermore, different g' s show the possible parameter space.`

8. What is the significance of the geostrophic transport? Below the Ekman layer (top 50 m or less?), shouldn't velocity be geostrophic? Or does nonlinearity from the eddies add an important term? The max geostrophic streamfunction shown in Fig 5 is some kind of perturbation due to the Ekman transport? How is it relevant to the discussion of the overturning? If it isn't, why is it discussed?

`We added explanatory statements to this section.`

9. Maybe separate transport and depth data. Since I think the depths should be plotted as a function of experiment, maybe the NH and SH transports should be plotted that way as well rather than plotted against depth as in Fig 6. Then again, the plot does do a good job showing that volume transport varies with η_ψ, so I wouldn't object to keeping it anyway. The dashed lines are distracting though; if graph kept as-is, eliminate them and perhaps use a more distinct symbol if geostrophic data is retained in revision – perhaps open symbol instead of lighter symbol. The transport estimated from shear and η_ψ, currently shown as a function of latitude in Fig 9, could also be included in the figure (10-30o average for each hemisphere as a function of run).

`We do not agree. We think the way the figures are displaced is the best way to understand the content of the paper.`

DONE

11. Abstract Clarity. The Abstract is okay as written, but has a number of awkward elements. Here I list those elements and give an alternative text for the first 2/3 of the Abstract. The authors can use all, part, or none of the alternative text at their discretion.
"wind forcing dependencies" is a little vague
"level of no motion as the depth of maximum overturning" is trying to say that the 1st phrase = 2nd phrase, but readers may be confused by "as the"
"interplay of nonlocal and local" also kind of vague – at this point reader still doesn't really know what abstract is talking about
"downwelling region where Ekman pumping takes place" Actually the wind is changed over entire hemisphere, so not clear that it's the Ekman pumping location that is key
In my rewrite, I try to give the reader a bit more context first, and to describe the issues and experiments in a more concrete way.

`We do not agree. The abstract is written in a technical way, in a way that corresponds to the content of the paper. It is technical. We do not believe that is necessary to show further content and context.`

REVIEW 2

Nonlocal and local wind forcing dependence of the Altantic meridional overturning circulation and its depth scale

COMMENT
REPLY
CITITATION

Dear reviewer, first of all we would like to thank you for your effort. We were happy to address the major and minor comments. We hope the paper is publishable according to your opinion which is important to us.

I appreciate the focus now on the depth scale and wind experiments (removing the 4xCO2 part), and while this is an improvement, some problems remain.

Starting with the introduction and motivation, in my opinion the authors raise two important questions that are often overlooked or not considered especially relevant, specifically 1) regarding the relevant AMOC depth scale and 2) regarding local vs. non-local winds. The content here is good, and appropriate references are ticked off, but there’s a remaining problem: (given that 1&2 ostensibly seem so different) which of these is the main question and which is used in experimental analysis/support? From the title, the depth scale seems the main question. But reading the abstract and the intro, the wind question seems primary. Which was the question the authors first considered? I think one could take 1 as the main question as explored via 2’s experiments, or take 2 as the main question and hypothesize 1 is the relevant diagnostic to consider/analyze. My point here being that the presentation of the intro comes across as a bit of a jumble, and improvement therein might help with my remaining problems in the manuscript. These two questions are obviously not totally unrelated; some better transitions, motivations, connections in the text etc would help.

We changed the introduction accordingly. We rearranged the structure of the introduction and added explanatory statements. Basically, we analyze the wind forcing dependence of the AMOC and explain the wind forcing dependence of the AMOC by the relationship between overturning and its depth. Considering the introduction, we now first discuss the wind forcing dependence of the AMOC and then explain why we analyze depth scaling and thermal wind.

The structure of the introduction now corresponds to the structure of the paper. line 53-71

The research of the present study is inherently about depth scaling that reflects the wind forcing dependence of the AMOC, because we understand the wind forcing dependence of the AMOC by the behavior of its depth scale. Oceanographers use theoretical scaling relationships to provide conceptual understanding and to estimate the strength of the AMOC in response to different forcings.
…
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. The latter question is implicit in the sense that we need to answer this questions in order to explain the wind forcing dependence of the AMOC. We hypothesize that the level of no motion is a proxy for northward transport in the inter-hemispheric cell because the background velocity shear of the meridional velocity may stay constant under changing wind forcing. In this connection, the study is based on different ways or definitions which describe meridional flow in order to analyze how the changes in wind forcing are translated into the changes in the AMOC. We demonstrate that, using the level of no motion, the flow can be subdivided into internal flow and external flow, because the wind-forced Ekman cells, which give the Ekman transport and its compensation, are found to be baroclinic and cancel out by vertical integration above the level of no motion. The internal flow is directly related to the AMOC wind forcing dependence.
\newline

I found going through the results was difficult; it took multiple read-throughs and a fair amount of effort. Below I identify some specific points requiring clarification, but in the bigger picture, main take-aways need to be better identified; in some places I did not follow what questions or points the analysis was attempting to address.

