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Geoscientific Model Development Discussions

Interactive comment on "Prospects for improving the representation of coastal and shelf seas in global ocean models" *by* Jason Holt et al.

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The manuscript discusses aspects of coastal versus global ocean modeling. Its basic conclusions rest on estimates of ocean scales. These estimates are of interest, but they are insufficiently deep. The technical side of the problem of matching coastal and global scales in a single setup is addressed from the side of mesh resolutions. Many physical aspects are mentioned, but without practical recommendations.

While it is true that a precise boundary between coastal and global scales does not exist and a lot will be possible as resolution is further refined, there are even smaller scales or physical processes resolving which in a global setup will make the global approach strongly suboptimal (for example, regimes with wetting and drying). So I see the technologies based on two-way nesting or unstructured meshes much more

promising and requiring much less computational resources in many coastal tasks. I would recommend to define more precisely the context of global coastal modeling. I would also recommend to put more focus on specific issues such as the reduction of spurious mixing on terrain following meshes as suggested by Lemarie et al. (2012, Ocean Modelling), the Arbitrary Lagrangian–Eulerian vertical coordinate (MPAS and MOM6), scale-aware mixing and eddy parameterizations, measures influencing numerical stability if the intention is to really discuss the prospects.

The analysis of future perspectives based on the evolution laws for the available computational power is a bit superficial. The point is not only the computational power on its own, but also time step limitations. The manuscript considers additional resources needed because the time step will decrease on finer meshes. However, if the scalability stays as its present level, the codes will become slower in terms of simulated years per day of computations. If a 1/72 degree model runs slower by a factor of 6 (because of the time step reduction) than a 1/12 one, the question is how many days it will take to run such a model over a cycle of CORE-II (or any other) forcing which is a bare minimum for global simulations. Any prognosis remains vulnerable to the issues of scalability and parallel efficiency which are difficult to estimate. The key question is which technologies can improve scalability and how, and without answering it the rest is just an exercise. Running 1/10 or 1/12 degree models is affordable today to many groups, but such models remain too slow for exploring climate over large time intervals.

The view on unstructured meshes proposed in the manuscript seems to be outdated and is based on just arbitrary comparison for a particular model (FVCOM). Here, the point is better parallel scalability of current unstructured-mesh codes on large meshes which warrants comparable throughput (but with somewhat larger demand to computer resources per degree of freedom). Furthermore, unstructured-mesh codes deal only with wet nodes, which creates noticeable economy in coastal areas. For example, even with FESOM1.4, which is relatively slow because of its 1D arrays, one reaches throughput of about 8 simulated years per day on global locally eddy-resolving meshes that are as large or larger than the 1/4 degree mesh (see Sein et al., 2016), which is not worse than the performance of regular-mesh models on 1/4 degree meshes. New developments such as MPAS, FESOM2 and ICON are a factor 2 to 3 faster than FESOM1.4 and can compete with structured-mesh models in massively parallel applications.

If the coastal area occupies less than 10%, running an unstructured-mesh model or model with two-way nests on a mesh that is three times coarser in the global ocean than in the coastal part is 5 times less expensive (in terms of computational resources) than running this model on a global fine mesh. Such arithmetic is trivial and hardly tells in favor of global fine meshes; devoting a significant place in the manuscript to all the would be issues is not very appropriate (for it depends on the slowness factor of 5 that the authors took for unstructured meshes).

Based on these remarks I would recommend a major revision. I would be happy to see more substance. The manuscript presents a view, but does not formulate solutions which respect of numerics of physics one expects to see in a GMD paper.

Below are more specific comments.

Background and motivation:

The contents of 1.2 and 1.3 are a bit shallow to warrant their publication. 1.3 fails to convey a message that downscaling cannot be done with nesting, for one just needs to take a (two-way) nest of appropriate size. Examples of 1.2 are well known, but the point is not only the resolution in the ocean, it is also bottom representation, spurious mixing etc. So it is not only upscaling. The manuscript does not really discuss measures needed for seamless representation of particular processes. This all leaves me wondering why 1.2 and 1.3 are needed at all? Most readers of this journal already know it, and I would omit them.

Page 3, lines 3-5: The sills between the Nordic seas and the North Atlantic are not as deep as 1500-3000 m. So the NADW is very indirectly connected to what is said.

Page 4, bottom, the first and third issues are rather close. Page 5: top, rivers are also accounted for in global models Fast ice is accounted (through parameterization) in some large-scale models; it is not a strong point of most coastal models.

The beginning of 2.1: To compute the scales one does not need a NEMO grid and can use the original data instead. So you need it to put the scales into the context of particular model. There is no issue of North Pole singularity for scalar data.

Page 6: If L1=c/f, where c is the speed of the first mode, then for the Eady instability wave the wavelength of most unstable wave is approx. 3.9 \pi L1, giving different size of eddies.

Page 8, top: Mentioning barotropic tidal models is hardly relevant in the context of 3D modeling. Instead of reviewing who did what it would be much more appropriate to formulate what is the essence of difficulties and only then discuss the current status.

