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Interactive comment on "Biogeochemical protocols and diagnostics for the CMIP6 Ocean Model Intercomparison Project (OMIP)" *by* James C. Orr et al.

Anonymous Referee #2

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This manuscript presents a plan for simulations and diagnostics of biogeochemical tracers during the CMIP6 simulations, including carbon cycle and biological tracers. Model intercomparison projects are somewhat unwieldy beasts, when comparing models it is often difficult to know what to say beyond "the models differ". Reasons for this include subtle differences in model construction and parameter values as well as more fundamental issues about which processes are represented. Ideally, model intercomparison projects will try to keep as many things as similar as possible, so as to narrow down the range of possible differences between simulations. The strategy taken in this version of CMIP seems to be to make sure the different models all use the same gas exchange, atmospheric concentration, gas chemistry and carbon chemistry parame-

ters, while using different ecosystem models. This seems a very sensible approach to me.

It would be good at the end of the introduction for the authors to define what the principal scientific goals are. Right now it appears that a principal goal is to quantify the change in ocean carbon inventory under global warming and to attribute this uptake to passive uptake by a changing circulation vs. changes in the natural storage of carbon by biology. It would be good to say a little more about why this is challenging, in particular that the long equilibration time for carbon dioxide means that the (poorly known) disequilibrium component of anthropogenic CO2 is the same size as the actual signal we are trying to detect. As a result, different estimates of anthropogenic CO2 differ by large amounts. Combining C14 with SF6 and CFC12 tracers offers us a way of not only testing the models, but of narrowing the observational uncertainty on anthropogenic CO2. However, doing this right requires not only getting inventories but fluxes right, which in turn requires standardizing carbon chemistry (the recent paper by Lovenduski et al in Global Biogeochemical Cycles would be a good one to reference here as it shows that systematic model bias dominates regional carbon fluxes). This would motivate the discussion later in the paper.

Additionally however, biogeochemical tracers can serve as useful constraints on ocean circulation, in particular the ventilation of the deep ocean, as they average over long periods of time and exhibit strong contrasts between different regions of deep water formation. A great example of this is Broecker et al. (JGR-Oceans) use of C14 and PO4* to derive ventilation rates for Antarctic Bottom Water and North Atlantic Deep Water-the former of which is likely still better than anything that physical oceanographers have been able to quantify directly. Radiocarbon is also useful for getting at upwelling pathways, as different overturning schemas can produce vastly different distributions of surface radiocarbon with very similar overall hydrography (see Gnanadesikan et al., GBC, 2004 for an example of this).

Finally, though, there's the issue of biological variability under climate change. This will be an important area going forward and I do not feel that the diagnostics for it have been properly prioritized. One of the main "consumers" of this work are going to be marine ecologists looking for changes in community structure and ecology. Because of this I would strongly recommend prioritizing monthly 100m-integrated biomass measurements for as many classes as exist in the model as a top priority. I disagree with the reviewer who worried about having too many diagnostics- frankly the field as a whole suffers from having too few diagnostics saved out to actually understand differences. In the current version of CMIP5, for example, many of the models save out minimum oxygen rather than the three-dimensional fields. In analysing this my group is finding that this limits the signals of changes in climate to convective zones, rather than allowing them to be tracked more broadly.

I have only one other quibble about the standards. The OCMIP2 standards for carbon and radiocarbon were generally for models that did not have interannual variability but were being forced towards a mean climate. When climate variability is included (and as noted by de Lavergne et al., 2015 this variability can have large amplitude) one can easily see variability accounting for differences of 5 GtC over the course of a century. I strongly recommend that the authors either raise the threshold for carbon trends or soften the requirement in some way.

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