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Geoscientific Model Development Discussions

Interactive comment on "Introducing LAB60: A 1/60° NEMO 3.6 numerical simulation of the Labrador Sea" *by* Clark Pennelly and Paul G. Myers

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General comments

This study presents LAB60, a model experiment based on a new configuration developed with the NEMO ocean model (version 3.6), coupled to the sea ice model LIM2. The configuration consists of a domain covering the Labrador Sea at 1/60° horizontal resolution, nested into a domain covering the subpolar gyre of the North Atlantic at 1/12°, nested into a regional model of the Arctic and North Atlantic at 1/4°.
As noted by the authors, there are other models at such high resolutions of the North Atlantic (NATL60 [Fresnay et al., 2018] and eNATL60 [Le Sommer et al., in prep.], both

1/60° based on NEMO, and a 1/50° HYCOM simulation by Chassignet and Xu [2017]). Also the concept of a nested domain within a nested domain in NEMO (using AGRIF) to achieve 1/60° resolution is not novel (see for example Schubert et al., 2019). However, the domain of very high resolution is restricted to the Labrador Sea and thus three passive tracers can be implemented and the simulation is planned to be run for more than ten years. These novel features of a simulation at 1/60° clearly make this manuscript a valuable contribution to foster the understanding of the Labrador Sea, its variability and the mesoscale's (and possibly sub-mesoscale's) impact on water mass formation and transport.

The manuscript is well structured and comprehensive in most parts, however I deem some major improvements by the authors necessary before publication:

1. You emphasize the need for a long simulation to study the decadal variability of the Labrador Sea and state that your simulation is suitable for that. However, at least at the time of submission, there were only 7 years (excluding spin-up) of the simulation finished and your statement of having a simulation of more than ten years do not hold. If the simulation does now (time of revision) extend over the stated period, this is fine, if not there need to be adjustments to the manuscript. Additionally, you should think about rephrasing the manuscript at some points concerning the suitability of a ∼10 year-long simulation to study decadal variability.

2. In my eyes, you do not satisfactorily advertise the advantages and improvements of LAB60 in comparison to other, existing high-resolution simulations carried out with the same model (e.g. the VIKING20X simulations at 1/20° [Rieck et al., 2019]). I do think that the presented LAB60 simulation has advantages over the other simulations mentioned and is a valuable addition to the suite of models/configurations simulating the Labrador Sea, but you should make these more clear. Please refer to the specific comments below for more details on this suggestion!

3. The current speed, convective energy, mixed layer depth, etc. are all valuable prop-

erties to investigate and clearly show the differences between the different horizontal resolutions for the processes of interest. However, considering this is the description paper of the model experiment, I suggest to add some analyses of temperature and salinity. Depth sections of temperature and salinity along AR7W for example could be compared to observations and should be familiar to most readers, thus providing a valuable reference future studies could compare their simulations to. Additionally this would give the reader some insight into the vertical structure of the simulation, as most quantities shown in the manuscript are surface values or depth-integrated.

4. There are many, many sentences starting with "While ...". This is not incorrect, however the readability would be greatly increased by a bit more work on the language. Additionally you could put more effort in smooth transitions between paragraphs, especially in the introduction.

5. There are hardly any specific numbers given in the manuscript. The properties are often described as being "large" or "smaller" etc. This makes the results hardly reproducible and also very hard to compare to other studies/models, especially given the fact, that the color scales of the figures are continuous and it is hard to read any values from the figures. Please refer to the specific comments for more details on this remark.

Specific comments

Abstract

l. 10: The restratification after convection could also be mentioned here, as this is a process that is expected to be differently resolved depending on the horizontal resolution.
ll. 11-12 "We implemented [...]": As you mentioned the 1/60° domain of the Labrador Sea before, this sentence reads as if they implemented additional nests into the 1/60° domain.

l. 12 "[...] spans over 10 years": See general comments above. (At least at the time of submission, this was not true.)
l. 16: Maybe better: "[...] impacts the simulation of the Labrador Sea." or "[...] impacts the representation of the Labrador Sea in the model."

