Dear Editor and Reviewer,

We would like to thank you for accurately reading and commenting the manuscript, and suggesting how to improve it. Answers to your comments are given in details hereafter. We hope that you will find them satisfactory. All authors agree with the modifications made to the manuscript.
Reviewer comments are in bold, and are followed by our response (in blue) that includes changes and/or additions to the text.

For the authors,
Dorotea Iovino

Answer to **Referee #2**

This manuscript outlines the first results from a new global 1/16° implementation (GLOB16) of the NEMO-LIM ocean-sea ice model. The manuscript outlines some key metrics from the model and compares some metrics to a lower resolution implementations of the same model (GLOB4).
The model described in this manuscript is close to the leading edge of global ocean-sea ice models. It's important to document these models as they develop, and thus there are good reasons for GMD to want to publish this paper. However, there are a number of areas in which the manuscript could be improved.
My primary query is whether this manuscript is here simply to document the existence of a viable model (that is, the model works and is sufficient) or whether the aim is to make the case that the model is an improvement over previous, lower resolution versions. I strongly recommend following the latter path, but I found on reading the paper that the case for the GLOB16 model being an improvement on GLOB4 was somewhat tenuous. For many metrics the GLOB4 results were not shown, and in some areas GLOB4 looked slightly better! If one is to justify the move towards eddy-resolving models then a stronger case that the additional computational expense is worthwhile must be built. (Alternatively, perhaps the conclusion may be that eddy-resolving is not worth the expense until models improve!)

The main objective of this study is to present a new global eddying configuration, to evaluate the first 10-year simulation and, not least, to show the overall improvements of the model solution due to the increasing resolution. For this aim, together with the validation of GLOB16 against observations, we presented a comparison between GLOB16 and the eddy-permitting configuration, GLOB4. As point out by the Referee, this study is certainly enriched by a more detailed analysis of the differences between the two models. We added text and modified plots in any subsection on "model validation" to emphasize the key role played by the ocean resolution.
GLOB16 simulation is by no means perfect, with notable discrepancies with observations in some areas, but we believe that our analysis demonstrates that most aspects of the GLOB16 circulation are more realistic with respect to GLOB4, and the eddying models is able to better represent the upper ocean dynamics and ocean variability at mid- and low latitudes. In particular, our analysis shows that eddying resolution improves the temperature and salinity biases at upper and intermediate depths, the extension and separation of western boundary currents, the strength of the overturning circulation as well as the poleward heat transport in the Atlantic Ocean, the volume transports through most of the considered critical passages, and the narrow boundary currents and flow over narrow sills. Tables 1 and 2 summarize the changes in volume transports.
The biggest caveat of this GLOB16 simulation is the representation of the mesoscale variability. The model has too weak eddy energy compared to observations, and the expected increase in eddy kinetic energy (EKE) due to resolution is not evident. In order to identify the source of this problem, we are analysing this aspect in detail performing a set of short (2-year) sensitivity experiments.

There are more details on these issues in the following list of suggested improvements that the authors may want to consider:

1. The use of acronyms (e.g. NEMO, CMCC) should be avoided in the abstract. In fact, CMCC is never defined in the text of the paper, and it seems unnecessary to list the affiliation of authors within the manuscript.
We deleted the CMCC acronyms in the abstract, substituting it with "Euro-Mediterranean Center on Climate Change". We do think that the acronym NEMO is well known in the ocean modelling community, and as suggested by the editor, we included it in the title and abstract. We deleted the

version of the code that now appears only within the model description in section 2.

2. There is an ambiguous phrase on line 79: ". . . all (most of) the domain. . ." I suggest being more explicit.
We changed the sentence in "… the numerical simulation is eddy-resolving in most deep ocean regions equatorward of 60°, while it is mostly eddy-permitting at higher latitude and over shelf regions."

3. In section 2.3, it's important to list more details about the magnitude of biharmonic viscosity, diffusivity, etc. If it's complicated, then a figure can be justified.
We emphasised the differences in parameter settings between the two configurations at the end of section 2.

4. The SST restoring timescale seems very strong . . . This value needs justification.
The SST relaxation is implemented to limit the propagation of the atmospheric forcing biases into the upper ocean, and thus, with this constrain, reproduce a fairly realistic variability of the upper ocean heat content. The air-sea heat fluxes correction induced by the SST relaxation is shown to drive the strength of deep convection in crucial areas such as Labrador Sea and Nordic Seas, thus also positively impacting the strength of the meridional circulation (Storto et al. 2016).
The value of the relaxation time-scale was set equal to that used in the 1/4° reanalysis and simulation system (Storto et al. 2016; Haid et al. submitted to The Cryosphere) and is chosen as trade-off between the daily frequency of the SST analyses, without strongly constraining the air-sea heat fluxes themselves.
However, we acknowledge that due to the SST relaxation the verification of the sea surface temperature may be misleading and driven by the relaxation dataset and time-scale. Thus, in the revised version of the manuscript we focused our analysis (in section 3.1) on the subsurface biases.

