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Discussions

Interactive comment on “Influence of numerical schemes on current-topography interactions in 1/4° global ocean simulations” *by* T. Penduff et al.

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Q1. An interesting point that may be discussed is that the 3 elements tested here all bring the z-level model closer to a terrain-following-like model (i.e., sigma models do not have steps, usually do not have side walls if extend to coastal areas and can run with low horizontal viscosity due to the smooth bottom). In fact, the improved results here, such as more intense bottom currents and recirculation gyres resemble to large extent the results from early basin-scale terrain-following ocean models (e.g., Ezer and Mellor, 1997, discuss those elements when compared their sigma model to the z-level CME model).

The bottom pressure gradient between two adjacent partial cells is computed at the depth of the shallowest of the two points, after having interpolated T, S and computed density at that depth. The bottom pressure gradient is thus computed on the horizontal, thus not subjected to the pressure gradient error that affects sigma-coordinate models (emerging from truncation errors in the small difference between two very large terms). Therefore, with respect to pressure gradient, our partial-step model formulations does not become sigma-like. Note also that since the pressure gradient computing scheme does not extrapolate tracer values vertically, it is not subjected either to the "hydrostatic inconsistency" sometimes met in sigma-coordinate models. Unlike full steps, both sigma-coordinates and partial steps [1] do not distort topographic slopes, thus represent correctly the ambiant vorticity field and topographic vortex stretching; [2] allow direct communications (advection, diffusion, pressure gradient computations, etc) between adjacent cells along a sloping bottom, and thus simulate a more continuous bottom Ekman transport (also see remark 8 below). This might act to smooth bottom flows and make our partial steps solution more sigma-like. Verifying this latter hypothesis lies beyond the scope of our paper, but we have summarized the present discussion in the revised manuscript (conclusion) with a reference to Ezer and Mellor 1997.

Q2. Fig. 1 shows mean transports, but this does not tell the whole story. It may be useful to show the MOC stream function, say for the Atlantic (is it shown in the previous paper?).

As told in our section 3.1, the Atlantic and Global MOCs are shown in Barnier et al (2006)'s Figure 3: we propose not to reproduce it here.

Q3. The introduction discusses the DYNAMO program, but another recent program, the Dynamics of Overflow Mixing and Entrainment (DOME) is also very relevant as it compares topography-current interaction in overflows in isopycnal, z-level and terrain-following models (Legg et. al, 2006; Ezer and Mellor, 2004; Ezer, 2005). [a] In this context it may be useful to mention that the improvements done in the DRAKKAR model here are likely also to help in improving overflow simulations (has this been tested?). [b] The more intense deep boundary currents seen in the EENP experiment may not be only due to better treatment of local topography, but also due to better deep water formation. This should be looked into. Also, in Fig. 7 the fact that partial cells cause the large Ts values to shift into deeper ocean depths relative to a step topography case may be related to dense bottom waters that extend further downslope. This is similar to results seen in the DOME experiments where terrain-following and isopycnal models transport dense plumes further downslope compared with stepped topography z-level models of the same resolution.

[a] The use of EEN instead of ENS has been tested in a DOME-like context (G. Hervieux, pers. comm.), and does not modify the behavior of overflows. [b] In the DRAKKAR realistic setup, neither overflows nor their expected large-scale influences (i.e. AMOC) are modified by these schemes. Indeed, Barnier et al (2006, their section 2.4.2) and we mention that EEN and partial steps do not change the southward transport of Nordic overflows which remains very close to 6 Sv; do not change the structure, depth and magnitude of the upper AMOC (see section 3.1 and Fig 1-a); improve the path of the DWBC in the Atlantic, make it narrower and faster but do not modify its transport or depth (In that respect, the sentence “The increase of barotropic transports at Bering, Fram and Denmark Straits may be linked with the enhanced deep overturning mentioned above, since these throughflows feed the Deep Western Boundary Current (DWBC)” in our section 3.1 appears misleading and was removed, since the lower overturning cell that increases with the new schemes is not fed by these throughflows). More importantly perhaps, EEN and partial steps rapidly affect many large-scale areas throughout the world ocean, far from overflows. Improvements of the solution, including stronger and deeper-reaching topostrophy, are thus not consequences of modified overflows. These arguments are summarized at the beginning of section 3.2.1

Q4. P. 496, first par.- if turbulence closure model is used, does the artificial large vertical mixing imposed in static instability cases really needed?