Introduction

ll. 23-25: Confusing, first you describe that the current flows northwards and then you say it combines to be the WGC. In my opinion descriptions of current systems should be successively downstream, otherwise it is very hard to follow.
ll. 28-29 "[...] now called the Labrador Current [...]": Please specifiy from which point on the current is called Labrador Current. This is not clear to me at this point.
ll. 31-42: The paragraph describing the eddies in the Labrador Sea is a bit short, considering that resolving eddies is the major improvement and advantage of very high-resolution configurations. You could for example mention the ongoing debate over which type of eddies in the Labrador Sea is most important for the restratification and how your new simulation could help in solving this issue.
l. 33: "[...] instabilities that occur **within the boundary currents** along the shelf break." The boundary currents are a quite substantial ingredient to the instabilities and should be mentioned.
ll. 41-42: This statement requires a reference as it is not obvious that eddies generated at a western boundary should travel eastward into the basin.
ll. 43-47: Listing all the sites of deep convection seems unnecessary here, as they are never referred to again.
ll. 48-49: Weak stratification is a criterion for deep convection, the cyclonic circulation is not strictly necessary, please clarify this. For example "[...] weak stratification which is often achieved by a cyclonic circulation, [...]"'.
l. 53 "[...] relatively drier air [...]": Relatively drier compared to what?
l. 59: You could mention the role of convective eddies in the restratification process

(Lilly et al., 2003; Rieck et al., 2019).
ll. 60-62: I do not understand what you want to convey with this sentence. Please clarify this, maybe you should consider the next sentence when rephrasing, as it seems that there is a repetition of the information that there is buoyant water transported towards the interior.
ll. 67-68: It is not clear what "throughout the North Altantic" refers to. The DWBC is part of the North Atlantic as well, but is mentioned seperately.
l. 74: You should briefly describe how polar amplification is causing additional freshwater and preferrably also include a reference for this statement.
l. 78: What kind of information does satellite altimetry provide and what is it used for?
l. 87: "larger spatial extent" compared to what?
ll. 90-92: One ofyour goals defined at the beginning is to investigate the role of horizontal resolution in simulations of the Labrador Sea. Here, it now seems as if this question has already been answered by an earlier study. You should make clear how your simulation is different to the earlier ones and how it can help in solving the question of how resolving eddies affects the Labrador Sea. Additionally you do not show any investigations into the numerical drift of your simulation, despite stating that the numerical drift is a major problem of simulations of the Labrador Sea even at high resolution.
ll. 96-97: You should carefully rephrase this sentence. Stating that the multi-decade 1/20° simulation resolves eddies in the Labrador Sea makes it hard to justify the need for a 1/60° simulation.
ll. 101-102: Instead of vaguely stating that these simulations have a length of "perhaps only a few years", you could exactly state how long the existing very high-resolution simulations of the Labrador Sea are.
ll. 105-115: See general comment 1.

Methods

ll. 122-123: I suggest briefly describing the extent of the domain here and use the figure as an additional source of information and not the only source.
ll. 125-126: I suggest briefly describing the extent of the domain here and use the figure as an additional source of information and not the only source.
l. 124: The mansucript would benefit from a short explanation of ARGIF's concept of parent and child domains at this point.
l. 134: You should cite Barnier et al. (2006) at this point.
l. 144: I am not sure whether you should phrase this as the "usual NEMO method". There are many ways to compute the mixed layer depth implemented in NEMO.
l. 150: Stating that "[...] smoothing between domains ocurred [...]" makes it sound like you only had a passive role in that. It should be made clear, that you actively decided to smooth between domains.
ll. 152-153: Where and how exactly are the boundary conditions applied? Is there a sponge layer? Etc.
ll. 155-159: It seems random that you give a detailed explanation of how not explicitly including icebergs in the model could affect the freshwater budget of the subpolar North Atlantic, but do not mention other factors, like the choice of initial fields that could also significantly influence the freshwater budget.
l. 160 "[...] including [...]": Is there any part of the atmospheric forcing that is not listed afterwards? If not, then "including" is redundant.
ll. 169-170: What period does "long-term" refer to here and over which area is the heat loss calculated? These numbers should be made reproducible for comparison with future studies/forcing datasets/model simulations.
ll. 174-175: You never declared your interest in conducting a simulation past 2017, so the need for a different forcing set is not obvious here. Your goal to have a simulation that extends (almost) until present should be stated as it could be quite important for potential collaborators on the analysis of the produced model output!
ll. 177-179: I guess that the lack of interannual variability is caused by the missing deep convection due to the weak forcing. You should clarify this.