We added explanatory statements.

That being said, a major point seems that eta_rho doesn't change between 2xSH and 2X whereas eta_psi (and psi itself) do. There is some follow-up on what this result suggests in terms of previous work (in section 4), but fairly cursory, and if this is indeed a significant result, more could be said in this regard, even if previous work seems a bit tangential in experiments and conclusions.

We explain it now in an explicit way. line 323-334

At this point, we would like to summarize why it is important to distinguish between the pycnocline scale and the level of no motion considering the scaling of maximum overturning. In this way, we avoid a tangential analysis. The pycnocline scale does not scale maximum overturning, whereas the level of no motion scales maximum overturning. In addition, the pycnocline scale cannot capture the details of stratification, and at the same time we cannot capture the wind forcing dependence of the AMOC when only knowing how density unfolds vertically. The pycnocline scale is commonly taken as appropriate depth scale in current literature but actually it does not reflect the wind forcing dependence of the AMOC. The level of no motion does reflect the wind forcing dependence of the AMOC and is thus more appropriate to scale the strength of the northward flow. Based on these results, in the following we focus on the level of no motion only. Furthermore, the pycnocline scale cannot provide any detailed information about the relationship between overturning and vertical velocity shear of the meridional velocity, which is needed to understand the wind forcing dependence of the AMOC as we learn later on. Even a difference of one grid layer likely makes a significant difference in the accumulation of vertical shear as we have identified above.
\newline

Examining psi_g (ie fig 5) seemed more a distraction than anything fundamental. Thinking about it, the result in fig 5 must be as such (for dynamical balance), but what is the relation of this analysis to the main question?

We explain it now in an explicit way. line 268-279

\subsection{Maximum overturning and its depth}
We now analyze the wind forcing dependence of the northward flowing branch of the mid-depth cell. We compute the total maximum overturning streamfunction ψ_t and the geostrophic maximum overturning streamfunction ψ_g. Conceptually, the differences between ψ_t and ψ_g provide insight on the degree to which the depth scale(s) are proxies for the strength of the AMOC. Computing the geostrophic maximum overturning streamfunction ψ_g, the level of no motion is unchanged, but the clockwise (upper) and counterclockwise (lower) rotating overturning cells are substantially altered. The maximum streamfunction ψ_t includes the surface Ekman flux and the maximum streamfunction ψ_g exludes the surface Ekman flux. However, the surface Ekman fluxes have to be compensated by an interior return flow that changes in relationship between overturning and its depth. With this section we simply answer the question whether the depth scale does scale overturning over the full depth range of the upper AMOC branch, including the surface Ekman layer, or whether the depth scale does scale overturning below the surface Ekman layer. It is important to answer this question because it does not only provide insight whether the depth scale is a proxy for northward flow but it points also in the direction why the level of no motion does scale northward overturning and why there is a certain relationship between overturning and its depth.
\newline

Similarly I did not really understand fig 6 other than reinforcing fig 4's main result (?) ITS

It is a technical summary and therefore important for the subsequent analysis which is introduced. We added some more explanatory statements. line 305-310

Combining our findings from Fig. 4 and 5, we describe the relationship between northward overturning and its depth from a more nonlocal perspective on hemispheric differences. To highlight hemispheric differences in the inter-hemispheric region, we show the meridional averages (30S-10S) and (10N-30N) (Fig. 6). It is technical because we also show the vertical model grid in order to provide an indication for the importance of a single layer change only as well as ability of the depth scales to account for the details in the accumulation of vertical shear. It summarizes the relationship between overturning and its depth. The latter is preparatory for the subsequent analysis. ...

And, not sure what relevant I learned from fig 7 (maybe one could argue the main fig 2 response could be explained as "compensation" effectiveness shown in fig 7?).

