Author's response, discussion gmd-2015-36

As was required by the executive editor, we have modified the title of the manuscript to include the version number of NEMO (3.4), and **we have added a "Code availability" section** at the end of the manuscript.

All the comments from the reviewers have been addressed. In this document, we first show the comments from reviewer 1, then those from reviewer 2 (from p.11). Our response is organised as follows for each comment:

Comment from the reviewer, in bold fonts
Our response, in plain fonts
Corresponding changes made to the manuscript, in blue

I-   Response to Anonymous referee 1

The paper is very well written and structured; conclusions are supported by meaningful figures presenting results from 8 well selected simulations. The results are very useful to the ocean modeling community, not only but in particular too the NEMO community. I recommend publication of the manuscript in GMD after minor revisions of text and figures. In this respect I make some suggestions below.

We thank the reviewer for their constructive comments and encouragements. We have modified the text and the figures (including the supplementary one) as they and the other reviewer suggested. Please note that the work of the reviewers is acknowledged at the end of the manuscript.
Referee 1 will find a point by point response (plain font) to their comments (indicated in bold font) below, with the associated text modification in blue.

Some general comments:

I agree that open ocean deep convection to the extent as it occurs in many ocean and coupled climate model simulations is not supported by any observations. In this respect it is valid to speak of "spurious" deep convection, which modelers seek to avoid in future experiments. However, I would like to stress that there ought to be a sweet spot for the choice of model parameterizations and parameter settings at which deep convection is suppressed most of the time but still allows for a Weddell Polynya-like event—the only observed occurrence of Southern Ocean deep convection—to form under conditions resembling those of the mid 1970s in the Weddell Sea. I know, a topographic feature such as Maud Rise is possibly key to the preconditioning of the ocean for forming a large open ocean polynya (Holland, 2001, Science) and such feature may not be resolved in the model bathymetry. But a cautionary note in Section 4 not to push the vertical

mixing too hard would be a helpful reminder that in principle open ocean deep convection can occur in reality.

We agree with the reviewer that models should be allowed to reproduce the actual Weddell Polynya and any potential future new polynya. Holland (2001) does suggest a key role for Maud Rise as a feature where eddies are trapped. It would be interesting to perform an experiment modifying the bathymetry so that Maud Rise is removed, but this is beyond the scope of this paper. It is intriguing that the large Weddell Sea deep convection events in our experiments, but also in other models (personal communication with Paul Holland and Dan Jones), is triggered by a smaller event in the Riiser-Larsen Sea. It is not clear what is key in this sea, e.g. the proximity with the eastern limb of the Weddell Gyre? Are there specific sea ice conditions – or is there a weakness in the sea ice model? Is the bathymetry of the coastline trapping anomalies?
We have added two notes in Section 4 to summarise these remaining questions and the reviewer's comment:
"Note that other ocean models have a similar chain of events and timing, with deep convection in a small area in the Riiser-Larsen Sea triggering a larger event in the central Weddell Sea. The reason why the Riiser-Larsen Sea is so prone to deep convection in ocean models remains an open question to be investigated."
"current state-of-the-art models require Southern Ocean deep convection in order to form their Antarctic Bottom Water (Heuzé et al., 2013). Ideally, they should also represent the actual Weddell Polynya. Hence, open ocean deep convection should be reduced but not totally suppressed. New methods"

Further, I miss some discussion of how the tested parameter changes reflect settings in other global ocean models than NEMO. Are the results particularly NEMO dependent? Why or why not?
We cannot tell what the settings of other models are, for we do not have access to their detailed parameter list. The near-inertial wave breaking (γ), as it is implemented in NEMO, is a very ad-hoc parameterisation that we do not expect to see in other models. We could not find any study linking the Langmuir turbulence parameterisation or background diffusivity in other models and their impact on our issue.
However, these three parameters are not the main focus of our paper. They are studied here because Calvert and Siddorn (2013) found them to have the larger impact on vertical mixing in the Southern Ocean, which is what we are interested in.
We have added the following sentence in the conclusions:
"Our conclusion is that a similar result can be achieved within the parameterisation of an existing, globally consistent, mixing scheme. Although the parameters changed here are specific to NEMO, increasing vertical mixing in the Southern Ocean in other models is likely to lead to reduced Southern Ocean deep convection, for the mechanism would be unchanged."

