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Interactive comment on "Experimental and diagnostic protocol for the physical component of the CMIP6 Ocean Model Intercomparison Project (OMIP)" *by* Stephen M. Griffies et al.

F. O. Bryan (Referee)

bryan@ucar.edu

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This manuscript provides an in-depth and lucid description and justification for the physical ocean quantities that are requested for the CMIP6 archives, as well as the experimental protocol for the OMIP component of CMIP6. The later is relatively brief, but supported by the recent CORE-II analysis papers. The overwhelming majority of the material is devoted to the former. It is my understanding that the protocol and variables lists have been negotiated through various international panels and CMIP governance processes, and are not really open to criticism in this review of the manuscript. Rather, I have been directed to focus my review on issues of clarity of presentation. that said, in the comments below, I do question some omissions.

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Discussion paper

Overall, the manuscript is an outstanding piece of work and reflects the depth of expertise represented in the author list, and especially of the first author. The level of detail makes the manuscript rise to a level that can serve as a standard reference in ocean modeling. I expect that it will be used by the ocean and climate modeling communities for purposes well outside and beyond the scope of CMIP6.

I have only one comment of substance. This is in regards to the issue of remapping the output. The authors make the case that for the purpose of comparative analyses, it is critical to have the output remapped to a common grid, and they encourage the use of a standard 1 deg. grid commensurate with that used in the World Ocean Atlas (notably, this is coarser than the native grids of many CMIP6 models). I have no fundamental objection to this position. Where my concern arises is the equally strong emphasis of the authors on being able to diagnose and test for exact conservation of mass, heat, salt, etc. In the presence of complex topography, these two objectives run counter to one another. In particular, any remapping will necessarily require the definition of new land-mask and topography fields, inevitably different from those of the native model grid. It is not trivial, and perhaps not even possible, to retain conservation when the surface area and volume of the ocean differ on the two grids. Perhaps the authors envision use of partial cell type ideas to recover such properties from both grids. However, there is ambiguity in how the partial cell volumes and areas should be partitioned between surface area and cell thickness/mass. Indeed, for the case of variable cell thickness/mass models, I wonder if all remapping (vertical + horizontal) would need to be done online to guarantee conservation? If the authors are going to push for these twin constraints, then they need to provide more complete guidance on how they are to be mutually satisfied. At several points in the manuscript they defer to the unpublished Balaji et al manuscript (which I did not have access to), but I have my doubts whether this problem will be adequately addressed there.

Detailed Comments:

pg 7, line 9-10: WOA13v1 or WOA13v2 ?

pg 7, line 22: “an implied surface temperature restoring” a matter of semantics, but I think it would be better said as “a negative feedback on SST anomalies”

pg 9, line 21:-23: I would argue that for more complex manipulations, the analysis needs to be done on the native grid with the analysis output targeted to the common grid

pg 10, line 23-25: This is not clear. You are convolving temporal variability with spatial sampling. The spatial covariances of subgrid-scale (on the target grid) structure needs to be properly accounted for.

pg 11, line 3-4: See above. A locally conservative remapping of a variable does not necessarily guarantee global conservation if the global areas or volume change.

pg 12, line 3: what happens to native grid levels with depths greater than the 5500m max depth of the Levitus grid?

pg 12, line 23: did any of the final fields meet this criteria (I can’t find any)

pg 10, line 9: This would seem to apply to all variables on the remapped grid

pg 19, line 8-10: This seems like an inconsistent level of detail. The region mask is going to be blurred on the remapped domain, and remapped staggered quantities will likely sit on the edge between to regions.

pg 19: section 4.9 This section needs considerable expansion to deal with the issue in my general comments

pg 22, line 25-28: How is vertical staggering to be handled with respect to recording cell volumes or thicknesses?

pg 27, line 7: “are not trustworthy” This is a bit presumptuous - the CMIP6 land ice models have not yet been assessed.

pg 31, line 5: “a measure of simulation drift” will also include a component of true,

forced low-frequency variability

pg 33, line 5-8: What is the rationale for diagnosing sub-daily variance in SST? Why not in surface velocity, SSH or SSS?

pg 40, table 3: why is wo (vertical velocity) excluded?

pg 42, line 16-17: only for the _steady_state_, rigid-lid, Boussinesq case

pg 44, line 15-16: “comparable to the model native grid” Why was the same specification not included for overturning streamfunction? Presumably this is the prescription for the “native” resolution with a further decimation to the spherical grid resolution?

pg 53, line 10: Why is river runoff prescribed as a surface (XY) flux rather than a lateral flux?

pg 60, line 17: Figure 1

pg 62, Table 9: Why is wind work excluded. Several variables related to energy dissipation are included.

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