ll. 179-182: Do the different spatial and temporal resolutions have any consequences for the observed behaviour in Fig. 1? Otherwise I suggest to move this information to the part were the forcing datasets are described in general.

ll. 182-185: You state that the remainder of the manuscript will only deal with LAB60-DFS, however later in the Methods section you describe the start of the computation at the Graham cluster and the spin-up period, which were done under CGRF atmospheric forcing if I understand correctly. This is confusing. I understand that describing such a complicated simulation pathway (switching forcing, switching computing cluster) is not an easy task. However, you should try to make it more clear. Is the whole used simulation from 2002-2011 called LAB60-DFS, or only the part from 2007-2011 where the DFS forcing is actually used?

l. 186: It is not clear to me what "internal testing" refers to in this context. Why is it internal?

l. 187: You should specify what "overhead" means. I guess it refers to the additional computational costs/time, but this needs to be made clear.

ll. 187-193: A list with three items (the three passive tracers) would be a beneficial structural element at this point. Additionally, you should explain the choices made regarding the thresholds for the definition of the different water masses.

ll. 194-195: You should clarify what "here" refers to, also there seems to be word missing between "such" and "resolution".

ll. 208-209: It is not clear to me how you learned that your simulation is unstable from interpolating data. Additionally, "quickly go unstable" is rather unspecific.

l. 211: You probably mean the opposite: Large (long) time step first, then decreasing to small (short) time step. Please clarifiy.

ll. 217: At the end of the Methods section, the mansucript would greatly benefit from a clear short description of the simulation used for the analysis, making clear that the spin-up was done under CGRF forcing from 2002 to 2003, then there are 2004-2006 under CGRF forcing, followed by 2007-???? under DFS forcing. Maybe a simple schematic could help here, otherwise the reader has to skip back and forth through

the Methods section to gather this information.

Model Simulation Results

ll. 220-222: Compared to what do the large differences occur? If you refer to the differences between LAB60, SPG12, and ANHA4, then it sounds like you want to use the large differences (compare the simulations) to understand the large differences. This sentence should be rephrased for clarification.

ll. 224-225: At which depth are the current speeds compared? Additionally, you should provide some numbers here, "greater" and "slower" are not very specific.

l. 230: Which shelf breaks? Shelf breaks have not been mentioned before in the manuscript.

l. 231: At which depth is the eddy kinetic energy investigated?

l. 231-253: You should use values when comparing the EKE in different regions and among different simulations. Using mostly larger and smaller makes it hard to keep track of how which result compares to which. Comparing numbers makes this a lot easier in most cases (additionally a comparison to other simulations and/or observations could be enabled).

l. 231-232: You should clarify how you compute geostrophic velocities, and discuss that (at 1/60° resolution) your simulation might resolve submesoscale processes and features that are important for the restratification (among others) and are not completely represented by geostrophic currents.

l. 232: In the definition of EKE, you should specifiy what the primes and overbars denote. Specifically, over which period the currents are averaged to calculate the deviations from. The choice of this period can have an influence on the results (Kang and Curchitser, 2017).

l. 232: AVISO has not been introduced to the reader at this point. You should describe the data you use in the Methods section. Which data is used from AVISO, SSH or the geostrophic currents? Also, please note that many of the commonly used SSH products are not distributed by AVISO anymore (since 2017) and it might be useful to update the data and use the new versions distributed by CMEMS (https://marine.copernicus.eu).

l. 233 "[...] EKE coming from [...]": One can not necessarily infer any direction of propagation from the maps of EKE. In these cases where it is likely that the high levels of EKE in one region are caused by propagation of (mostly) eddies into that region, I would suggest writing something along the lines of : "High levels of EKE can be found along the west coast of Greenland, extending into the interior of the basin at ?? North...". You should check the whole manuscript for this formulation ("[...] EKE coming from [...]") and adjust it.