I also think that the recommendations at the end of the paper could be a bit more specific. Although specific parameter values may be model dependent, the authors could indicate a preference for the three parameterizations discussed.
The authors think that answering this question is beyond the scope of this paper. Having run only two experiments per parameter, one increasing and the other decreasing the value, we cannot comment on the best value but only on the direction of the change that is needed to reduce Southern Ocean deep convection. Our recommendations more generally relate to levels of general

mixing in models, rather than to the exact value that needs to be attributed to each parameter. We have modified the final two sentences of the conclusions to reflect this point:

"By performing a set of vertical mixing sensitivity experiments on the NEMO model, we have shown the general direction that models need to take to at least reduce spurious Southern Ocean deep convection: the vertical mixing needs to be increased, not decreased as one might intuitively think. This paper paves the way for further model improvement that could help ocean models to not form their bottom waters unrealistically"

In the following I suggest a few improvements and corrections by page and line:

page 2951
24 remove ", maybe counter intuitively,"
The sentence has been modified as suggested

page 2952 23 please add a sentence describing the "Control" simulation separate from all other experiments. How does it differ from the mentioned GO5 configuration? Or is "Control" an existing "GO5" run? Why is the Control run only 10 years (Table 1)?
"Control" is an existing GO5 run, as is now specified in the methods:
"which provide the base settings used in our experiments. Our "Control" experiment is the GO5 run "amhih" (see Code availability at the end of this manuscript). Following their findings as well...".
Only 10 years of the Control run are presented because it stopped running unexpectedly due to issues with the super computer and could not be restarted.

25 ". . . diffusivity experiments extend throughout the entire period of available CORE2 atmospheric forcing ..."
Sentence now reads "The background diffusivity experiments extend throughout the entire period of available CORE2 atmospheric forcing (27 years, 1980 to 2006)"

page 2953
13 Are there any newer or additional observations also from regions outside the Baltic Sea that confirm or extend the range suggested by Axell?
Although there have been publications on the overall topic of Langmuir turbulence in ocean models, to the best of our knowledge none answers the reviewer's question on the range for c_{LC}. We have added a comment about new observations that could confirm or extend this range in the conclusions:
"Our results, however, need to be treated with some caution […]. Further observations would also be beneficial to validate or extend the range of parameter values used in this study, notably LangmuirI."

18 Table 1 states that c_LC is in fact not set to zero but the whole parameterization was turned off. This should be stated somewhere near this line to match the actual parameter setting in Table 1. In the list itself c_LC = 0 illustrates well the difference to the other experiments.
The reviewer is right in pointing the discrepancy between the table and the text. The table is correct: the whole parameterisation is turned off. This has been corrected in the text:
"Here we test three cases:

- Langmuir turbulence parameterisation turned off ("LangmuirD")

- c_{LC} = 0.15..."

Page 2954

21 please add, which profile type Megann et al. used: constant or linearly increasing

Text now reads "Megann et al. (2014) increased the background diffusivity (constant through depth) from..."

page 2955

14,15 add "increase", i.e. "linear increase profile"

The two lines have been modified as suggested

24-26 "We compute the total area of deep convection as the sum of individual model grid cell areas where . . ."

Sentence now reads "We compute the total area of deep convection as the sum of individual model grid cell areas where the MLD exceeds 2000 m."

page 2956

3,4 "Likewise, we compute the total polynya area as the sum of the areas of connected model grid cells where . . ."

Sentence now reads "Likewise, we compute the total polynya area as the sum of the areas of connected model grid cells where the criteria for a polynya are met"

6,7 ". . . 1984), before the onset of deep convection.", 'initiated' has a flavor of artificially introduced to the run.

The sentence has been modified as suggested.

8 please spell out SD. I think it is only used twice in the entire manuscript (see also caption of Fig. 3)

The text and the caption of Fig 3. have been modified (see also later comment on Fig. 3)

9 ". . . deep convection on the large-scale circulation . . ."

The sentence has been modified as suggested.

16 Why do you use two different thresholds to diagnose deep convection, 2000 m for the southern and 1000 m for the northern hemisphere. Would 1000 m also work for the Southern Ocean? If not, why?

