Geosci. Model Dev. Discuss., 8, C827–C834, 2015
www.geosci-model-dev-discuss.net/8/C827/2015/

Geoscientific Model Development Discussions
Open Access

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

Anonymous Referee #1

Received and published: 20 May 2015

In "Increasing vertical mixing to reduce Southern Ocean deep convection in NEMO" Heuzé, Ridley, Calvert, Stevens, and Heywood investigate the possibility of controlling the occurrence of open ocean deep convection events in the Southern Ocean in a global ocean model simulation by changing parameter settings in vertical mixing schemes. In particular, they look into three parameterizations of Langmuir turbulence, near-inertial wave breaking, and background diffusivity. The authors conclude that enhanced vertical mixing can prevent the model from creating spurious deep convection events. The associated parameters can be changed without affecting deep convection in the North Atlantic in a global model simualtion, at least within the 10-year study period chosen by the authors.

The paper is very well written and structured; conclusions are supported by meaningful

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

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.

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.

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?

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.

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

page 2951

GMDD

8, C827–C834, 2015

24 remove “, maybe counter intuitively,”

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)?

25 “. . . diffusivity experiments extend throughout the entire period of available CORE2 atmospheric forcing ...”

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?

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.

Page 2954

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

page 2955

14,15 add “increase”, i.e. “linear increase profile”

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

page 2956

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

GMDD

8, C827–C834, 2015

6,7 “. . . 1984), before the onset of deep convection.”, ‘initiated’ has a flavor of artificially introduced to the run.

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

9 “. . . deep convection on the large-scale circulation . . .”

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?

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.

10 “. . . Fig. 2e) 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”?

21 “. . . (Fig. 2f). This polynya reopens further south than the one in 1986.”

26 “low stratified”, do you mean “weakly stratified”?

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.

GMDD

8, C827–C834, 2015

13,14 " . . . 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"

page 2959

5 "summer and autumn 1986"

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

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 effectsâĂŤanother major driver of ACC acceleration in your simulation.

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). —

GMDD

8, C827–C834, 2015

Martin, T., W. Park, and M. Latif, Southern Ocean Forcing of the North Atlantic at Multi-centennial Time Scales in the Kiel Climate Model, Deep Sea Res. Part II, 114, 39–48, doi:10.1016/j.dsr2.2014.01.018.

26 “While open ocean deep convection in the . . .”

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.

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.

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.

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.

page 2963

9 “. . . as one might intuitively think.”

References

GMDD

8, C827–C834, 2015

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.

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".

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.

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).

Figure 3: Last sentence of caption: "Horizontal (panels a and d) and vertical bars

GMDD

8, C827–C834, 2015

(panel c) on symbols indicate the standard deviation related to spatial variability."

Interactive comment on Geosci. Model Dev. Discuss., 8, 2949, 2015.

GMDD

8, C827–C834, 2015