Ocean Sci. Discuss., 4, S300–S305, 2007
www.ocean-sci-discuss.net/4/S300/2007/

Ocean Science
Discussions

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

T. Penduff et al.

Received and published: 23 October 2007

Preliminary remark: Comments #1, #2 and #3 do not point at any particular weakness in our results or interpretations. They question our objectives, approach, and the structure of the paper. We comment on the approach in #1, and provide answers to specific question thereafter.

Q1. The scope of the manuscript appears rather engineer-like and I miss the physical motivation of the work. It is clear that the choice and numerical representation of the momentum scheme, topography representation and sidewall boundary condition affects the model simulation. Choosing the best one compared to observations amongst all simulation is the standard engineering approach (and common amongst modellers). However, as a scientist (writing a scientific paper) one has to ask what the principle dynamics are which might be at work (concerning dissipation near the bottom and side walls) and how one can represent or parameterise those dynamics in the model (eventually by choosing adequate numerical formulations).

The reviewer raises important but general questions (how does dissipation work in the real ocean? Which set of model parameters and parameterisations would best represent those?). As clearly stated in our title, our goal is, however, not to explore these vast issues. We aim at carefully quantifying the sensitivities of a widely-used model to numerical schemes in a complex, global, non-linear, realistic setup, and proposing dynamical interpretations for these based on previous numerical, observational and theoretical studies. Reviewing present knowledge concerning topographic constraints, dissipation processes, and numerical schemes would require dedicated publications each. More importantly, basically nothing is really known about the links between these issues at a given grid resolution, even in very idealized, unforced dynamical contexts.

The demonstrative approach mentioned by the reviewer is thus not adapted to the study of the impact of complex, numerical schemes interacting together at a given resolution on the 4D multivariate balances at work in a realistically-forced non-linear global ocean/sea-ice PE model after 10 years, and on subsequent circulation changes. In particular, one would not expect *a priori* that our model sensitivities (to momentum advection schemes and topographic representation) and subsequent circulation changes should be interpreted in terms of near-bottom dissipation. Integrations need then to be performed before describing, evaluating (sections 3, 4, 5) and providing plausible interpretations (section 6) of the model's sensitivities. Our investigations actually go beyond the present study. As mentioned in our paper, our work extends below the surface a comparison performed between DRAKKAR solutions, other model solutions, and surface observations (Barnier et al, 2006), and complements a numerically-oriented investigation of the impacts of 3 momentum advection schemes in a similar realistic setup (Le Sommer et al, 2007).

Additional remarks: Experienced modellers know that the calibration, evaluation, and improvement of models requires pragmatism in the approach, rigor in the interpretation,

OSD

4, S300–S305, 2007

Interactive Comment

and the expertise of GFD-oriented scientists, applied mathematicians, observationalists, atmosphere scientists, engineers, etc.. As explained in the introduction, our numerical choices are grounded on present knowledge coming from highly (e.g. Arakawa and Lamb 1981) or partly (e.g. Treguier and Hua 1988; Pacanowski & Gnanadesikan 1998) idealized numerical studies, process-oriented theoretical studies (Dewar 1998; Holloway 1992), detailed assessments of previous "realistic" experiments (e.g. FRAM, CME, DYNAMO, CLIPPER, DRAKKAR, etc), as well as observational results (Arhan et al 1989; Dengler et al 2004). Also see item 4 below. This approach is indeed very "common amongst modellers" and is fruitful; it helped improve the dynamical consistency and the realism of numerical simulations over the last decades, thus stimulating scientific research. The distinction between "engineer-like" and "scientific" is thus subjective, and somewhat schematic.

Q2. [a] I would advise the authors to try to reformulate their paper, by stating in the introduction what is known about the role and meaning of side-wall boundary conditions, dissipation and topography for the large-scale dynamics, mesoscale eddies and the energy cycle (including energy and enstrophy cascades in wavenumber space) in the ocean. Part of such a discussion is given in the last section of the manuscript, which should be extended and moved to the front. Further they should outline [b] why they discuss the different model experiments and [c] what insight can be expected into the fundamental dynamics. The model versions differ in the numerical formulation of the momentum advection scheme, the representation of topography (full step vs. partial step) and sidewall boundary conditions (no slip vs. free slip). It is shown [d] qualitatively, that all such differences have comparable effects on the simulated circulation, i.e. demonstrate the sensitivity of the model simulation on dissipation. [e] Although this result is not very surprising it might warrant publication in order to document it.