l. 235-236: The result, that EKE is closely bound to the Labrador Current and the shelf break in the western Labrador Sea does not receive enough attention in my opinion. As far as I know, whether these boundary current eddies impact the deep convection region and the restratification or stay too close to the basin's boundary is still a matter of ongoing debate (e.g. Chanut et al., 2008; Gelderloos et al., 2011; Rieck et al., 2019) and a 1/60° simulation could clearly help in sovling this issue.

l. 236 "[...] has lower levels of EKE [...]": Lower compared to what?

l. 238-240: See above. EKE "coming from" somewhere and "entering" does not seem to be the best way do describe these results.

l. 242-243: See above. You could mention at some point earlier in the manuscript that "the EKE coming from the west coast of Greenland" is related to, or mostly consists of, Irminger Rings. That would probably help in describing the results later on (e.g. by using the phrase "Irminger Ring path" or something similar).

l. 244 "AVISO observations": See comment above on AVISO.

l. 246-253: I suggest that you additionally investigate EKE at depth. EKE at depth in the central Labrador Sea could be an indicator of the presence of convective eddies (which should be resolved at this resolution in contrast to other simulations with up to 1/20°) and could even be compared to observation of Fischer et al. (2018).

l. 254: I suggest you use a different date to show a snapshot from, as you mention

earlier that 2003 is still in the spin-up phase and you will only present model results from 2004 ongoing. This is not consistent.

l. 255-256: Again, no numbers, just "very strong" and "reduced".

l. 256: A brief explanation of convective energy would be extremely helpful here. The meaning of convective energy does not coincide with what the reader might intuitively think of when reading "convective energy" (the energy of convection).

l. 261-264: If you observe the described properties and processes in your simulation, I strongly suggest to show that and not just speculate. Additionally, there could be observational support for these speculations in Lilly et al. (2003) so I suggest checking that.

ll. 276-277: From the sentence, it is not clear to me whether the path of strong stratification is located between the 2500m and 3000m isobaths everywhere, or the path starts at the coastline between these isobaths. I suggest to formulate this more clearly.

ll. 280-281: This sentence sounds like the small convective energy is required to achieve weak stratification, whereas the convective energy is basically just another measure of stratification. I suggest rephrasing this. Addtionally, this fact has already been mentioned before.

ll. 285-286 "[...], limiting the mixed depth between the 2000m and 3000m isobath.": It is not clear to me what exactly you want to convey with this part of the sentence.

l. 287: You probably mean that the region where the mixed layer is deeper than a certain threshold is larger. The ocean has a mixed layer everywhere, so you cannot really reduce its spatial extent.

l. 293: The bottom of the mixed layer returns to the near-surface, the mixed layer is always connected to the surface.

ll. 295-296: There are several questions regarding the definition of the Labrador Sea Water. 1. How can you define the maximum density as the thickest depth? What is a "thickest depth"? 2. Referred to what does the density need to change by 0.001

kg/m^3? 3. Why do you calculate the MLD based on gradients and then for the definition of the LSW you use thresholds, wouldn't it be more consistent to also use a threshold for the MLD then? 4. How does this way of defining LSW compare to the way you defined your LSW tracer and what implications does this have?

ll. 297-298: Where could stair-stepping patterns emerge? Between years? And what are stair-stepping patterns? The same as staircase patterns?

l. 316: I am not completely sure what "enters the interior 2000m and 3000m isobath" means. Do you want to convey that the water mass propagates into regions where the water is between 2000 and 3000m deep? Please rephrase this sentence to make this more clear.

ll. 318-319: Stating that water ends up in the Labrador Current sounds like this water will never leave the Labrador Current. However, I suspect that the water still in the Labrador Current has just not yet left the current to the South or East due to the short integration time of your simulation. Using the phrase "ends up" is thus rather misleading.

l. 323: I am not sure what "within the 2000m and 3000m isobath" means.

l. 324: What is a "thicker amount"? Do you mean a "larger amount"?

ll. 328-329: Could you state how your definition of the tracer compares to your earlier definition of LSW?