In the case of static instability, NEMO’s TKE closure model does increase vertical mixing up to a few tenths of m.s-2. Following the results by Lazar (1997: La branche froide de la circulation thermohaline: sensibilité à la diffusion turbulente dans un modèle de circulation générale idéalisée. PhD thesis, Université Pierre et Marie Curie, Paris, France, 269pp.), we increased it to 1 m.s-2 to restabilize unstable water columns more efficiently. Note however, that changing this convective mixing coefficient from 0.1 to 1 then to 10 m.s-2 does not have any significant impact on the time-mean MOC (Marotzke and Scott, JPO1999: Convective Mixing and the Thermohaline Circulation). This classical choice is made in many model studies, with or without NEMO; we thus propose not to mention it, unless the editor requests it.

Q5. Fig. 3- it may be a little easier to compare model and observation figures when using similar color scales (at least add the zero contour to distinct between different flow directions).

We agree: the figure has been modified. Now it shows the observed and simulated mean velocity sections with the same color code and contour intervals.

Q6. P. 500 & Fig. 4- It is interesting to note that despite the improvement in the recirculation gyres, the Gulf Stream separation off Cape Hatteras is still not satisfactory and resembles (though to lesser degree) the problems experienced in the early CME model. The fact that isopycnal models and terrain-following models of comparable or even coarser resolution get better GS separation indicates that there is still something missing in the treatment of topography in z-level models.

The only rigourous comparison between sigma-, geopotential-, and isopycnal-coordinate North Atlantic models at eddy-admitting resolution has been done in DYNAMO. The GS separation was different in the 3 models, but no clear superiority of the sigma- and isopycnal-coordinate solutions has been shown in Dynamo. It is thus not obvious that GS separations are more realistic in eddy-admitting sigma and isopycnal models than the one simulated in EENp (which sigma-coordinate and isopycnal simulations does the reviewer refer to?).

However, the GS still overshoots somewhat to the North in EENp, suggesting that either "something is missing", or that something is not well discretised. (The revised figure 3 better illustrates that somewhat downstream of Cape Hatteras, the Gulf Stream is quite realistic, though). It is acknowledged that the GS separation problem is a complex issue controlled by the interplay of many processes (e.g. Dengg et al 1996). Enhanced inertia/non-linearities, in fact, appear as a more solid candidate for the "missing" ingredient. Smith et al (JPO2000) for instance show that enhanced resolution largely improves the POP solution in that region. More generally, recent 1/10-deg-class or finer simulations based on diverse numerical formulations (POP, NLOM, OFES, HYCOM, MICOM, etc) yield more realistic GS separations. A 1/12° version of our present NEMO configuration also exhibits a realistic and persistent GS separation (see Fig 2 in The DRAKKAR Group, 2007: Eddy-permitting ocean circulation hindcasts of past decades. Clivar Exchanges, No 42 (vol 12 No 3), 8–10.).

We prefer not to address the link between vertical coordinates, resolution, and the GS separation issue for three reasons: (1) this is not our objective; (2) it is not clear whether eddy-admitting sigma and isopycnal models do improve the GS separation (3) the 1/12-deg NEMO integration is still ongoing. A comparison between our solutions with specific sigma-coordinate solutions is interesting (but not particularly in terms of GS separation, see item #1 above). It is discussed in the revised conclusion.

Q7. P. 503- The topostrophy analysis is interesting. However, mesoscale turbulence may not be the main reason to align mean currents along topography in partial cells experiments, but that slopes and topographic features are simply better resolved and are not distorted by stepped topography.

We do not write that "mesoscale turbulence (is) the main reason to align mean currents along topography in partial cells experiments": section 4.1 and figure 7 show that topostrophy is clearly more sensitive to EEN than to partial steps. However, both numerical changes contribute, in various proportions, to improve the dynamics and various aspects of the solution.

First, we note that EEN+partial steps yield a remarkable improvement in the Argentine basin w.r.t. both eddy and mean flows, in particular a strong Zapiola anticyclone that is driven by eddy-topography interactions (Dewar 1998; de Miranda et al 1999). At least in this area, these interactions are certainly responsible for the improved mean state. More generally, enhanced resolution is known to simultaneously improve the distribution of eddy activity and mean currents (e.g. at 1/10°, Smith et al, JPO2000) in numerical simulations. Merryfield and Scott (2006) recently showed that improved mean and eddy flows come along with stronger topostrophies at global scale, thus suggesting that eddy-topography rectification might contribute to the improvement of mean flows, in accordance with theories by e.g. Holloway (1992) and Dewar (1998). Barnier et al (2006)'s results and our study show that EEN and partial steps bring the 3 features (mean circulation patterns, EKE distributions, topostrophy) close to 1/10° standards throughout the World Ocean (not only in the Argentine basin). These major improvements do not come from increased resolution in our case, but from the use of improved numerical schemes at eddy-admitting resolution.

