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Interactive comment on "Formulation of an ocean model for global climate simulations" *by* S. M. Griffies et al.

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... continuation from "1. How can we judge the GFDL model?".

2.0 Detailed Comments

2.2.1 Tripolar Grid

This is a case where a useful comparison with the previous IPCC model can be made. Has the new grid really reduced smoothing or Fourier truncation effects in the Arctic? If so presumably it also affects the ice physics. What effect does the tripolar grid have on the timestep?

2.2.2 Horizontal Grid Resolution and 2.2.3 Vertical Grid Resolution

Both sections show that in this model emphasis has been given to the equatorial wave

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guide, presumably to produce better equatorial dynamics a better El Nino signal. The fact that the climate change signal sometimes looks like an enhanced El Nino may have contributed to this decision.

This sounds fair enough but is the equatorial ocean really better represented? Is the undercurrent more realistic? Is the El Nino variability better? (Also if a climate model designed to give good El Nino variability produces an El Nino like climate change signal, where is the catch?)

The downside of concentrating on the equator is that it is done at the expense of other features such as the mid-latitude thermocline and the thermohaline circulation. The question of ocean model resolution at mid-latitudes is really a major problem with climate models and needs a bit more discussion. What quantitative statements can be made about the realism of the thermohaline circulation? Is it completely satisfactory? How much can one believe rapid changes in the circulation?

I am also a bit surprised about the comments to do with sea ice. Freezing at the surface may increase the volume of sea ice in the top level, but is should not affect the pressure deeper in the water column, so the sea level in the leads between the ice should not change. Why then all this stuff about the pressure of sea ice? Isn't this just Archimedes?

I can see why there may be problems with the tracer equations as the volume of the top cell is reduced. However if the levels are 10 m thick then even in the Southern Ocean where sea level drops by 2 m the code should be able to handle more than 4 m of sea ice.

2.2.4 Bottom Topography

The paper emphasizes the present problems in generating a global ocean depth dataset. To give readers more confidence I suggest that you publish the topography dataset with the final article so that anyone concerned can plot the data themselves.

The failure of the overflow scheme is disappointing - but (see below) this may be the result of using a very diffusive upwind advection scheme. However it does emphasize the fact that the present paper could do with a more detailed discussion of the overturning stream function and meridional heat transport - including comparisons with previous models and observations.

2.2.5 Equation of State

I like the improvements but instead of statements like "This approach has limitations which are no longer acceptable ..." or " ... more than adequate for ocean climate purposes", the section really needs some hard numbers say comparing the errors arising from the old and new form of the equation.

2.2.6 Tracer Advection

One problem not mentioned here is the numerical mixing introduced by internal waves in a z-level model. Sigma coordinate models have the same problem. If the ocean is coupled every few hours to the atmosphere then as well as strong inertial oscillations there is likely to be a strong internal wave field almost everywhere.

If central differences is used for vertical advection then you can generate short wavelength noise as the water is advected upwards but this is canceled out when the the water moves back down. If you use a higher order scheme with any diffusive or upwind terms (in some ways they are equivalent) then, although the errors may be smaller, this cancellation does not occur. As a result internal waves can produce enhanced 'numerical' vertical diffusion.

So the question is - have you checked this out? It is possible that I have missed something but any upwind scheme is likely to produce a similar result. If it is present then it will affect any tests you do on vertical diffusion.

2.2.8 Background Vertical Mixing Coefficients

There is quite a lot of work here but the discussion is mainly qualitative - even figure 8 shows only the difference between two runs, it does not show absolute errors. I think this is another case where it would be good to show the latest scheme has improved over the last IPCC model.

2.3.1 Ocean Free Surface and Freshwater Forcing

I was surprised with the length of this section. It is of moderate interest to see what changing to a free surface does to the GFDL model but maybe my surprise arises because I did not know that "tracer budgets in many (climate) models still assume that the ocean volume is constant". Is this really true? (the change involved is relatively trivial). At page 185, line 22, why do you not name any of the models?