Discussion

ll. 337-339: In the manuscript you do not really describe how submesoscale processes impact deep convection so it is irritating that you mention it in the discussion. I suggest that you add a paragraph to the Results section briefly showing that your simulation resolves the submesoscale and how that could impact deep convection and water mass formation. One of the key reasons to carry out a 1/60° simulation probably is that it resolves the mesoscale in the Labrador Sea and starts to resolve the larger end of the submesoscale range. I think you should make it more clear that

your simulation is capable of doing this and not just showing the end result (LSW for example) and speculate that the differences to lower resolutions are due to the missing (sub-)mesoscale.

ll. 348-349: At this point you should compare your results from the 1/60° simulation to earlier studies with lower resolutions to point out the differences and espcecially improvements achieved by increasing resolution. At least for the Greenland meltwater, there are several studies investigating the fate of this tracer in simulations with lower resolutions (e.g. Böning et al., 2016 and others...).

Tables

l. 526: In the Methids section you state that you refer to the whole configuration is LAB60. In this table it looks like you refer to the parent domain as ANHA4, the first nest as SPG12 and only the second nest as LAB60. This should be made consistent.

Figures

In general I suggest to use larger fonts in the figures, especially for the titles (The titles should be at least as large as the manuscript font size.). Additionally, you could use some summarizing titles stating the property to be seen in the individual subplots (additional to the LAB60/SPG12/ANHA4/AVISO titles). I strongly recommend adding the units to the colorbars and also suggest using different colorscales, as these continuous scales sometime make it nearly impossible to read accurate values from the figure. It is not easy for example to distinguish between values of 0.2 m/s and 0.3 m/s in Figure 3 or 200 cm^2/s^2 and 500 cm^2/s^2 in Figure 4. (The colorscale used for the supplementary video showing LAB60s MLD is a good example of a discrete color scale where one can read values from the plots easier!)

l. 552: Speed at which depth?

l. 557: Eddy kinetic energy at which depth?
l. 563: Relative vortcity at which depth?
l. 568: Speed at which depth?

Technical corrections

Abstract

ll. 9-10 "The transport of these fluxes [...]": Transport and fluxes are used synonymously here and thus this should just read "These fluxes [...]" or "This transport [...]".

Introduction

ll. 32: Frajka-Williams
ll. 80-83: This sentence should be split for better readability.
l. 89: "[...], both which" should be rephrased
l. 105: "high resolution" should be "high-resolution"

Methods

l. 123 "includes a nest": To be precise, it includes two nests.
l. 133 "horizontal grid resolution": I suggest using "horizontal grid spacing" here.
l. 136 "[...] primarily only [...]": You should decide on either "primarily" or "only".
l. 137-139 "All domains used [...]", "Lateral diffusion used [...]", etc.: This should be rephrased to something like "[...] scheme was used in all domains.", "A Laplacian operator was used/implemented to compute lateral diffusion [...]", etc.
l. 151: "boundary nests" should be "the nest boundaries".
l. 171: "which were" should be "which was".
l. 175 "[...] Fig. 1 identifies [...] between [...]": should be "[...] Fig. 1 depicts [...] the

difference in mixed layer depth between [...]".
l. 190: "pathways which" should be "pathways along which".
l. 194: "masses" should be "water masses".
l. 194: "before in the past" should be either "before" or "in the past".
l. 202: "increase in simulation length" should probably be "decrease in simulation length".
l. 207: "[...] the occurrence of seasonal sea ice."

Model Simulation Results

l. 222: "ANHA12" should be "SPG12".
l. 243: "produce" should be "produced".
l. 244: "they match" should be "it matches".
ll. 252-253: duplicate mention of "supplemental"/"supplementary".
l. 255: "show" should be "shows".
l. 270: "ANHA12" should be "SPG12".
l. 271: "supplies" should be "supply".
ll. 275-276: duplicate use of "visible".
l. 283: "depth" should be "depths".
l. 283: "observation" should be "observations".
ll. 299-301: Please rephrase this sentence, the "though" seems unnecessary and "has this [...] being less dense." does not seem right.
l. 305: "between the" should be "in all three".
l. 306: "indicate that deep mixing is easier" should be something like "indicates that deep mixing is more likely".

Discussion

l. 350: "project" should be "projects".

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