Storto, A., S. Masina, and A. Navarra (2016), Evaluation of the CMCC eddy-permitting global ocean physical reanalysis system (C-GLORS, 1982–2012) and its assimilation components. Q.J.R. Meteorol. Soc., 142: 738–758. doi: 10.1002/qj.2673

Haid, V., D. Iovino, and S. Masina. Impacts of Antarctic runoff changes on the Southern Ocean sea ice in an eddy-permitting sea ice-ocean model. Submitted to The Cryosphere

5. The first part of 2.7 should be shifted to 2.8. It also refers to an appendix which isn't present?
Accordingly to reviewer's suggestions, we removed the text in the subsection 2.7 and moved into subsection 2.8, which has been partially rewritten to highlight the differences between the two configurations and avoid repetition in the set-up description. By mistake, we were still referring to an appendix, initially used to describe the eddy-permitting configuration, and then removed, as suggested by the editor. The descriptions of both models are included in section 2.

6. I don't understand the phrase bi-Laplacian (line 217). I'm used to either Laplacian or biharmonic.
We replaced the word bi-Laplacian by biharmonic.

7. On line 233 and beyond, replace TKE with simply KE (as many fields use TKE to represent Turbulent KE).
Thank you for this comment. We wrongly used the acronym TKE to identify both the Turbulent Kinetic Energy the Total Kinetic Energy. In the revisited version, TKE stays for Turbulent Kinetic Energy in section 2, while "total KE" refers to the total kinetic energy in section 3.

8. In 3.1, I'm not convinced that the mean surface biases mean anything in the presence of such strong restoring.

We do agree with the Referee and we removed the plots showing SST and SSS biases (Fig. 2a and b in previous version). Accordingly, we modified text in section 3.1 as follow "As expected, due to the temperature and salinity restoring applied at surface, the global mean SST and SSS biases are small (-0.06 for SST and -0.04 for SSS). There are weak cold biases in the tropics, extending over much of the subtropical band, with the largest SST biases (over 1 °C) collocated with positive SSS error (0.5–1.5 psu) over the western boundary currents in the Atlantic and North Pacific oceans (not shown)."

9. What is called AIW here is usually referred to as AAIW.

Sorry for this oversight. We corrected the Antarctic Intermediate water acronym in AAIW.

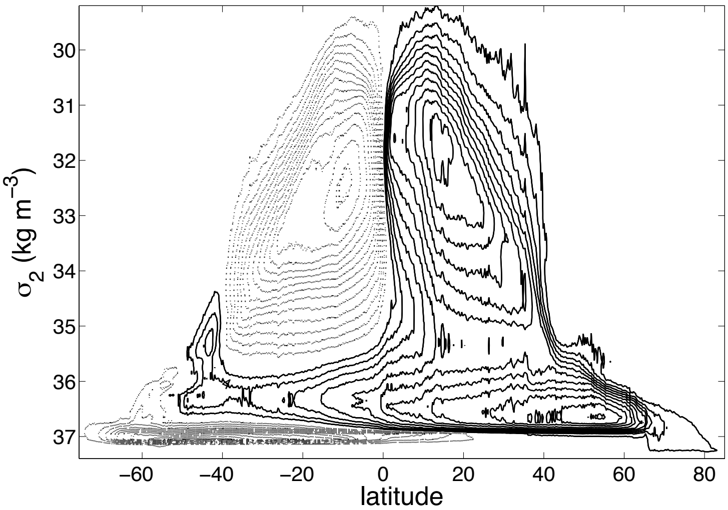
10. I found the results to be somewhat out of order. I suggest putting the global SSH variance maps second, right after the EKE results. In addition, I would put the global transport values before the AMOC results.

We thank the referee for the comment, but we do prefer to keep the actual structure. The global SSH variance is a weakness (probably the largest) of this first GLOB16 experiments. We would prefer not to open the "Model Validation" section with that, but rather to keep it as its close, and recall it in the discussion, where we also suggest how to overcome this weakness.

We liked the idea to move the global MOC before the AMOC, but considering your comments #11, we replaced the global transport computed in depth space with the Southern Ocean MOC computed in density space. After this substitution, it makes sense to us to leave it after Atlantic and Indo-Pacific MOC results.

11. In 3.2, the depth-space overturning means very little in the Southern Ocean. The global MOC should be calculated in density space. The Deacon cell (line 350) is not a physically relevant cell and it would be better to estimate the size of the lower overturning cell in density space.