2.3.2 Time Stepping the Model Equations

I have a number of problems with this section. First most ocean models do not use leapfrog time stepping. Instead the most basic form used is a combined leapfrog and forward time stepping scheme in which leapfrog is used for the advective and wave/oscillatory terms and a 2*dt forward timestep for any terms involving damping (i.e. involving viscosity or diffusion). As is well known, (for example Mesinger and Arakawa, 1976, GARP Publication Series No 17) the advection and wave terms are stable with leapfrog but unstable with forward time stepping. Conversely the diffusive terms are (usually) stable with forward but unstable with leapfrog. If there is any chance that the diffusive terms are unstable with forward, because of the very short diffusive time scale, then an implicit scheme is used, as with the F terms in equation 8.

Equation 8 is wrong because it shows the advective term as a forward timestep. The reason why h and T are filtered and why the filtered versions are used in one term on the l.h.s. needs explanation. What does "Sect. 19" refer to?

At first sight the new time stepping scheme is attractive, giving similar accuracy for waves and diffusive processes but at half the computational cost because the 'maximum stable timestep' can be doubled (note: the authors should not call this the 'stability' as the word already has a good scientific meaning). Diffusive processes use a 2*dt timestep anyway so no change is needed and, for baroclinic internal waves without a Coriolis force, the terms contributing to pressure (temperature and salinity) are only needed at the odd timesteps whereas the velocity terms are only needed at the even timesteps.

Problems arises with Coriolis term, discussed below, and the advection, for which leapfrog needs the variable value (T in eqn 9) at the central timestep. If the earlier timestep is used for advection, as has been done with the GFDL model (see eqn 9), then the timestepping scheme becomes a forward timestepping scheme and is unstable. This is most easily shown for the case where there is a steady current, so U is constant, but the scheme will also be unstable for all cases where the mean current at a point is non-zero.

The usual way to control this is to introduce damping terms large enough to kill of the most unstable wave. With the GFDL model the flux limiter used with advection will help to control the instability - but probably only to the extent that sinusoidal wave will become a square wave. My suspicion is that extra damping has also been introduced, either by using upwind finite differencing in space (which introduces a large numerical diffusion term) or by adding extra explicit diffusion or viscosity. Either way instead of the new scheme producing a better long term model behaviour, as the authors believe, my guess is that although it speeds up the calculation, it reduces model realism. But as discussed above, with climate prediction models, model realism is a key issue.

I may be wrong but if so the paper needs to give enough detail to prove the point.

Implicit and semi-implicit terms.

The paragraph on the implicit terms is misleading. The reason an implicit scheme is used is because the time scale for the diffusive processes to reach equilibrium is very short. The normal scheme, leapfrog, forward, etc, is used for the long timescale processes because it is their average effect over the whole timestep that is important.

The implicit scheme is then used for the short timescale processes because the effect of such processes early in the timestep has little effect the final balance. The net result is the calculation of a realistic value of the field being calculated (tracer, velocity etc) at the new timestep allowing for all processes affecting the system.

I would therefore argue that the phrase about the 'implicit portion leading the explicit portion' is meaningless. Also the fact that one scheme uses a double timestep and the other two single timesteps does not make the first more accurate. Finally the only way that the long timescale scheme can effect the implicit scheme is through its own accuracy - and as discussed above the new scheme is likely to be less accurate.

The Coriolis terms need special treatment because of the way they can produce wave like features, (i.e. inertial oscillations), by exchanging energy between the different components of the velocity field. A leapfrog scheme can be used but it needs the velocity at the central timestep. However because, with the Arakawa-B grid, only the local grid point is involved, it is possible to replace leapfrog by what Mesinger and Arakawa call the trapezoidal scheme and the authors call a semi-implicit scheme. Like leapfrog this is neutrally stable for waves.

The authors claim that both the semi-implicit and 'a time explicit implementation' (their new scheme?) are second order accurate. For the semi-implicit scheme, the amplitude has no error but the phase probably has errors proportional to the square of the timestep. But the new scheme, if they have used a forward timestep as with advection, will not just have second order errors - it will be unstable. So I suspect that the correct statement is that the new timestepping scheme is unstable with inertial oscillations and that the semi-implicit scheme is better because it is neutrally stable.

Sensitivity to the Time Stepping Scheme

A number of the comments made above also apply to this section. The problems are related in that there is not enough analysis of the timestepping schemes, or at least a discussion of their properties, and too much reliance on inconclusive model tests and the personal preferences of the authors.

