

## ***Interactive comment on “Twenty-first century ocean warming, acidification, deoxygenation, and upper ocean nutrient decline from CMIP6 model projections” by Lester Kwiatkowski et al.***

**Anonymous Referee #2**

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The manuscript by Kwiatkowski and Coauthors discusses results from the latest Coupled Model Intercomparison Project Phase 6 (CMIP6), focusing on the biogeochemical component of ocean change in response to a variety of future radiative forcing (RF) scenarios. The CMIP6 models are further compared with the previous generation of simulations and scenarios, CMIP5. The analysis indicates similar ocean biogeochemical responses to anthropogenic RF as for CMIP5: warming, acidification, deoxygenation and reduction of surface nutrients (i.e., ecosystem “stressors”), with varying magnitudes depending on the scenario, and various spatial patterns underlying interactions between changes in ocean circulation, chemistry, and ecosystem.

C1

The analysis shows that responses in the CMIP6 simulations are very consistent with the responses in CMIP5, with two notable differences. First, the RF scenarios used in CMIP6 imply higher CO<sub>2</sub> concentrations in the atmosphere than the equivalent RF scenarios in CMIP5. This results in stronger C uptake, and thus acidification, for the same level of RF. Second, the climate sensitivity of CMIP6 models appear to be stronger than CMIP5. Thus, for a given RF scenario, the warming response in CMIP6 is overall more intense. This is reflected for example in stronger stratification, the ensuing decrease in O<sub>2</sub> ventilation and surface nutrient supply, etc.

Together with these main differences, the Authors detail a series of changes that were not documented at the same level of detail in the CMIP5 equivalent of this paper (Bopp et al., 2013, Biogeosciences; B13 hereafter). First, they detail responses in the benthic layer, showing similar but muted effects, and larger inter-model variability. Second, they detail changes in the seasonality of specific stressors, focusing on surface warming and acidification.

The results are worth publication. These CMIP6 experiments encapsulates the latest “consensus” of the climate and ocean community on future climate change, and are the result of an impressive scientific undertaking. Results from CMIP5 scenarios have guided much of the research on anthropogenic ocean biogeochemical/ecosystem change over the past decade, and CMIP6 experiments are likely to guide the next round of studies. Thus, there is a need to document the main features of these simulations, and provide a reference for future work. In this sense, the manuscript by Kwiatkowski et al. is needed and welcome. I should add that, by themselves, the results documented by Kwiatkowski et al., are not particularly novel: they mirror results from CMIP5 and previous work, with the appropriate (somewhat minor) differences related to changed scenarios and sensitivities. That said, the Authors strive to add a sense of novelty to the paper wherever possible, e.g. by presenting a few new analyses, and interpreting differences with CMIP5.

As the outcome of a community effort, the results in the paper appear robust, although

C2

it is impossible for a single reviewer to assess the vast amount of information that went into this synthesis. However, all models utilized are presumably documented independently and thoroughly, and I am sure that the literature describing them will continue growing over the coming years, allowing more in-depth evaluation of individual models, or of specific responses.

Overall, the manuscript is informative and well written, and will be useful. The figures are clear, and the main results supported by the evidence provided – with the caveats discussed above. Overall, I am supportive of publication in *Biogosciences*, after the following comments are addressed.

- A comparison with observations is missing. Such a comparison was part of B13, and provided needed background to discuss anthropogenic changes. I suspect this comparison is missing for conciseness, but *Biogosciences* should allow for a few additional figures “for the record”. I also suspect that some of these comparisons will be shown in other papers – although they would be helpful here too. Specifically, it would be useful to see maps of present-day mean properties relating to the various stressors (SST, pH, O<sub>2</sub>, etc.) at the relevant depths, for the best observational products and the CMIP6 model ensembles. This would allow the reader to contextualize the maps of changes. I would suggest adding this comparison to observations for the properties shown in Fig. 1 and 8, and perhaps 10, 11.

- Related to the above, a figure showing individual model performances vs. observations would also be useful, to provide context on overall model biases and spread. Again, I suspect that such a comparison will be presented in more detail in other publications, but it would be beneficial in this paper too. My suggestion would be adding a figure along the lines of Fig. 2 in B13, i.e. a “Taylor Diagram” of individual model performances vs. observations. (Taylor Diagrams provide a good “summary” compromise.)

- A discussion of the robustness of spatial patterns of impacts, e.g. of the agreement between model spatial changes, is glaringly missing. Different stressor changes are

C3

obviously associated with different degrees of uncertainty in different regions, as the Authors discuss in the text – with pH changes being very robustly constrained, and other changes (e.g. in the benthic layer) being much more uncertain. B13 addressed this point by plotting a measure of model agreement on maps of changes – i.e. the “stippling” on Figs. 5, 6, etc. in B13. This is extremely valuable information that contextualizes the magnitude of the stressors and our knowledge of how they will likely play out. I strongly encourage the Authors to address this point by revising the relevant figures.

- The paper focuses on physicochemical changes, possibly to limit the amount of information that needs to be discussed, but it stops short of addressing major ecological changes predicted by the models. In particular, a discussion of NPP changes (which again was included in B13) is missing. Again, I suspect this will form part of a more ecologically-focused publication, but at the same time I feel that NPP changes are an integral part of the story told in this paper – e.g. they are the real implication of including stratification and declining surface nutrients, and in turn may drive more or less important changes in the other stressors discussed. I see how discussing NPP could open up discussion of an entire new set of (complex) processes (export, recycling, remineralization), but if a demarcation should be arbitrarily imposed, I would suggest it includes NPP in the current manuscript, at least as the major ecological change (and potentially stressor), and a “tease” for future, in-depth analysis of other ecosystem implications.