The use of potential density as the vertical coordinate, rather than depth, is more suitable for representing the MOC in the Southern Ocean, resulting in a better characterization of water mass transport. In particular, the wind-driven Deacon cell, which normally appears in depth-space MOC, is mostly an artefact of zonal and vertical integration at fixed depth level, and does not reflect any real-cross isopycnal flow. Following the Referee's comment, we replaced the global MOC in depth space with the computation in potential density space (where the potential density coordinate is referenced to 2000 db). The new plots for the global density-space MOC is shown below (Plot 1).

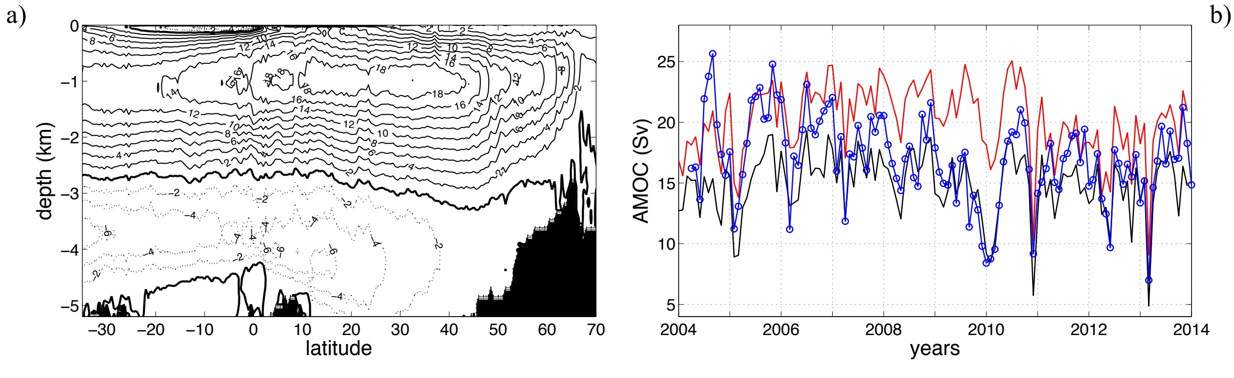


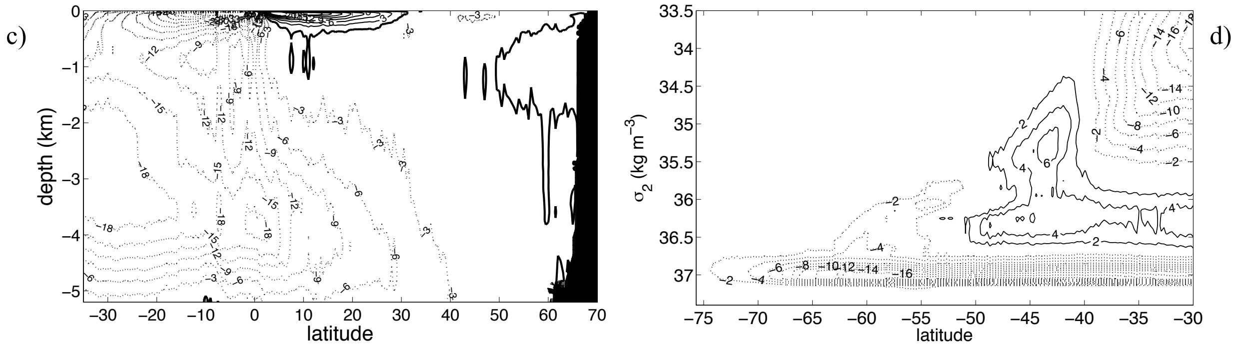
Plot 1. Meridional overturning circulation (in Sv) in potential density space (referenced to 2000 db, $\sigma 2$) for the global domain. Solid lines represent positive (clockwise) contours. The contour interval is 2 Sv.

In the revisited manuscript, in order to better highlight the Southern Ocean circulation, we display a selected latitude range, from the southernmost boundary to 30ºS.

We added a paragraph to describe the new plot: "The MOC in depth-space is not the most suitable representation of the Southern Ocean overturning circulation. The Deacon cell, for example, is mostly due to a geometrical effect of the east-west slope of the isopycnals and no cross-isopycnal flow is associated with it (Döös and Webb 1994, Farneti et al. 2015). To account for a better characterization of water mass transports, the Southern Ocean MOC is presented in density space as a function of latitude and potential density σ2, referenced to the intermediate depth of 2000 m (Fig. 3d). Three primary cells are identified. The wind-driven subtropical cell is part of the horizontal subtropical gyres and is confined to the lightest density classes. This anticlockwise cell comprises a surface flow spreading poleward to 40S, compensated by an equatorward return flow. GLOB16 produces a subtropical cell of 18 Sv at 32ºS. Below, the upper cell is depicted by the large clockwise circulation, with a time-mean maximum value of 7 Sv. It mainly consists of upper circumpolar deep water that flows at depth southward to ~55ºS, upwells from 36.5 kg m^{-3} to lighter density classes and returns northward as AAIW. The anticlockwise lower cell, in the densest layers, reaches 22 Sv and consists of the poleward lower circumpolar deep water and the deeper equatorward AABW. From 60ºS to the Antarctic continent, the transport represents the contribution of subpolar gyres in the Weddell and Ross Seas. Compared to GLOB16, the Southern Ocean MOC in the eddy-permitting configuration presents a stronger and more extended upper cell, but a slightly weaker transport in the subtropical cell, and an almost absent deep and dense flow in the lower cell (not shown)."

