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Interactive comment on "Increasing vertical mixing to reduce Southern Ocean deep convection in NEMO" *by* C. Heuzé et al.

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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 *italics* font.

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 explana-

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tion. Warmer surface waters are not of themselves sufficient to permit the development of deep convectionâĂŤin 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 $\sigma\theta$ and the density itself on the panels h) and i). We chose the density $\sigma\theta$ 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 $\sigma\theta$ using the equation of state EOS80 (Fofonoff and Millard, 1983).*"

In order to keep the manuscript relatively short, we decided to discuss salinity but not show it, and instead show density. Salinity can then be deduced from the temperature and density. Regarding salinity and density, some of the sentences added to section 3.1 and 2 are the following:

"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 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 succes-*

sion 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:

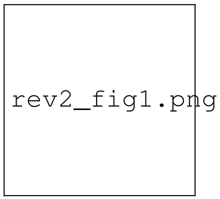

Fig. 1. changes made to Eq. (1) - now renamed Eq. (2)

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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:

rev2_fig2.png

Fig. 2. changes made to Fig. 4

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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 Fig. 6a here):

rev2_fig3.png

Fig. 3. changes made to Fig. 5 - now renamed Fig. 6

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

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

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:

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

where $V_s|_{z=0} = 0.016V_{10}$ 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.

Fig. 4.

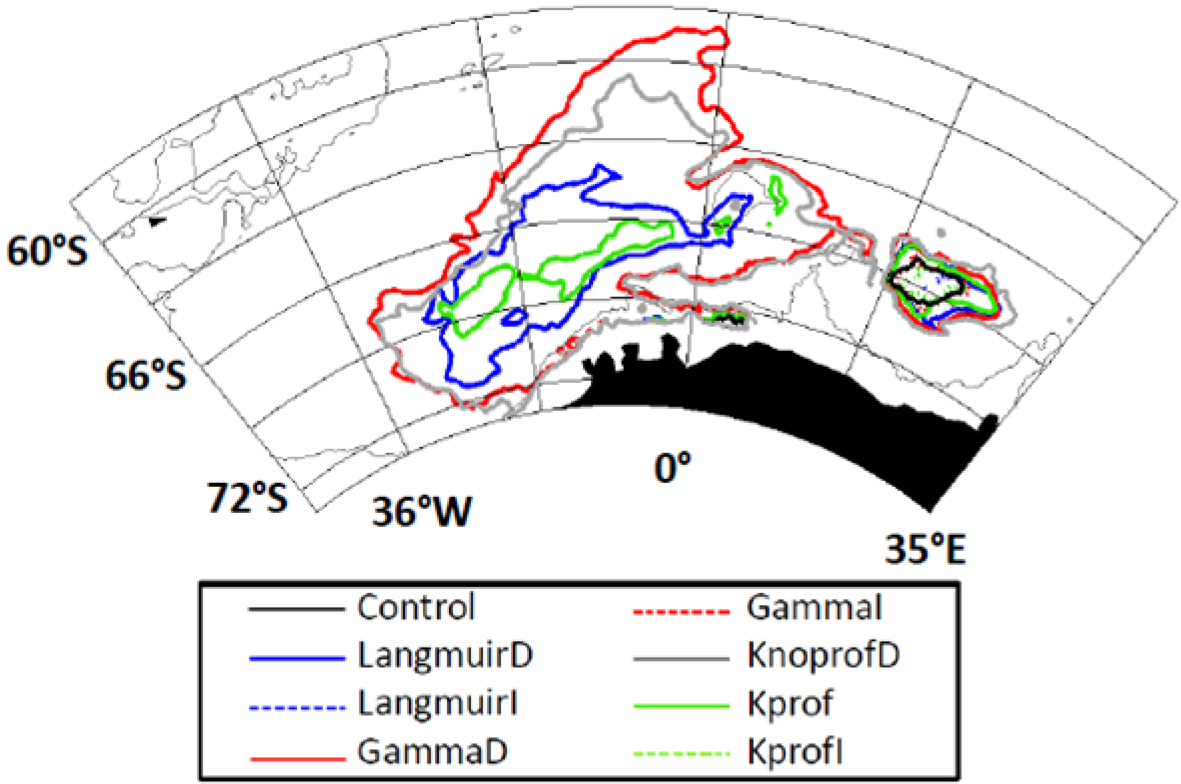

Fig. 5.

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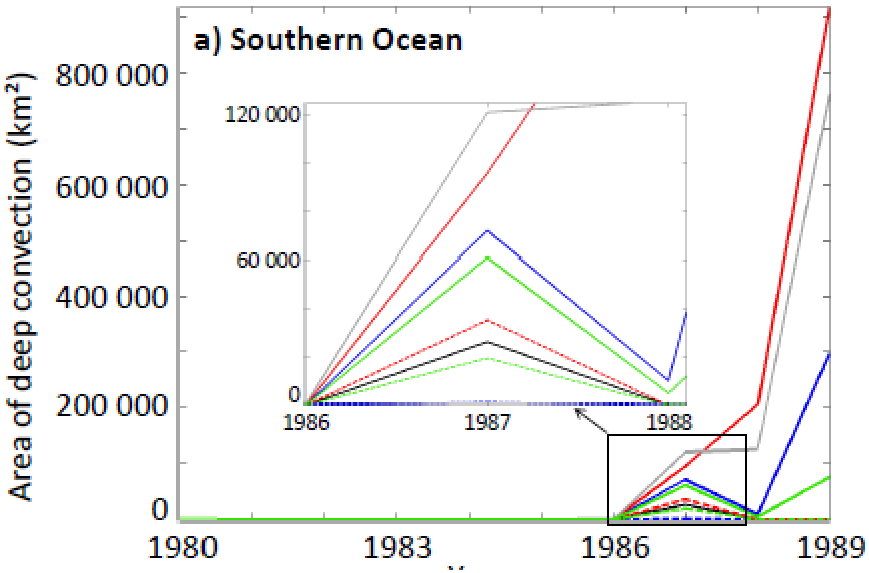

Fig. 6.