- Related, the introduction could do a somewhat better job rationalizing the scope and rationale of this paper. E.g., it is clearly not a comparison of different models, and can not go into too much detail on the effects of model structure, or resolution, or representation of different processes. Yet, all of these aspects underlie the imputes discussed in the paper, or at least their uncertainty. Similarly, it stops at mostly physicochemical changes, but ecological changes (NPP) are part of the picture, even when considering the stressors discussed here.

- At times, I wished for more details on the models than are summarized in the two ta-

C4

bles (perhaps including an additional table), mostly to avoid having to go to the primary references, or to other syntheses (e.g. often the reader is referred to Seferian et al., in prep.; I did not have access to that publication, and I would have preferred to see the relevant information in this paper). The information that I think would be useful, at least when contextualizing individual model results, inter-model spread etc., includes in particular model resolution (horizontal and vertical; atmospheric), and biogeochemical complexity (e.g. functional groups, ecosystem model structure, stoichiometry, etc.).

- Parts of the paper (e.g. abstract; Section 3.4, and others), read at times like “laundry lists” of changes and uncertainties for different stressors, scenarios, and Intercomparison Phases. This doesn’t make for a particularly engaging read of those sections, and the reader could be easily referred to Table 3, where changes are summarized, while discussion could rather focus on new findings (e.g. consistency with CMIP5, etc.) or processes. Along these lines, I think the abstract could be made much more incisive, and could highlight novelties compared to previous studies (e.g. B13), rather than reporting lists of numbers.

- Section 1.2: I suggest summarizing in a few sentences the relevant information from Seferian et al., in review, mostly to provide the required context without referring the reader to another publication.

- Section 2: I am a bit confused by the use of the word “integral”. I tend to think about the mathematical definition of integral, although here the term is used to refer to an average (related, but not identical).

- Line 264: I suppose what declines is the relative contribution of structural uncertainty, rather than the absolute value. This could be clarified.

- Section 3.5. The discussion of benthic changes is a useful addition to the paper. However, a discussion of deep-ocean model resolution and other sources of uncertainty could be included. Ocean models are usually not designed to resolve deep-ocean properties (and processes) as well as in the surface ocean, so the caveats may

C5

be different and more important here.

- Lines 304-307: More detail could be given on these processes, in particular the effect of freshwater dilution. Also, changes are quite hard to see on the maps, especially for the low RF scenario, e.g. Fig. 2c. I wonder if on the maps, contour lines and labels could improve readability.

- Lines 310-315: Looking at O<sub>2</sub> changes in the N Pacific, I cannot help wondering what the importance of marginal seas is on some of the stressor changes – for example, in this specific case the role of the Sea of Okhotsk in ventilating NPIW (other examples can be thought of, e.g. Persian Gulf, Red Sea, etc.). I suspect global models have significant biases in marginal sea circulation, but sometimes these poorly-resolved regions can disproportionately affect the open ocean. Perhaps a discussion of these issues can be included somewhere, with some indication of obvious biases and possible directions for improvement.

- Lines 339-340, and 350-351: I have a hard time seeing the effect discussed in practice, e.g. by comparing Figs 2h and 4b.

- Section 3.3. This is a nice section, and it’s clearly important to look at the compound effect of multiple stressors. That said, the Authors could do a better job in discussing the (arbitrary) thresholds selected. Especially for O<sub>2</sub>, picking a change threshold may not be that informative – a change by 30 mmol/m<sup>3</sup> may be negligible in waters close to saturation, and would be massive in waters close to suboxia. I realize a best summary threshold that encompasses a heterogeneous range of stressor responses may not exist, but some rationalization (and caveats) would be useful for context.

- Line 381: I am not sure I get the referent to the MAGICC7 model – I couldn’t find it mentioned elsewhere in the manuscript.

- Lines 415-420: I was surprised by the inconsistency in the bottom water O<sub>2</sub> changes, which are in fact larger in SSP1-2.6 than SSP5-8.5 (although indistinguishable given

C6

the uncertainty). Maybe this can be commented on. This also bring up an additional thought: bottom ocean ventilation, especially in the Southern Ocean, may be strongly affected by another sets of processes poorly captured by current climate model, namely, open-ocean polynyas. I wonder if different RF scenario result in somewhat non-trivial changes in SO deep ventilation, which in turn affect bottom water O<sub>2</sub> and other properties.

- Line 542: the relationship between RF scenario and impacts is shown in the paper in a somewhat indirect way: i.e. there is not a single figure (e.g. along the lines of Fig. 6) that relates RF to impact. I think the closest would be a figure relating stressors to SST, somewhat along the lines of Fig. 6a.

- Fig. 6b: I'm puzzled by the fact that SSP3-7.0 shows more dramatic changes here than RCP8.5. I suppose this may have to do with the stronger climate sensitivity of CMIP6 compared to CMIP5. But the SST response is similar in the two scenarios (Fig. 6a).

- Figure 7: This is a useful figure, but I wonder how straightforward the interpretation actually is, since it conflates changes at very different depths. I.e. most of the ocean sits at around 4km depth, where impacts are muted, but much stronger impacts would occur in shallower benthic waters (which it should be noted host more important benthic resources). I wonder if an additional figure showing depth-dependent changes (i.e. profiles for benthic grid boxes only), e.g. for the end of century, could be a useful addition to Figs. 7-8.

- Line 156: "bacteria" -> "heterotrophic bacteria"? (I'm thinking that classic picoplanktonic functional groups already include bacteria).

- Line 272: "follows" -> "follow".

- Line 428: "in" -> "from"?

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