We base both diagnostics on the published literature, which suggests that each threshold is the most suitable for each region. In the southern subpolar gyres, unpublished work by the lead author for her doctoral thesis (Heuzé, 2015) showed that 1000 m was an unsatisfactory threshold – to reach full-depth convection, the water needs to descend at least half way through the water column in regions where the bathymetry exceeds 3000 m (see the grey contours on Figs 1 and 2). In the Arctic in contrast, 2000 m is too deep a threshold, for the water tends not to descend vertically but rather spill and spread horizontally.

4

page 2957
4 the time axis of Fig. 2c is too short to support the statement of a three month lasting warming phase. See comment on Fig. 2 below.
We have extended the time axis of Fig. 2c (now Fig 2f) from January 1985 to December 1987 as the reviewer suggested. See also response to comment on Fig. 2 below.

10 ". . . Fig. 2e) north of the 1987 polynya."
Sentence now reads "in September 1986, Fig. 2c), north of the 1987 polynya."

14,15 How do you differentiate between "convection" and "deep convection"? This should be clarified, maybe already in the introduction. Why does an event that pushes the mixed layer to 800 m (line 13) not qualify as "deep convection"?
We define as "deep convection" events that are deeper than the climatological observed MLD and could be called "near-full depth convection". We have made the distinction clearer at the beginning of the introduction:
"Full depth open ocean deep convection has been observed only once in the Southern Ocean, following the Weddell Polynya of 1974-1976 (Gordon, 1978). Otherwise, the mixed layer depth in the southern subpolar gyres does not exceed 1000 m (Schmidtko et al., 2013). In state-of-the-art CMIP5 models, Antarctic Bottom Water (AABW) is formed via open ocean deep convection (deeper than 2000 m and up to full-depth)"

21 ". . . (Fig. 2f). This polynya reopens further south than the one in 1986."
The sentence has been modified as suggested

26 "low stratified", do you mean "weakly stratified"?
Yes, we do. Sentence now reads: "The conjunction of weakly stratified waters"

page 2958
5,6 all your simulations start in 1980, which means that the mentioned spin up extends over the period 1980-1984 and thus these years are not a very good reference period. I understand that you do not have the option of a longer spin up because of the applied forcing. I suggest to simply drop the sentence "The parameter changes . . . of the simulation." here. Spin up issues can be and are already partly addressed in Section 4—add a note there.
We agree with the reviewer that the fact that our spin-up period and reference period coincide presents some issues, but addressing them would be beyond the scope of this paper.
As suggested, we have removed the sentence "The parameter changes will consequently spin up a new ocean state over the first years of the simulation."
We have added the following to the conclusions:
"(i.e. a spun-up model) changes to the mean state of the global ocean simulation. [...] Longer simulations are also needed to obtain a more appropriate reference period (i.e. after the model has spun up)."

13,14 " . . . sea surface temperature (SST), as the area is ice-covered and SST at the freezing point during 1980-1984 in all experiments."
Sentence now reads "There is no significant difference in the winter sea surface temperature (SST), as the area is ice-covered and SST at the freezing point during 1980-1984 in all experiments."

20 delete "the fixed anomaly in"

As suggested, sentence now reads "at the same location, constrained by the atmospheric forcing."

page 2959

5 "summer and autumn 1986"

Sentence now reads "The warmer the ocean is in summer and autumn 1986"

5-12 this paragraph and Figure 3 is a really nice demonstration of the sequence of events!

We thank the reviewer for this particular comment!

page 2960

14 judging from Figures 4 and 5 KprofI, which is one of the 27-year runs, has no continued deep convection. However, taking your Figure S1 into account, I wonder whether the plots ending in 1989 actually demonstrate the full effect of the altered parameter settings. Further, I don't see a strong correlation between ACC strength and deep convection area in Fig. S1; ACC strength seems to be highly correlated between all three cases whereas the convection area evolves very differently ('no prof' and 'prof' cases). I have the impression that there must be—in addition to deep convection effects another major driver of ACC acceleration in your simulation.

Following for example Russell et al. (2006) and Meijers et al. (2012) who looked at the ACC in CMIP3 and CMIP5 models respectively, we have added to Fig. S1 (now Fig. 5) the timeseries of the mean annual meridional density gradient across the ACC. See in methods:

"Following for example Russell et al. (2006), we compare the ACC strength to the horizontal gradient in density across the ACC dρ, defined as the zonally averaged density difference between 45°S and 65°S."

