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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.***

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Received and published: 27 June 2016

We sincerely thank Frank Bryan for his comments and for his encouragement. Here are our responses.

## **Reviewer comment:**

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

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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.

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.

#### **Author response:**

Many thanks for your encouraging comments. We too hope that this manuscript will be of use for the broader ocean and climate modelling communities, even beyond CMIP6.

#### **Reviewer comment:**

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

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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.

#### **Author Response:**

We were originally hopeful that a robust remapping method could be identified during the many months developing this manuscript. Alas, no such tool is available or has been championed. We therefore conclude, as suggested by the reviewer, that it is not sensible to strongly encourage regridding Priority=1 output to a spherical grid. We have taken a more modest perspective into the revised draft, and modified the text where appropriate (see in particular Sections 3.1 and A3).

The new draft continues to acknowledge the dilemma facing the community (i.e., ease of analysis without compromising data integrity). We now, however, more strongly emphasize native grid diagnostics as the common element to be saved across all model submissions. Regridding, if desired, will be facilitated by area weights requested for the grids through the WIP contribution to this special issue (Balaji et al (2016) in prep). We now make these points more clear in the revised draft (see Sections 3.1 and A3).

#### **Detailed Reviewer Comments and Author Responses:**

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

→ WOA13v2 is now noted.

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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”

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→Agree; wording changed as suggested.

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

→Text is now fully rewritten, with new version consistent with this comment (now in Appendix A3).

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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.

→We agree. The questionable text has now been removed given re-focus on native grids, and computation of products online rather than offline.

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.

→Agree. We now recommend saving native data, with new discussion noting problems with conservation when regridding.

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

→This is indeed an ambiguous situation. We offer no firm recommendation.

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

→Agree; this is an obsolete point, now removed in revised draft.

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

→Text has been removed in revised draft.

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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.

→Agree for the sphere, but useful if native. Point is now clarified.

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

→As noted above, we are no longer emphasizing spherical regridding. Instead, we are relying on Balaji et al (2016) to provide details for the area weights required for regridding. These details are best presented in Balaji et al (2016) since the WIP is coordinating that work.

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

→We are only asking for the tracer cell thickness, so no need to consider vertical staggering between velocity and tracer.

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

→Agree; text is now modified to reflect this point, and to acknowledge that some groups may have an ice sheet model. We also point to the ice sheet model comparison project now part of CMIP6 (Nowicki et al. 2016).

pg 31, line 5: “a measure of simulation drift” will also include a component of true, forced low-frequency variability.

→Agree, with text now added to reflect this point.

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

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→ We presently request *tossq* ( $SST^*SST$ ) on daily and monthly time sampling. The daily sampling is requested to help diagnose Tropical Instability Waves (TIWs). We have not had requests for similar sampling of SSS or SSH, although note that daily mean SSS is requested.

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

→ This was an oversight. This field is now been requested in Section J4.

pg 42, line 16-17: only for the *steady state, rigid-lid, Boussinesq case*

→ Agree, and corrected.

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?

→ This is admittedly imprecise; it remains unclear what is the best and most practical approach. Text to this effect has been added.

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

→ Agree; text changed to allow for lateral flux with *friver* now XYZ field in general.

pg 60, line 17: Figure 1

→ corrected

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

→ Good suggestion. As no one has requested it, we did not include it. More to the point, we are not requesting budget terms for the kinetic energy. The energy dissipation terms that are requested are meant to diagnose impact from subgrid scales. As mentioned in the Closing Comments section, we anticipate future CMIPs to address

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more budgets, beyond the new heat and salt budgets requested in CMIP6.

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Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2016-77, 2016.

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