We added an explanatory statement. line 363-372

Fig. 7 shows the zonal-mean meridional velocities ($\frac{\partial {\psi}}{\partial {z}}$) in the wind experiments and the difference in $\frac{\partial {\psi}}{\partial {z}}$ between these wind experiments. It shows the importance for the Ekman cells for the meridional flow and that these Ekman cells cancel out above the level of no motion. Considering the relationship between maximum overturning and the level of no motion, we can think of internal flow in which the Ekman cells are canceled as subsequent analysis reveals. This, in turn, explains the wind forcing dependence of the AMOC. The differences in $\frac{\partial {\psi}}{\partial {z}}$ between the wind experiments are strongest near the equator at the upper levels where the vertical velocity shear changes drastically. Taking the difference between the 2X and 1X experiments, we find an increase in $\frac{\partial {\psi}}{\partial {z}}$ south of the equator and a decrease north of the equator. To a substantial extent, these changes can be attributed to the strengthening of the local Ekman cells. The differences in $\frac{\partial {\psi}}{\partial {z}}$ at the upper levels between the different experiments demonstrate that the Ekman return flow is baroclinic and occurs mostly above $\eta_\mathrm{\psi}$. The strong influence of the Ekman cells near the surface suggests that, at these levels, the external wind-driven flow associated with the Ekman cells superposes the internal flow that is associated with the level of no motion. These considerations support the perspective that $\eta_\mathrm{\psi}$ is a proxy for ψ_t rather than a proxy for ψ_g. Small differences in ψ_t emerge in case of weak compensation of the surface Ekman flux below $\eta_\mathrm{\psi}$. \newline

In figure 2, I cannot discern much difference between b and c. Would suggest lighter color shading and black contours at -10,-5,0,5,10,15 etc. It seems to me that the difference in the NH between b and c is very critical to the paper (as shown in fig 4, but not so carefully articulated) but is washed out in the figure here ???

We changed the figure accordingly.

Fig 3, line 212, caption, and other places: to me, plotting density stratification would be drho/dz. What is show here is anomalies in density (normalized by rho_o), plotted in the yz plane (ie y-axis is depth). I would suggest being clearer and more precise in descriptions as such (this is a general issue, and contributes to difficulty in reading). ineWhy are two different g' shown? Scaling relies on the specific estimate not sure g not substantial

We do not change the manuscript because we believe the term stratification as used in our study is clearly defined. Defining a word like stratification is up to us. We show different g' s to illustrate the possible parameter space.

Line 225, "deep isopycnals…" – what does this mean? Depth range

Done

- Lines 249-250 unclear and vague sentence

Done

- Line 252 "deepens … in the northern hemisphere" – doesn't seem to be true 30N-40N (?) downwelling region

We believe it is self-explanatory because we defined the different regions in order to analyze the wind forcing dependence.

- Fig 5 caption: remove "with respect to" (sounds like you are subtracting, or examining anomalies)

Done

- Line 267 Not sure of the purpose of this sentence; I think you mean by definition/construction, the level of no motion and psi_t max coincide? Or am I completely missing something? Suggest removing this sentence and explaining the second sentence better. (see also lines 307-308, 327-328)

Done

- Line 299 "not shown"; wasn't this shown earlier in the paper?

We just clarified.

- Lines 315-317 if you end up keeping the psi_g results, I would provide this explanation earlier on in the paper.

- Line 330 please be more specific as to “mid-depth”, didn’t see this in plot ok

Done

- Fig 8: I think it might be useful to show mathematically what you compute. Several times it is said “hold the vertical shear constant” – does that mean compute the vertical shear from the 1x experiment (as f(y,z)) but then integrate this quantity using the diagnosed level of no motion in 2xSH and 2x? (lines 348-350). But then why is there a dash line for the 1x case? And, might it be useful to plot the shear, to get some sense as to this quantity between runs?

Done

- Fig 8 caption, line 351: not clear what is meant by “deep”. Are you discussing below the level of no motion, or above?

Done

- It was unclear whether the authors felt the (finding of) baroclinic adjustment of the Ekman compensation is a major result. But it would seem to be relevant to the main story, so I think needs to be better woven into the explanation.

Done

We believe with the introduction it is self-explanatory now

- Whether having an eddying model here is critical in these results is also not clear; there are several vague allusions to this, but no clear statement on this point

We added a proper section on the robustness of the results. see below

- I was not entirely satisfied with the justification of the experimental procedure, ie time mean years 10-30 being sufficiently adjusted (I would not have guessed this to be the case, a priori). Perhaps Luschow ref in line 134 could be written more definitively. It would not be inconceivable to have done one high-res run out longer than 30 years to see how well yrs 10-30 adequately captures the quantitative results. Could one assess this using a coarse-res model, or might that muddy any conclusion too much? In any event, it comes across a bit as “take our word for it” rather than having actually attempted due diligence that yrs 10-30 indeed is acceptable for this purpose.