We find high significant correlations between this gradient and the ACC volume transport, as was to be expected, as well as with the area of deep convection: deep convection is associated with an increase in density of the whole water column in the subpolar gyre, hence an increase in the horizontal gradient in density and in the ACC transport.

However, we agree with the reviewer that the ACC in the Kprof and KprofI experiments must also be accelerated by some other mechanism, as both show an increase in the ACC around 1995 when the density gradient decreases and there is no deep convection. However, we could not identify this mechanism.

19-21 It is in fact the shutdown of deep convection that leads to enhanced Westerlies because sea level pressure over Antarctica and the Southern Ocean is generally lower during years without deep convection (Latif et al., 2013)—except for the deep convection region itself where locally a low pressure anomaly forms over the polynya. However, this local feature is overruled by the large-scale SLP change, which means a decrease in the meridional sea level pressure gradient and weakening of the Westerlies during years with deep convection. Acceleration of the ACC as a result of the deep convection in comparatively coarse resolution models seems rather driven by the steepening of the meridional sea surface height and density gradients in the ocean due to the heat loss south of the ACC as pointed out by Martin et al. (2015). —Martin, T., W. Park, and M. Latif, Southern Ocean Forcing of the North Atlantic at Multicentennial Time Scales in the Kiel Climate Model, Deep Sea Res. Part II, 114, 39–48, doi:10.1016/j.dsr2.2014.01.018.

We thank the reviewer for taking the time to explain this mechanism that we had clearly

misunderstood. Unfortunately, we did not extract the sea surface height output during our simulation, but our results on density gradients (see previous comment) agree well with those of the reference mentioned here. There is an interesting discrepancy in the timing though, for their changes in ACC happen five years after their changes in SSH, while our changes in ACC are best correlated with changes in density gradient of the same month. Again, assessing whether it is the different resolution (2° for them, 1/4° for us), the fact that they are running a coupled model, or something else that is responsible for this difference would be beyond the scope of this paper.

We have removed the incorrect sentence "In a fully coupled climate model, where the large heat flux to the atmosphere by the polynya leads to reduced sea surface pressure and increased westerlies (Martin et al., 2013), the ACC would probably become even stronger."

We have also added the following sentence – note that we now include that figure in the core of the manuscript, rather than as a supplementary figure:

"In agreement with other modelling studies (e.g. Russell et al., 2006 ; Meijers et al., 2012 ; Martin et al., 2015), deep convection in the Weddell Sea is associated with an increase in the horizontal density gradient, a key driver of the ACC strength (Fig. 5)."

26 "While open ocean deep convection in the . . ."
The sentence has been changed as suggested.

page 2961
14 Some information on how MLD is or is not affected globally (not only Southern Ocean and northern North Atlantic) would be nice, such as global mean and SD of differences to the control run. Maybe you can refer to Calvert and Siddorn (2013)? Some of this is mentioned in Section 3.2 but could be repeated and extended here, in particular since Calvert and Siddorn's work is published "only" as a technical report.
Following the reviewer's suggestion, we have added a brief comparison of the global biases in MLD among the simulations:

"Our results suggest that at least up to a decade, deep convection in the North Atlantic is not significantly modified by the three vertical mixing parameters found to have a large impact on the Southern Ocean deep convection. We obtain similar results when looking at the rest of the world ocean: outside of the deep convection regions (i.e. 60°S to 50°N), the year-long area-weighted mean difference in MLD between observations and all our simulations ranges between 4.8 m (GammaI) and 6.5 m (GammaD), and no clear regional patterns can be detected. That is, outside of the southern subpolar gyres, the MLD is not significantly modified by our changes of parameters. It thus seems feasible..."

The following sentence was added to the methods section as well:

"The observed global MLD was obtained from the climatology of Schmidtko et al. (2013)."

18,19 ". . . which preconditions the ocean, initiates open ocean deep convection . . . in winter 1987 in our simulations. It begins . . ." Please do not make these sentences sound as something that really happened in the 1980s.

We apologise for the confusion. The sentence now reads:

"A complex chain of events, which preconditioned the ocean, initiated open ocean deep convection in the Riiser-Larsen Sea in winter 1987 in our simulations."

page 2962
28 ". . . in longer term changes to the mean state of the ocean simulation." 'damage' is an ugly

word that you want to avoid, I think.

We thought that "damage" would made it clear that it is a state that needs to be avoided, but we have changed the sentence as the reviewer suggested.