Discussion of spurious mixing in the context of tides sounds strange to me — according to Ilicak et al (2013) it is the grid scale motions that have the largest impact. The point of spurious mixing deserves much more attention, for it is also related to terrain following coordinates which intersect isopycnals at some angle and introduce spurious mixing precisely where they are most needed (the continental break boundary). The manuscript mentions it, but not the measures needed to overcome the difficulties.

Page 9: Transition to terrain-following coordinates in shallow water: This option is available in some other models (SELFE, FESOM) from much earlier date.

The message of Figure 5 is not very clear. It does not show why or which technology should be selected.

Page 10: "Mixing of temperature and salinity usually takes place along isoneutral, rather than geopotential, surfaces and this requires careful implementation in the case of sloping vertical coordinates." What about transport algorithms with upwinding or limiters ? Most coastal applications will need upwinding and limiters.

Line 20: The simplest is of course scaling with the horizontal mesh cell size for harmonic and cube of that for biharmonic operators as is routinely done in most codes. Discussion mixes horizontal (Smagorinsky) and vertical aspects. Scale-selective approaches deserve more attention. In most cases the horizontal subgrid operators just aim to remove the grid-scale variance, which is far from physically motivated solutions.

Line 25: "Quadrilateral meshes approximate coastlines by imposing zero-normal flow condition on specific edges of the mesh and masking the landward solution." — I think all meshes do the same in this respect.

The representation of coastlines should not be a big issue on terrain following quadrilateral meshes if the depth tends to zero. On the other hand, on triangular meshes smooth coastlines do not really help unless terrain-following meshes are used, for boundary of any layer should be smooth. If wetting and drying is present, then again it is the bottom representation. So there is general problem of bottom representation, and the example of Kelvin wave is in essence related to it. The representation of coastlines is a part of this general problem, and cannot be considered separately.

Section 3.2. ORCA meshes assume certain scaling (largely with latitude), so together with calculations for them it would be of interest to present a mesh-independent calculations: you have your pattern of the smaller of the baroclinic, barotropic, and topographic scales and can compute distribution of mesh nodes over scales for just resolving and for resolving with two points per scale. This is more informative for unstructured meshes. A delicate question is the behavior of time step with resolution, which needs some extra discussion. ORCA meshes partly account for the reduction of the phase speed of internal waves with latitude, but other processes may be (locally) limiting at fine scales. The discussion of the number of mesh points looks unsatisfactory (it is elementary) to me if not augmented by time step analysis, at least on a qualitative level. Scale L_min may imply different time step selection at different locations, so the question is what is the optimal strategy.

Section 3.3.

Page 11, line 40: In reality hybrid approach is used by all these models as concerns the computation of pressure gradients. It is also true for many other models.

Page 12: Line 1: I think the authors derived a wrong message. There is no point with formal accuracy on unstructured meshes, in fact smooth triangular meshes are more isotropic than quadrilateral and will be more accurate. Computational modes indeed require attention, but there are solutions how to handle them. The technology is mature enough, it requires more caution.

Line 3: Wave propagation on quadrilateral meshes has no advantages. Quadrilateral meshes are simply cheaper for there are less edges, which is crucial for finite-volume codes.

Line 10: "The former ....but have not yet reached ...." Please be careful, for the statement is wrong. FESOM, for example, was a part of CORE-II intercomparison (see the virtual special issue of Ocean Modelling), it is a component of AWI climate model (see Sidorenko 2015, Clim. Dyn), and in this way participates in CMIP6.

I think that the unstructured-mesh part of this section is weak and does not convey a correct message to community.

Section 4.

The discussion proposed in this section misses some important points. If the clock speed peak is already reached, the throughput of codes on fine meshes will decrease on mesh refinement. This aspect is not less practical than the availability of computational power and has to be mentioned. The authors state "This requires at least three-way nested parallelism ..." Are there solutions allowing to efficiently work with smaller mesh pieces per core? A perspective in this direction should be addressed. The other aspect is structured vs unstructured codes. In finite-volume unstructured-mesh codes the neighborhood information is two-dimensional, so it is related to the vertical column of computational points and accessing it is not expensive. Computations of high-order advection or gradients are more expensive than on sttuctured meshes, but if memory bandwidth is a limiting factor, the need in more computations will be less apparent. Furthermore, mesh partitioning is easier (it is derived from the connectivity pattern and involves only wet nodes). I am not sure whether discussing all this is possible in this manuscript, but proposing some discussion would provide a much more valuable message to the community.

The statement on page 15 "Hence, unless very efficient methods of multiscale modelling are developed ....." can be critisized, and the extent of this depends on what we define as efficiency and what oceanographic task we are considering. I would not do here, but only note that multiscale modeling methods can be (and are) much more efficient than assumed in the manuscript.

I think Fig. 10 is not really necessary, for much more work is needed to explore functioning of such meshes for eddy-rich dynamics. There is no substance at present. It is in contrast to numerous other efforts on triangular meshes which are barely addressed.

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