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Ocean Science
Discussions

Interactive comment on "Evaluating two numerical advection schemes in HYCOM for eddy-resolving modelling of the Agulhas Current" *by* B. C. Backeberg et al.

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Reviewer #1 Comments

Reviewer comments are given in bold font, with the responses following below.

1 Major comments

1.1 Detail the viscosity function you use in your simulations, and the boundary conditions (free slip?).

The "real" QUICK scheme (Farrow and Stevens 1995) has implicit diffusion/viscosity. Webb (1998) has developed an explicit version of the QUICK

where diffusion/viscosity is proportional to the magnitude of the velocity. In Winther et al, some modifications have been done to this term. Is it the same as in Winther 2007? Please give more details on your numerical configuration.

Winther 2007 used a fourth order advection scheme based on Holland 1998 and Webb 1998. They modified the bi-harmonic viscosity as to minimize the viscosity coefficient (from Annex A Winther 2007): Viscosity proportional to the velocity is chosen to avoid excessive smoothing of small currents. A small background (Smagorinsky) viscosity is applied to counter "shocks". Constant momentum dissipation terms (veldf2/4) are applied to dampen small currents.

We use the same scheme as Winther 2007. Values of parameters for the two experiments are given in the table below.

Parameter	O2	O4	Explanation
slip	-1	-1	+1 for free-slip, -1 for non-slip boundary conditions
visco2	0.10	0.07	deformation-dependent Laplacian viscosity factor
visco4	0.00	0.00	deformation-dependent biharmonic viscosity factor
veldf2	0.03	0.00	diffusion velocity (m/s) for Laplacian momentum dissip.
veldf4	0.01	0.00	diffusion velocity (m/s) for biharmonic momentum dissip.
thkdf2	0.00	0.00	diffusion velocity (m/s) for Laplacian thickness diffus.
thkdf4	0.01	0.00	diffusion velocity (m/s) for biharmonic thickness diffus.
temdf2	0.015	0.005	diffusion velocity (m/s) for Laplacian temp/saln diffus.

Table 1. Values and explanations of parameters chosen for the two simulation experiments.

1.2 Sensitivity tests of viscosity

Given the possible impact of viscosity on boundary currents, I think it is necessary to test the sensitivity of your results to viscosity. It may well be that the improvements are partly due to modifications of this term instead of the higher order advection. I suggest modifying the viscosity parameters (typically multi-

ply/divide it by two) in both the 2nd and fourth order simulations.

The viscosity parameters have been carefully adjusted at the lowermost acceptable value. Lower viscosities in the 4th order scheme cause the model to crash (by violation of the CFL conditions) and in the 2nd order scheme they exhibit unphysical numerical noise. Higher viscosity values have no physical justification and deteriorate the results, for example they tend to strengthen artificially the recirculation north of the Agulhas Plateau. Our recommendation is to use the minimum viscosity that gives physically acceptable results, and not to use higher viscosity values.

1.2.1 Explain the opposing conclusion to Webb (1998)

Initial tests of the influence of high order advection for momentum where done by Webb (1998), who concluded it has no positive effect. Winther et al (2007) conclude exactly the opposite and you too. Some explanations have to be given: is the fact that Webb tested this in equatorial regions the explanation? If yes why?

We wanted to apply the split-QUICK (SQ) scheme to the advection of momentum for improving potential vorticity dynamics at grid sizes inferior to the Rossby radius of deformation, as justified in section 2.1 of Winther et al (2007). The results from Webb (1998) are not in contradiction with ours since they also note a better penetration of the Agulhas Current with advection of momentum by the SQ scheme. The most striking feature in the Webb (1998) standard O2 scheme is the abundant numerical noise, but we cannot state with certainty what could be the cause for it.

1.3 Analysis of vorticity fields

Winther et al associate this effect to a better representation of the vorticity dynamics. Is it the explanation here too? you have to add some comparison of the vorticity fields in your paper. When studying processes where vortex have a major influence, analyzing vorticity is, in my opinion, necessary.

Agree, an analysis of the instantaneous vorticity fields after a given period will be included.

<u>2 Minor comments</u>

2.1. The QUICK scheme is actually equivalent to a fourth order advection term and a bi-harmonic viscosity whose coefficient is a function of the velocity magnitude. Webb showed that this is the term that imposes the predictor/ corrector approach in the QUICK scheme and his analysis allowed to define a numerical scheme that is equivalent to the latter, but fully explicit without the need of a predictor/corrector approach. This is the version that has been retained in Winther et al 2007 (with some modifications of the viscosity term), and if I understand correctly your introduction, the version that has been retained here too. 1/ So the scheme you use is not the QUICK scheme and you should correct your manuscript (use simply "high order scheme"?).

Agree. Winther 2007 in fact also refers to “higher order scheme” or “fourth order scheme” rather than to “QUICK scheme” in their paper.