28 Moreover, your supplementary Figure S1 pretty convincingly demonstrates that at least the runs with altered background diffusivity show some significant non-linear response in deep convection and thus also likely in large-scale circulation. So, I would argue that in longer simulations you would likely find a different mean state of the model ocean using different vertical mixing parameters. The key is to find the balance between a low-bias global ocean and a Southern Ocean without deep convection.

We agree with the reviewer. This is the idea that we wanted to convey by using the word "damage": the increase in vertical mixing will never be implemented if it results for example in an incorrect representation of ENSO, too biased global SSTs, a weak AMOC, and other standard diagnostics used to assess global ocean models, even if it drastically reduces Southern Ocean deep convection. We have added the reviewer's comment to that line:

"changes to the mean state of the global ocean simulation. Ocean modellers would then need to find the balance between a low-bias global ocean, and a Southern Ocean with reduced deep convection."

page 2963 9 ". . . as one might intuitively think."

Sentence now reads "not decreased as one might intuitively think."

References

You may want to reference the corrigendum to Timmermann and Beckmann (2004) by Timmermann and Losch (2004) as well. Also, Martin et al. (2012) was published in 2013, not 2012.

We have added the reference to Timmermann and Losch (2004), as well as the references to Russell et al. (2006), Meijers et al. (2012), Schmidtko et al. (2013) and Martin et al. (2015) mentioned above. We have corrected the date of the reference to Martin et al. (2013).

Table 1 I suggest removing the row for 'Control' since it is represented in column 3. The 10- year duration of Control can be mentioned in the caption or in an additional sentence on Control on page 2952. Further, I suggest to switch columns 3 and 4 and to change column titles to "value" and "value in Control".

We have modified the table and its caption as the reviewer suggested:

Table 1. Sensitivity experiments performed on NEMO, detailed in section 2. "Langmuir" experiments look at Langmuir turbulence velocity scale, "Gamma" at the penetration of an additional turbulent kinetic energy term below the mixed layer, "Knoprof" and "KProf" at background diffusivity. "I" indicates that the parameter was increased compared to the reference value, "D" that it was decreased. The parameters column identifies the shorthand name used in the NEMO simulation name-list. Note that the "Control" run is 10 years long.

name	parameter	value	value in "Control"	run length
LangmuirD	ln_lc	false	true	10 yr
LangmuirI	rn_lc	0.20	0.15	10 yr
GammaD	rn_efr	0.005	0.05	10 yr
GammaI	rn_efr	0.095	0.005	10 yr
KnoProfD	rn_avt0	1.0e-5	1.2e-5	27 yr
KProf	nn_avb	1	0	27 yr
KProfI	nn_avb	1	0	27 yr
	rn_avt0	1.3e-5	1.2e-5	

Figures

Figure 1: The blue box is barely visible as a closed box being overlayed by the green dashed line. Can you shift lines so that the blue box appears just within the green dashed box? I was surprised not to find the convection event of 1986 in this figure until I noticed that an MLD of close to 900 m only shows as pale yellow. Please change the color scale, maybe limiting it to 3000 m is enough, to let the 1986 event appear more clearly.

We have slightly shifted the boxes and changed the colour of the green one to magenta to make it more distinguishable from the blue box. We have changed the colour scale to saturate at 2000 m (the threshold used in our study) so that the 1986 event appears in a shade of orange:
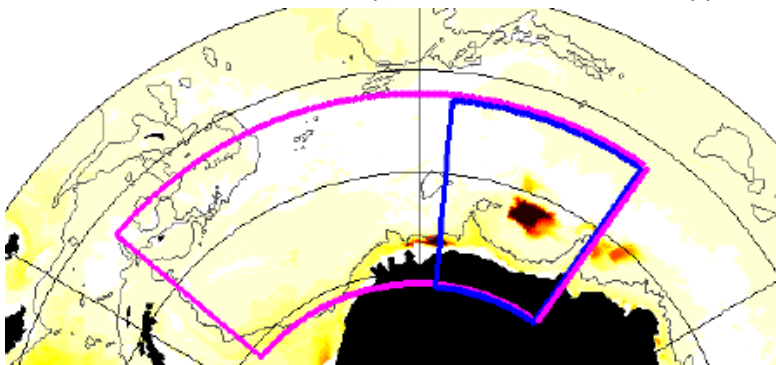