We added a proper section on the robustness of the results.
line 407-456

\section{Robustness of the wind forcing dependence}

In this section we would like to elaborate on the robustness of the results considering the wind forcing dependence of the AMOC. The question arises whether the wind forcing dependence of the AMOC found in the short-term integrations of TP6ML80 (1980-2010) is robust. In the study we use the time window (1991-2010) in order to allow for major adjustments at an initial stage. We state that the wind forcing dependence found in the time window would reflect a quasi-steady response. This is a strong assumption given that it is actually a transient response within a short integration time. The adjustment in the density field (Fig. 3) support the perspective that major adjustments in ocean dynamics to forcing are realized. However, the wind forcing dependence of the AMOC may still be time-dependent, and low-resolution model outcome may differ from high-resolution model outcome.
\newline

We first show the full time series (1980-2010) of maximum overturning and the level of no motion in TP6ML80 (30S-10S,10N-30N) (Fig. 10 a,b,c,d). There is a strong adjustment and time-dependence in both variables at an initial stage on a decadal time scale. During the course of the study we have neglected this initial adjustment by focusing on the time window 1991-2010 only. After the initial adjustment on a decadal timescale (1980-1990), the wind forcing dependence of maximum overturning and the level of no motion is robust. Nevertheless, there are

oscillations at low frequency which put into question whether the wind forcing dependence of the AMOC found in the short-term integration of TP6ML80 is quasi-steady. We cannot investigate the steady response of the AMOC in TP6ML80 due to the high computational costs. The temporal changes in the level of no motion, however, coincide with the temporal changes in maximum overturning in the sense that the vertical velocity shear of the meridional velocity stays approximately constant over time.
\newline

```
\begin{figure*}
\includegraphics[width=0.9\textwidth]{time_image}
\caption{The TP6ML80 time evolution (30S-10S,10N-30N) of (a,b) maximum overturning
and (c,d) the level of no motion after the forcing is switched on. The MPIESM1.2-LR
time evolution (30S-10S,10N-30N)of (e,f) maximum overturning and (g,h) the level of
no motion after the forcing is switched on.}
\label{fig:41}
\end{figure*}

\begin{figure*}
\includegraphics[width=0.9\textwidth]{warming_image}
\caption{The time evolution (30S-10S,10N-30N) of (a,c) maximum overturning and
(b,d) the level of no motion after the forcing is switched on in the global warming
experiments with altered surface wind stress using MPIES1.2-LR. Atmospheric CO$_2$
is quadrupled.}
\label{fig:41}
\end{figure*}
```

As a next step, we use a AGCM-OGCM coupled low-resolution model to simulate the wind forcing dependence in a low-resolution counterpart and on a longer timescale (50 yr). The coupled model better simulates the salinity balance in the OGCM to which ocean dynamics are sensitive. The coupled model is MPIESM1.2-LR, with the low-resolution configuration of MPIOM being the OGCM component. The ocean model (GR15L40) has a horizontal resolution of 1.5 degrees and 40 vertical levels only. We have a set of four experiments: the 2X experiment in which the zonal and meridional surface wind stress is doubled throughout the hemispheres; the 2XSH experiment in which the wind stress is doubled over the Southern Ocean only; the 1X experiment which is forced under no changes; and the 0.5X experiment in which the zonal and meridional wind stress is halved. We only change the ocean wind stress factor that multiplies the surface wind stress in the coupled model because I am interested in the OGCM dynamics only. It is an online multiplication of each wind stress value at each timestep.
\newline

We find that in the 50 years integrations of the low-resolution model the response is apparently quasi-steady on this timescale (Fig. 10 e,f,g,h). On longer timescales, internal, low-frequent variability may take place. We find that the wind forcing dependence of maximum overturning is similar to TP6ML80 and robust. However, there are major deviations in the level of no motion which does not reflect the wind forcing dependence in the high-resolution model outcome. The general finding that the level of no motion deepens with stronger wind forcing is confirmed, but the details between the 1X and 2XSH experiments are not well simulated. This may be due to model drift in the coupled model, or low-frequency oscillations, or the low vertical model resolution. The level of no motion is sensitive to small variations in the velocity field which may still adjust and oscillate. It seems that the nonlocal wind forcing dependence of the AMOC is less strong and the local wind forcing dependence is much stronger. The vertical velocity shear of the meridional velocity is not constant.
\newline

Disappointed from the finding that the level of no motion may not be well represented in the low-resolution MPIOM configuration, we looked for an alternative way to make sure that the wind forcing dependence of the AMOC is robust. We computed the wind forcing dependence of the AMOC in 100-year global warming experiments with altered surface wind stress, using also MPIESM1.2-LR. We quadrupled atmospheric CO$_2$ and applied the wind stress factor during the forward integration. We initialized with the control experiments with altered surface wind stress at year 30, after having explored that the initialization plays a minor role for the evolution of the AMOC in the global warming experiments. We believe the system is more strongly forced so that the forced underlying dynamics overcome internal oscillations and model drift. Fig. 11 shows the wind forcing dependence (30S-10S,10N-30N) of maximum overturning and the level of no motion in the global warming experiments with altered surface wind stress. Now the wind forcing dependence of maximum overturning and the level of no motion is the same as in the wind sensitivity experiments with TP6ML80.