2.2. P 432 your quotation of Barnier et al (2006) is a bit misleading. We understand you use this reference to justify the fact that higher order advection is necessary for momentum. I think the modifications of advection scheme proposed in Barnier et al is not higher order: it remains 2nd order but they use a conservative approach (which is what exists in HYCOM). Close to boundaries, such a scheme can even become 1st order. 2/ Please Check their paper and correct your paragraph if I am correct.

Barnier et al (2006) present two distinct momentum schemes: (1) referred to as the ENS scheme based on Sadourny (1975) which conserves enstrophy in non-divergent flows, and (2) referred to as the EEN scheme adapted from Arakawa and Lamb (1981) which conserves total energy for general flow and potential enstrophy for flows with no mass flux divergence.

Although we do not explicitly claim that higher order advection is necessary for momentum, the way it is phrased here does make it sound like Barnier et al (2006) applied higher order numerics in their improvements of the model numerics.

The citation has been removed in this instance.

2.3. I suggest you add some precise indications of the computational cost of the low and high order HYCOM.

Regrettably, the files which record the computational load have been lost / overwritten, and unfortunately from a computational point of view it is not possible to rerun the experiments for obtaining two numbers only. Additionally a precise indication of the computational cost of the different schemes would require that the computational load of the computer remain constant, which is not always the case with number of users varying on a daily basis.

2.4. P. 438, lines 7-9 : This is hardly visible in Fig. 3, how do you measure the southwestward penetration?

We refer in particular to the change of the 20oC isotherm, which extends approximately 390 km further southwestward in A3X compared to A2X. This is a qualitative measure of the southwestward penetration of the Agulhas Current.

2.5. P. 440 : Your analysis of the vertical section is very interesting. I suggest you also add vertical sections of stratification (density and/or temperature).

We have emailed Professor Bryden to ask if he could make the gridded temperature (and/or density) data available for inclusion in our discussion.

2.6. P. 440, lines 17-20: Also explain what you mean by lack of vertical resolution (what resolution do you have in this area)?

Our model uses 30 vertical hybrid layers throughout the domain. The fact that current velocities of 10 cm/s extend all the way to the bottom in both simulations, while the

mooring data from Bryden et al (2005) suggests a level of no motion at 2200 m, is attributed to too few vertical layers in the model.

2.7. P. 444, I was puzzled by the fact you use SSH as a measure for energy. Why don't you use EKE instead? Also you mention Fig. 6a represent EKE, which is not true, is it?

We agree that the model EKE is more precisely calculated from the variance of velocity but we rather chose an SSH-based proxy for consistency with the satellite altimeter EKE estimates. In practice the pattern described by the standard deviation of SSH is very similar to EKE. This holds true for both model output and altimeter measurements.

Agree. Fig. 6a does not represent EKE, but rather its proxy.

2.8. P. 445, line 11-12 : not so clear in your figures, explain?

Agree, this is a rather vague comment, will be explained in more detail.

<u>3 Trivial comments</u>

3.1. P. 431, lines 17-20: I do not understand your justification of the interest of density coordinates: PE models are all 3D. Do you mean you do not have to calculate the vertical velocity? What advantage does this represent?

Bleck (2006) in Chapter 4 of Ocean Weather Forecasting discusses the rationale for building hybrid-coordinate OGCMs. He argues for transforming the equations that constitute the ocean model into a coordinate system that exploits some symmetry or conservation laws inherent in the underlying physics. Such an approach would lower the truncation error in the finite difference equations, with the aim of improving the accuracy of the numerical solution.

Bleck (2006) explains that adiabatic motion follows surfaces of constant entropy or its proxy, potential density. Therefore, when using potential density as the vertical coordinate, adiabatic flow that is 3-dimensional in Cartesian space is rendered 2-dimensional in potential density space. Following this, it becomes easier to satisfy adiabatic constraints while modeling lateral transport of tracers, including temperature and salinity.

The principle difference between isopycnic and Cartesian coordinate models is that depth and potential density trade places as dependent and independent variables. It remains that both model types solve the same physical problem, but because they are based on different sets of differential equations, their numerical properties can be expected to be different as well.

3.2. P. 442, line 4 : what is a Q-Q plot? Difficult to understand if it is not shown ...

Removed comment about the Q-Q plot.

3.3. P. 443, line 11 : the mean SURFACE circulation.

Included.

3.4. P. 454. line 23 : "of the of"

Edited.

3.5. P. 455, line 3 : "In addition [TO] this Fourier transform relies ..."

In addition to this the Fourier transform relies on the ...

3.6. P. 455, line 20 : " was chosen"

Edited.

3.7. Fig. 5 : I suggest you keep the same colors for O2/O4 as in Fig. 2 (reversed here).

Made HYCOM O4 red in Fig. 5.

3.8. Fig. 6 : the black lines are hardly visible, modify the colorbar (to have white instead of blue) or make two plots.

Agreed.

These comments will be included in the updated version of the manuscript and will be submitted in due course. Thank you for your constructive review of our work.

Best regards,

Bjorn Backeberg et al.

Interactive comment on Ocean Sci. Discuss., 6, 429, 2009.