Figure 2: Please add information to the caption on the reference period used to calculate the anomalies. Please extend the time axis of panels c) and d) to at least June 1985 to October 1987, even better would be January 1985 to December 1987. Instead of grid lines for every "pixel" in panels c) and d) I suggest to add solid and dashed vertical lines for September (winter) and March (summer) respectively. Also, it would be nice to indicate mean or maximum mixed layer depth in panels c) and d), which represent the deep convection region only. In the current presentation mixed layer warming in the summer months could be easily misinterpreted as the warming related

to convection and the effect of active deep convection is barely visible at the very right boundary of panels c) and d).

This sentence has been added to the caption:

"potential temperature anomalies (relative to Jan. 1980 to Dec. 1984)"

We have extended the time axis of c) and d) (now f and g) from January 1985 to December 1987.

We have removed the grids and added solid vertical black lines for September and dashed vertical black line for March as suggested.

We now indicate the local MLD in grey on the anomaly plots and white on the actual property plots.

Note that the figures have also been changed following a major comment from reviewer 2: we now follow the anomalies as they travel from the site of the 1986 polynya to that of 1987 (see trajectory on Fig. 2e).
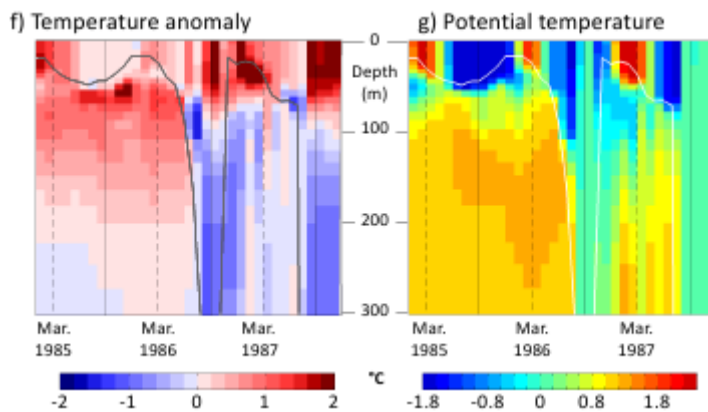

Figure 3: Last sentence of caption: "Horizontal (panels a and d) and vertical bars (panel c) on symbols indicate the standard deviation related to spatial variability."

Sentence now reads: "Horizontal bars on a) and d) and vertical bars on c) indicate the standard deviation relative to spatial variability."

II-   Response to Anonymous referee 2

We would like to thank the reviewer for their comments and suggestion, and in particular for pointing out where our argument had been weakened in our desire to keep the paper short. We hope that they find this version more convincing. Please note that the work of the reviewers is acknowledged at the end of the manuscript.

Referee 2 will find a point by point response (plain font) to their comments (indicated in bold font) below, with the associated text modification in blue.

Major Comments

The conclusion that increasing vertical mixing helps to suppress the spurious development of polynyas is interesting but I am not convinced by the explanation. Warmer surface waters are not of themselves sufficient to permit the development of deep convection—in the absence of deep convection winter cooling will simply eventually cool these waters to freezing point. Somehow the near-surface waters need to salinify (or the waters below must be freshened and/or warmed) in order to reduce the stratification.

We agree with the reviewer that the surface water salinification (via brine rejection when the polynya is open and via lack of ice to melt the following spring) is an important process in the mechanism we describe. We now discuss it more in section 3.1 (see next comment). Please note that the mechanisms leading to the opening of polynyas in the UK ocean models, and in particular in NEMO, are discussed in more details in the doctoral thesis of the lead author, hereafter referred to as Heuzé (2015).

The mechanisms section 3.1 needs to be considerably strengthened, with proper discussion of the evolution of the salinity and density (referenced to the appropriate depth) as well as the temperature.

Following the reviewer's suggestion, Fig. 2 now also presents the anomalies in density σ_θ and the density itself on the panels h) and i). We chose the density σ_θ for we are mostly working with the top 300 m of the water column only, and for consistency with the density used by the model to calculate the mixed layer depth. The following sentence was added to the methods section:

"We compute the potential density (hereafter referred as density only) relative to the surface σ_θ using the equation of state EOS80 (Fofonoff and Millard, 1983)."