In the present section one example is the statement "We prefer the predictor-corrector ... because of its increased stability relative to the leapfrog and well as its ability to dissipate grid scale noise ... ". Elsewhere 'stability' is used to mean 'allows an increased timestep' but I presume here that you really mean stability - i.e. the known property of a predictor-corrector schemes to add numerical damping to a system. Although it may compensate for other unstable terms in the timestepping scheme it will also degrade the realism of the model - so without further justification it cannot be considered an improvement over a scheme which freely propagates waves without changing their energy.

I agree though that there is a problem with short period gravity waves on a B-grid where the Coriolis terms are too small to couple waves on the black and white grid squares. Adding a time dependent term to remove the waves may be a solution, but the statement really needs to be made explicitly and the amplitude of the numerical damping term justified.

Comments on Time Stepping Schemes

This section is not really concerned with the model so I am not sure why it is here. I agree that the time splitting mode is unsatisfactory but for the reasons given above am unconvinced by the emphasis on 'new' schemes. As far as I can tell, all of the low order timestepping schemes, such as the ones discussed here, were probably first investigated in the 1950's or earlier. People like Mesinger and Arakawa then summarized the properties of the schemes they thought were best suited for atmospheric and ocean models.

The argument about computational fluid dynamics has similar problems. From the beginning the CFD codes almost always used two timestep schemes. This is because with the early computers memory requirement was always a key issue. As a result when people modelled the energy cascade of a turbulent fluid, the extra memory re-

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quirement of the leapfrog scheme was a serious defect whereas its main advantage, the ability to propagate waves without damping, was not really needed.

2.3.4 Horizontal Friction

The step changes in the viscosity coefficients of fig 15 seem extreme. Can you show that the changes had no significant effect on the velocity fields? Was no attempt made to smooth the transitions?

I am surprised that the transport through Bering Strait was affected by changing horizontal viscosity. The shallow flow is normally thought to be controlled by bottom friction. If horizontal viscosity is found to be significant the values must be unusually high.

2. Appendix A

I am not really sure why this is here. As an introduction to the model equations it is not particularly clear and it has a number of idiosyncrasies - for example the use of the term 'advective metric frequency' to describe something that is not measured in cycles per second. It is also confusing. For example after equation 21 it states, without explanation, that the semi-implicit approach is second order accurate, implying that there is something special about being second order accurate.

Replacing alpha in eqn 21 by 'a', where different alphas correspond to different timestepping schemes, and writing the square root of -1 as 'i', then the amplification of a wave with angular velocity 'w' each timestep 'dt', is given by:

L = [(1 + ip - a(1-a)p**2)/(1 + ((1-a)p)**2)] /exp(ip)

where p = w dt and p**2 indicates the square of p, and exp(ip) is the correct time dependence of the wave being modelled (see Mesinger and Arakawa). Thus,

L = 1 + O(p**2).

This shows that the timestepping scheme is correct to second order whatever the value of alpha. For the semi-implicit scheme, for which alpha equals 0.5, it is not the fact that

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L is second order accurate that is important but the fact that its modulus is always equal to 1.

3.0 A different Leapfrog-Forward Scheme

I don’t want to defend the standard leapfrog-forward timestepping scheme if someone can find something better but the present work has highlighted an important point concerning the scheme. This is the fact that the timestepping solution can conveniently be split into two ’threads’, one involving tracer values at even timesteps with velocity values at odd timesteps, and the other thread in which the roles are reversed.

Each thread only needs the other for the advective terms and, if the semi-implicit scheme is not used, for the Coriolis terms. It therefore seems sensible to treat one of the threads as the ’best’ and the other as the ’workaday’ thread used just for these extra terms when solving for the ’best’ thread.

This approach would affect the way models are restarted after a break. It would also affect the way filtering is used to prevent time-splitting as filtering would only be used for the workaday thread so that it did not directly affect the ’best’ solution. If instead a forward-backward timestep is used occasionally to reduce time-splitting then it would only be used to produce a new workaday thread from the best thread, i.e. used at timestep 2nm for the tracer fields and timestep 2nm+1 for the velocity fields (instead of both at timestep 2mn), where 2n is the interval between forward-backward timesteps and m is an integer.

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