[a] As in many model studies in realistic setups (e.g. Willebrand et al, 2001, among many others), the construction of the present manuscript simply reflects the impossibility to predict the detailed response of a complex model to complex numerical changes

OSD
4, S300–S305, 2007

Interactive Comment

EGU

in non-linear regimes (see our answers to comment #1 above). We thus think it is more honest and legitimate to keep the current construction (presentation of model strengths and weaknesses, of promising numerical formulations, then quantification of sensitivities and dynamical interpretations), than to announce in advance results that were objectively not predictable. For this reason, the general structure of the paper was not modified. However, many remarks made by the reviewers have been taken into account and helped improve the manuscript. [b] The reasons why we discuss the model experiments are actually given in the introduction (also see our answer to point #1 above). [c] We do not look for specific "insights (...) into the fundamental dynamics": our title and introduction explain that we aim at quantifying and interpreting how and why this complex simulation tends toward observations in many aspects, with several references to previous model studies (see section 5). This is part of the modeller's responsibility. Instead, our approach provides new quantitative results and interpretations about the tridimensional, global impact of ocean model parameters; our conclusion sheds light on various dynamical and modelling questions that had been addressed in idealized contexts (e.g. Barnier and Le Provost 1993, Dewar 1998, Adcroft and Marshall 1998) and provides a retrospective interpretation of significant modelling efforts (e.g. comparative impact of numerics used in models like OCCAM, CLIPPER, OFES, SPEM, etc). Present DRAKKAR simulations, which confirm the benefits of EEN and partial steps, are being studied by many research groups worldwide. Our study is different from (but preliminary to, thus neccessary for) such process-oriented studies, that should provide additional insight into specific processes. [d] We do quantify how the schemes affect the dynamics and the solution (e.g. topostrophy, sections, kinetic energy, vertical velocities, etc) of the simulations at global and regional scales. [e] Some readers might not find our results "very surprising", but these major sensitivities had not been investigated previously. Experience shows that the modeling community needs descriptions and interpretations of the sensitivities of widely-used models for further improvements. Researchers working on simulation outputs also request observation-based model assessments and dynamical interpretations.

Q3. [a] It would also be useful to discuss the impact of lateral friction and [b] in particular the choice and amplitude of bottom friction in the model. [c] Ideally one would like to see one or two additional model experiments in this respect, but it might also be that previous experiments with similar model versions can be utilised.

[a] The impact of both explicit (no-slip) and implicit (numerically-induced) sidewall friction on simulated currents is quantified, compared and discussed in terms of large-scale and regional circulation patterns, kinetic energy structure, topostrophy, and current-topography interactions throughout the paper. [b] The formulation of bottom friction was missing in the submitted draft indeed. It is detailed in the revised manuscript (section 2): the quadratic bottom stress parameterization is standard (see e.g. Willebrand et al 2001, Treguier 1992) and takes into account the impact of residual tidal currents in a simple way. [c] As explained in the introduction, we do not aim at investigating the effect of the amplitude of bottom friction. More generally, understanding the combined impact of many parameters in complex setups is important, but this is a longer-term effort that requires a lot of computing time, sustained collaborations among the modelling community, and much more than one paper. Barnier et al (2006) and our study go in that direction through the comparison between several past and recent model solutions, and dynamical interpretations in the light of existing results (as done in our conclusion).

Q4. Furthermore, I would like to see a discussion of the meaning of enstrophy and energy conservation properties of the momentum scheme. What physical principles are implemented here and why? I guess the different numerical implementation yields different dissipation, so what kind of parameterisation for dissipation is inherent to these schemes?

We agree that additional information is needed on this issue. Arakawa and Lamb (1981)'s main results are summarised in the revised article. As mentioned in our paper, Le Sommer et al (2007, in revision for Ocean Modelling) present both momentum advection schemes and study in detail their dynamical behavior in a similar realistic

OSD
4, S300–S305, 2007

setup (these authors are ready to provide the editor with this presentation). It does not seem necessary to duplicate this information here.

Q5. It should also be stressed that it is no good scientific style that those schemes are only detailed in a technical report (referred to as Madec, 2006) which is apparently not accessible to the public.

The schemes are described in Sadourny (1975) and Arakawa and Lamb (1981). Le Sommer et al (2007) presents these schemes in detail and extensively study their dynamical impacts in our model; we are ready to provide the editor and reviewer with this description. NEMO's new reference manual is almost ready and will soon be available on http://www.lodyc.jussieu.fr/NEMO. In the revised paper, we cite this website on which the reference manual will be available when our article is published. Note that all the technical information relevant to the present study can be found in the previous OPA manual (on the webcite), in Sadourny (1975) and Arakawa and Lamb (1981). The paper by Le Sommer et al (2007) and the new NEMO manual will be both available soon.

Q6. Having read reviewer's A comments I cannot stop myself in asking the following: When the present z-level model becomes similar to sigma-coordinate models, does it also suffer from the problems of those models? That is, how large is the pressure gradient error due to the partial step formulation? Is it possible that the model sensitivity near boundaries are due to this error? Note that again the partial cell (and pressure gradient) formulation in the OPA code appears to be undocumented.

See our answer #1 to Tal Ezer.

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