In order to keep the manuscript relatively short, we decided not to show salinity and instead show density. Salinity can then be deduced from the temperature and density. Regarding salinity and density, the following sentences have been added in section 3.1:

"the model polynya allows the formation of dense water at the surface (Fig. 2h and i) due to brine rejection, destabilising the water column"

"preconditioning the ocean for deep convection. In October 1986, the isopycnals are vertical (Fig. 2i) and the surface waters are anomalously dense because of the brine rejection in the polynya (Fig. 2h). Moreover, [...] Meanwhile, not only are the surface waters anomalously saline and dense because of brine rejection in winter, they also remain anomalously saline and dense through spring and summer (Fig. 2h), as no ice is to be melted at the location of the polynya. As the surface waters..."

The Hovmöller diagrams in Fig 2c and 2d need to take account of the movement of the fluid column: the fields in 1985 and 1986 should be plotted further northward e.g. (presumably) at the site of the 86 polynya in Sept 1986. More generally, there needs also to be proper discussion of the

11

advective effects: e.g. is there a rotation of the velocity vector with depth that is causing the stratification to evolve?

The reviewer raises an important point that had to be corrected in our methodology. Rather than showing the Hovmöller diagrams at a specific location, we now follow the waters as they travel from the site of the 1986 polynya to the 1987 one (see trajectory on Fig. 2e). This method proved easier than expected to implement as there is little shear between the surface and the depths of the anomalies. We have added the following sentence:

"This polynya reopens further south than the one in 1986. That is because the warm and dense anomalies have been advected in a barotropic subsurface flow that brought them to the site of the 1987 polynya (Fig. 2e). Also,..."

The method to infer the trajectory of the water is detailed in section 2.2:

"To account for the advection of the anomalies by the local currents (which have little vertical shear for the depth range studied and exhibit temporal variability, not shown), we define the trajectory between the first two polynyas (1986 and 1987) as the succession of monthly positions occupied by the water that was in the polynya in September 1986, inferred from the horizontal velocity vectors."

Detailed Comments

p2950, l 3-4 triggering mechanisms leading to ⇒ mechanisms triggering

Sentence now reads: "To identify the mechanisms triggering Southern Ocean"

p2950, l 14 are ⇒ give

Sentence now reads "The experiments with decreased mixing give warmer surface waters,"

p2953, Eq. (1). What is Vs? More generally, how does the W impact on the TKE in Axell's parameterization?

We have added equation (1) relating W and TKE, as well as more explanation about the notation of equation (2), in particular the Stokes drift Vs:

Langmuir turbulence is represented in NEMO by the parameterization of Axell (2002), which appears as an additional production term in the TKE budget equation:

$$\frac{d\bar{e}}{dt} = \frac{W^3}{L},$$ (1)

where W is a velocity scale, taken to be the maximum downwelling velocity of the Langmuir cell and L is a length scale, taken to be the vertical extent of the cell. A sinusoidal profile is assumed for the cell so that:

$$W = cV_{10} = c_{LC} V_s|_{z=0} \sin(-\frac{\Pi z}{L}) \quad \text{for} -z \leq L,$$
$$W = 0 \quad \text{for} -z > L,$$ (2)

where $V_s|_{z=0} = 0.016 V_{10}$ is the surface value of the Stokes drift for a fully-developed sea (Li and Garrett, 1993), V_{10} is the 10 m wind speed and c_{LC} is a scaling coefficient, suggested by Axell (2002) to be between 0.15 and 0.2 based on a comparison with Large Eddy Simulation results.

p2954, Eq. (2). Is this correct? As written it seems that an amount of energy $e_{inertial}$ is added each time step. This would mean that the shorter the time step, the more rapidly energy is added, which makes no sense.

We thank the reviewer for bringing our attention to this equation, there was a small typo in the integral term. The addition of $e_{inertial}$ however is correct. As Rodgers et al. (2014) (whose paper this equation comes from) wrote, it is an "ad-hoc parameterization of wind stirring".

p2957, l5-6. 'When ice-free in summer, the warm waters are incorporated in the mixed layer'. Presumably the summer ML is shallower than the winter ML, so how does this happen? Upwelling?