The new Fig 3 is reported below.





Plot 2. (Fig. 3 in the manuscript) Meridional overturning stream function (in Sv) averaged over the period 2009–2013, calculated in depth space for (a) the Atlantic and (c) the Indo-Pacific basins, in density space as function of σ_2 for (d) the Southern Ocean. The contour interval is 2 Sv in (a, d) and 3 Sv in (c). Thin solid lines represent positive (clockwise) contours; dashed lines represent negative (anticlockwise) contours. The stream functions were calculated with 0.5º latitudinal spacing to smooth out small-scale variations. (b) Time series of the AMOC at 26.5º N from RAPID observational estimates (blue), GLOB16 (red) and GLOB4 (black) numerical simulations.

12. In Figures 2, 3, 5, 6, 8a,b, 9 and 10 there was no information on the GLOB4 results. However, occasionally, there were references in the text. As noted above, this manuscript will be much stronger if we know where and how improvements between GLOB4 and GLOB16 are manifested, so these results should be included wherever possible.

We thank the referee for this comment. We agree that a more detailed comparison between GLOB16 and GLOB4 can definitely strengthen the manuscript. To present a more complete comparison with GLOB4, we modified subplots and/or add text in any relevant part of section 3, trying not to alter too much the manuscript structure or increase its length.

13. In Fig. 5 I would also like to see a line indicating estimates and errors of each quantity from observations. (Some are listed, some are not). In particular, the Mozambique Channel transport is stated as being "within the range of observed estimates" without a reference!) Also, the ACC transport, listed as the average over all years, is steady and very low for the last 6 years XX it is this equilibrium value that should be listed, not the average over all years.

Thanks for this comment, which helped to improve the plot. On Fig. 5, we added all the observed estimates with associated errors (when available) as reported in Table 2.

To better describe the variability of the ACC transport, we modified the related text in section 3.3 as follow "The zonal circumpolar transport drifts from a mean value of 131.2 Sv in 2004 to 117.3 Sv in 2013. The average volume transport is 122.6 (117.2) Sv over the 2004-2013 (2009-2013) period, lower but comparable to the recent observational estimate over the period 2007-2011 by Chidichimo et al. (2014)." Text describing the Mozambique Channel transport was probably not clear enough. We changed it in "The southward flux across the Mozambique Channel is 23.4 ± 5.4 (20.8 ± 5.8) in GLOB16 (GLOB4) and, for both models, follows within the broad compass of observed estimates, spanning from -29.1 Sv (DiMarco et al. 2002) to -16.7 ± 5.1 Sv (Ridderinkhof et al. 2010)."

14. On line 444, it is ambiguous as to which "two transports are out of phase".

We reworded this sentence as follows "Those transports vary out of phase with each other (Fig. 5d). When the flow is stronger through Fram Strait, it is weaker through Davis Strait and vice versa, indicating that the fluxes out of the Arctic Ocean across those straits partially balance each other."

15. One open question which deserves more investigation is the lack of mesoscale variability in the Southern Ocean. There is a suggestion (in the Conclusion) that this is due to viscous parameterisations, but no quantitative information on what those parameterisations are. The Southern Ocean is one of the key locations where one might expect this resolution to make a

dynamical difference, but the very low variability and ACC transport indicate that something is missing here. I suggest a deeper quantitative comparison with other high resolution models is in order.

Yes, the underestimation of Southern Ocean mesoscale variability reproduced in this GLOB16 simulation needed a deeper investigation. Unfortunately, re-running 10-year experiments is not faceable at the moment. To improve this aspect of GLOB16, we are currently performing short (2-year) sensitivity experiments, which includes a set of values of the coefficient for horizontal eddy viscosity. Preliminary results suggest improvements on both the ACC transport and mesoscale variability.

We have made the following additional changes to the paper:

In the list of References, we updated Stepanov et al. 2016, Storto et al. 2016.

We corrected the Mozambique Channel transport in Table 2 and accordingly added the following reference

Ridderinkhof, H., P. M. van der Werf, J. E. Ullgren, H. M. van Aken, P. J. van Leeuwen, and W. P. M. de Ruijter: Seasonal and interannual variability in the Mozambique Channel from moored current observations, J. Geophys. Res., 115, C06010, 2010.