The enhanced topostrophy (alignment of mean currents as expected from these theories) seen globally in EENp may not be due to mesoscale turbulence only, but our findings strongly support a scenario where [A] topographic constraints are much better represented by EEN and partial steps, [B] yielding stronger eddy-topography interactions and [C] contributing to improve the mean circulation (at least) in eddy-active, topographically-controlled regions.

Point [A]: we note in the manuscript that partial steps largely reduce the distorsion on topographic slopes (especially gentle ones), on planetary vorticity (f/h) contours, allow "sigma-like" connexions between bottom cells and thus a better representation of bottom Ekman pumpig and vortical dynamics (see our answer #1 to reviewer A, and the modified conclusion). EEN and partial steps also reduce near-bottom friction and momentum dissipation (as shown by EENp_ns solution in section 5; this is shown for EEN by Le Sommer et al 2007).

Point [B]: both smoother f/h contours (partial steps) and EEN (partly because of a wider stencil, see Le Sommer et al 2007) reduce mesoscale topographic roughness (explicitly and implicitly, respectively). QG and PE studies by e.g. Panetta (1993), Rivière (2004), Treguier and Hua (1988) showed that this reduction is expected to yield two consequences: enhanced subsurface EKEs and MKE (consistently with a more efficient inverse cascade). Stronger eddy flows along better-resolved topographic slopes are also expected to promote eddy-topography interactions (Dewar 1998; Holloway 1992), thus to yield mean flows that align more in the direction of topographic Rossby waves (stronger topostrophies, Merryfield and Scott 2006). All these predicted effects appear simultaneously in EENp: enhanced mean and eddy flows at depth (Fig 8), and stronger topostrophies (fig 7).

[Point C] Enhanced deep EKEs and MKEs get closer to current meter observations (see Penduff et al 2002, 2005). Ongoing investigations by T. Penduff and G. Holloway (unpublished) show that Drakkar topostrophies get increasingly realistic (with respect to current meter observations) in the sequence ENSf-EENf-EENp, up to 1/10° model standards. Simulated currents are also improved by EEN+partial steps in many ways and globally (Barnier et al, our study).

The eddy-driven character of mean circulation improvements is thus not formally demonstrated, but very consistent with our findings and the literature. Accordingly, we conclude in section 4.1 that the strong evolution of DRAKKAR topostrophies from a non-eddy-admitting character toward an eddy-resolving character in the sequence ENSf-EENf-EENp does "not demonstrate that eddy-topography interactions in ENSf and EENp are responsible for the changes in Ts, but (is) strongly suggestive of such an effect." (as Merryfield and Scott, 2006). We do not think that the manuscript is equivocal on this particular issue (the one raised by the reviewer).

To keep the paper short enough, and since it is already given in another way, we suggest not to add the discussion above in the revised manuscript. However, if the editor and/or the reviewer consider that the rephrased discussion above would clarify our arguments, we may add it in the revised paper.

Q8. One point that should have been mentioned is the fact that the models do not resolve bottom boundary layers. If BBLs were resolved, the direction of the near bottom flow would have been affected.

Resolving the bottom Ekman spiral would probably modify near-bottom velocity directions and topostrophy indeed, (but this would require at least 500 z-levels...), but probably not large- and meso-scale current-topography interactions. Indeed, 46-level models using a bottom stress parameterizations like ours do not resolve the details of the bottom Ekman layer, but correctly represent bottom Ekman transports and pumping, both of which control eddy-topography interactions (see e.g. Dewar 1998) and topographic steering of mean currents (Arhan et al, 1989); this is mentioned in the revised conclusion. Moreover, bottom Ekman pumping is certainly better represented with partial steps (see remark #1 above).

Q9. It is difficult to see the details in Fig. 9a.

Fig 9 is a high-definition picture. Could Ocean Science editors print Fig. 9 as big as possible? We have slightly magnified Fig 9a. We might also split Fig. 9 in two parts if this is not sufficient.

Interactive comment on Ocean Sci. Discuss., 4, 491, 2007.