In fact, the summer ML is locally deeper than the winter ML: the reader must remember that the winter ML is anomalously (very) shallow. To facilitate the reading, we now give the following values: "which is no longer anomalously shallow (deepening from 48 m in January 1986 to 120 m in May)."

p2957, l8. 'The warm surface waters impede sea ice formation, resulting in the development of an open ocean polynya over August to October 1986'. I can't see any warm anomaly in T in Fig. 2c until Feb 87.

Thanks to the reviewer's suggestion of following the anomalies as they are being advected, the warm anomalies in temperature are far easier to see now on Fig 2c (now numbered Fig. 2f).
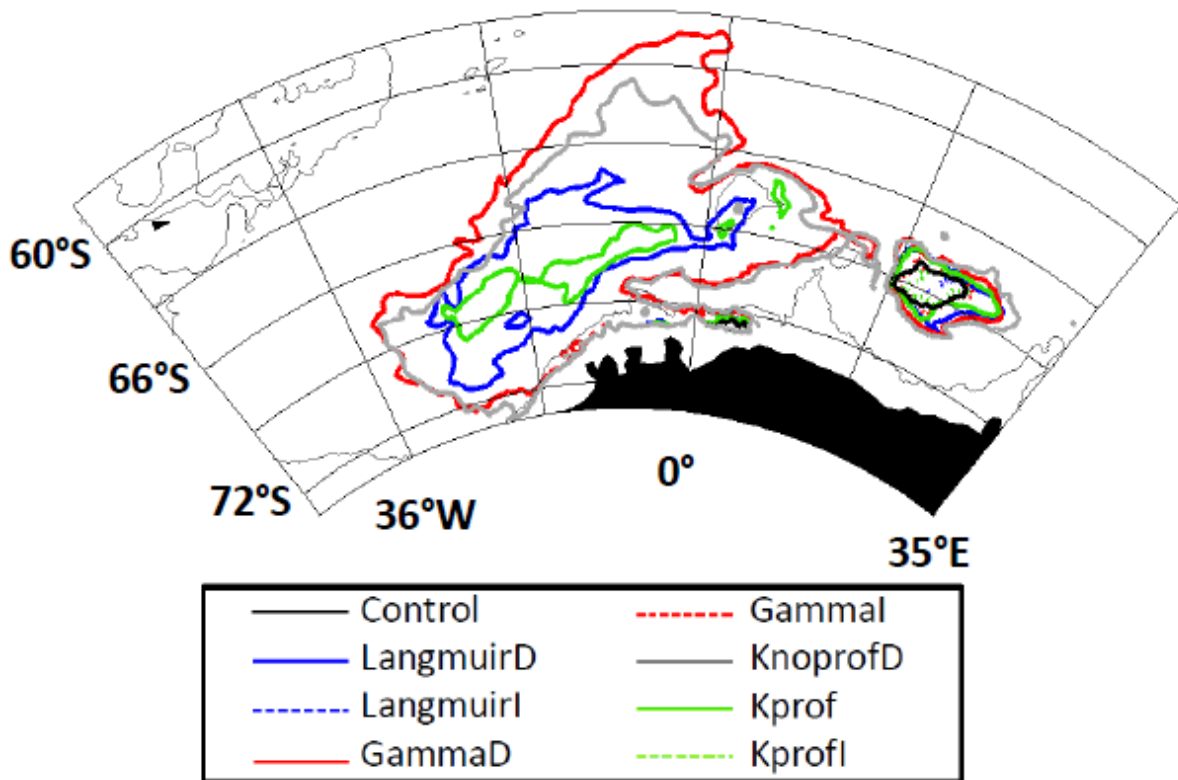
p2957, l22 'similar to observations'. I'm confused. Are you saying a polynya in 1986 is realistic? If so more discussion of the observations would be useful. However, on p2950, l 23 you state that full depth open ocean deep convection only occurred in the 70's

We apologise for the confusion. We only meant that the 1986 polynya behaved like real-world polynyas do in impacting the ocean over a large area. We have rephrased the sentence:
"Also, like real polynyas do (Smith and Barber, 2007), the 1986 model polynya and"

p2971, Fig 4. Yellow lines are hard to see

We have changed the yellow lines to dark green ones to improve the readability:

p2972, Fig. 5a. I can only see 4 lines; Fig. 5b. Again yellow line is hard to see.

Fig. 5a (renumbered 6a) now features a "zoom" over 1986-1988 where the lines were very close to each other. Again, the yellow lines have been change to green on both panels (see Fig5a here):