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Interactive comment on “Inconsistent strategies to spin up models in CMIP5: implications for ocean biogeochemical model performance assessment” by R. Séférian et al.

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This paper examines the impact of different initialization procedures and spinup times in CMIP5 models, the resulting drift, and its impact on model skill assessment. I am delighted to see that finally the issue of spinup times and drift is addressed comprehensively for the CMIP5 model suite. However, I have two concerns or comments, that I think should be kept in mind, and a few minor issues.

(1) As far as I understand, the core model experiment, IPSL-CM5A-LR, was spun up from rest for 500 years. I am aware that it is sometimes quite expensive - in terms of computational cost - to simulate global or earth system models over a long time. How-

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ever, I am not quite sure that a spinup time of 500 years, as used for this experiment, is always sufficient to draw conclusions about the long-term model drift. As has been shown recently (Kriest and Oschlies, 2015; www.geosci-model-dev.net/8/2929/2015/), simulated global average oxygen, nitrate, or total fixed nitrogen can exhibit a non-linear trajectory over time, sometimes with inflection points within the first few centuries of spinup; i.e., the model drift may not only decrease or increase, but change its sign. In practice, it means that, due to the many timescales involved, a model that shows a bad fit and negative trend within the first few hundred years e.g., with respect to global average oxygen, may cease to do so after some more centuries, and finally show a quite good fit to observed oxygen after some millenia.

(2) The above doesn't have to hold for all model types. It can depend on the biogeochemical time scales involved, i.e. on particle sinking speed or remineralization (Kriest and Oschlies, 2015), circulation, and probably other parameters as well. Given that the CMIP5 biogeochemical models involve a huge variety of these parameterizations (e.g., Cabre et al., 2015; www.biogeosciences.net/12/5429/2015/; Fig. 6), together with very different circulations, resolutions, etc., the time scales associated with model equilibration, as well as their transient may be very different, and not always follow linear relationships for the decay term.

Therefore, I would suggest to include some discussion on this in the paper. Overall, nevertheless I think this paper gives a helpful and timely overview about potential limitations of model-model and model-data comparison of this suite of models.

Other comments:

p. 8760, line 27ff: "Oxygen is prognostically simulated using two different oxygen-to-carbon ratios, one for the oxic remineralization of matter and one for the sub-oxic pathway (Sarmiento and Gruber, 2006)." - It is not clear to me what is meant with "oxygen-to-carbon ratios": the ratio of organic matter, or of the process itself? If the latter, how can oxygen be used in sub-oxic pathways? If the former: doesn't this imply that either

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oxygen or carbon is not conserved when switching between these processes? E.g. consider that - implicitly - organic matter built during photosynthesis has a composition according to Anderson (1995, Deep-Sea Res. I, 42(9), 1675-1680), with C:H:O:N:P = 106:175:42:16:1. Of course, one usually does not describe OM in models exactly this way; but the assumption particularly about C:H:O (in some way: the amount of carbohydrates) is reflected in the stoichiometry for O₂ release and CO₂ consumption. If then the C:O-ratio of OM is different between remineralization and denitrification/anammox (whatever is considered), wouldn't this affect mass conservation of either C or O?

p. 8763, subsection 2.3: I would suggest to more clearly define drift, to make this term more easily accessible for users outside the modeling or CMIP5 community.

p. 8773, lines 10-11: "We employ Δ RMSE to penalize the normalized distance from the observations assuming that this drift-induced deviation in tracer fields can be added to RMSE. " - Why choose an additive model?

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