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Interactive comment on "Multiple stressors of ocean ecosystems in the 21st century: projections with CMIP5 models" *by* L. Bopp et al.

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We would like to express our appreciation to the reviewers for the time they took to thoroughly review this paper and for their valuable comments and suggestions, which we hope greatly helped to clarify the paper.

We first summarize the main changes that we made to the manuscript in response to remarks and comments from the reviewers. We then respond point by point to the reviewers' comments.

1). On the fact that, despite its title, our study does not directly address the coincident effects of multiple stressors on marine ecosystems.

The reviewer is right that we do not discuss the combined effects of climate change

and ocean acidification on marine ecosystems. Using the recent CMIP5 models and simulations, we present the evolution of several ecosystem stressors, as identified in the literature, over the course of the 21st century. Our main result is that, even if the global evolution is simulated in a consistent way across the different models, the regional projections are much less robust (even the sign of changes for NPP or O2 is not consistently simulated across the models). In addition, we show that future changes in pH, T, O2 and NPP may not be coincident, as it often assumed in multiple stressors studies. The scope of our study and the conclusions will be made clearer in the abstract and conclusions. We however think that our title "Multiple stressors of ocean ecosystems in the 21st century: projections with CMIP5 models" is adequate.

2). On the lack of model evaluation for the interior fields (other than dissolved oxygen) and on the ability of models to represent present-day fields.

We agree with the reviewer that the model evaluation section was incomplete: only surface temperature and surface pH were assessed despite the fact that projections of 3D pH and 3D temperature were presented in the water-mass analysis section. We have now included assessments of 3D fields (T, O2 and pH) in the text and in the corresponding figures (Taylor diagrams). In addition, we also assess simulated carbonate ion concentrations (surface and interior fields). We show that the model skills are much better for 3D pH and surface/3D CO3 than for surface pH – this is now detailed and explained in the manuscript.

3). On the fact that some model fields (like pH) are not ideally suited for demonstrating marine ecosystems impacts.

We agree with the reviewer than pH may not be ideally suited to demonstrate impacts on marine ecosystems. But focusing only on saturation state, as suggested, would target only calcification and calcifiers. Acidification may have lots of other impacts on ecosystems as demonstrated by more and more studies. We propose to keep pH as the main stressor here, and we add a section to show projections of CO3 concentrations (and how models project undersaturation in the surface of the ocean in high latitude regions). One new figure, showing projections of CO3 concentrations, is added to the manuscript.

4). On the (incomplete) description of biogeochemical models.

We agree with the reviewer that more details of the differences in the ecosystem/biogeochemical aspects of the models could be given. We now include a new table with new and specific information on the ecosystem component of the different earth system models. The information includes now the different nutrient considered, the number of phytoplankton and zooplankton types, the inclusion of an explicit bacterial pool, of non-redfieldien processes, the role of temperature on auto- and heterotrophic processes.

We now respond point by point to the reviewers' comments.

Reviewer 1

This is a well written article addressing the uncertainty in model prediction of potential stressors of marine ecosystems in a future changed world: warming, acidification, alterations to primary production and de-oxygenation. While the 10 CMIP5 models agree (mostly) on the warming and pH changes, they have much less agreement on the regional changes in primary production and de-oxygenation. In fact there is a surprisingly small number of regions that even agree on the sign of the change in primary production (PP). To me, this is the main take-home message of the paper: current models are extremely uncertain in how primary production and oxygen concentrations will change and I really like Fig 5 for this. The water mass analysis and the robustness analysis are very nice.

We thank the reviewer for his/her positive remarks.

Individual Science Questions and issues:

1) As stated above, my main take home was the level of uncertainty in models pre-

dictions of PP and de-oxygenation: Could this be emphasised more, and potentially even used as a cautionary note to the community to be careful on relying on any single model result to suggest future changes?

» Agreed. We now insist explicitly in the abstract and in the conclusion on the need to use multiple models instead of relying on any single model to project future changes.

2) I wonder whether the "Multiple stressors" aspect of the paper is the best aspect to emphasis. Though the authors do look at 4 main stressors, the story here is a bit muddled: part of the introduction (e.g. pg 3631, lines 20-line 4 next page) and several place in rest of text (e.g. 3647, lin3 5-6), the authors mention how the multiple stressors interact with one another (e.g. impact of acidification on deoxygenisation), but (to my knowledge) none of the models parameterise these interactions / feedbacks. The models only capture each stressor separately. I think this needs to be more clearly noted. It could be that model results will be quite different with some multiplying effect of changes, and it would be good to clear on this. Also figure 13 showing where multiple stressors might coincide, but only from the model mean. This needs to be very clearly stated, and in fact where models don't agree, should be masked in some way.

» Agreed. Most models do not simulate interactions between stressors; most of them even do not consider any impacts of pH changes on ecosystems and biogeochemical processes. We have reduced the introduction on stressors interactions and now explicitly stress the facts that these interactions are not represented in the models used.

3) This article is a nice intercomparison between models. And the authors provide a table (Table 1) to show some of the differences in the models. Since this is an intercomparison, much of the results depend on some of the parameterizations of the ecosystems/biogeochemistry. Though I do think more in depth understanding of the reasons for the differences would be nice, it is maybe beyond the scope of the paper. However, for the readers to think more about possible reasons, I think it important that the authors include an expanded (or new table) to give more details of the differences

in the ecosystem/biogeochemical aspects of the model. For instance - how does each model treat grazing? Never mentioned in the paper, but differences in top-down controls could cause some of the differences seen in NPP and export. What nutrients limit productivity in each model (i.e. do some include different set of limiting nutrients)? A potentially large reason for the differences in O2 changes might come from the param eterization of sinking of organic matter and remineralization: it would be good to know how the different models treat these. The authors state that differences in treatment of temperature dependence of growth and remineralization are likely responsible for the difference seen in NPP changes: it would be good to know how different the model parameters are in this regard.

» Agreed. We now include a new table with new and specific information on the ecosystem component of the different earth system models. The information includes now the different nutrient considered, the number of phytoplankton and zooplankton types, the inclusion of an explicit bacterial pool, of non-redfieldien processes, the role of temperature on auto- and heterotrophic processes.

Others Specific Comments: pg 3636, line 25: Why not leave out coastal zones in Taylor diagram calculations? Also note that the satellite derived NPP has large error estimates on it.

» Agreed and acknowledged. Coastal points are now left out in Taylor diagram calculations and we note that satellite derived NOO has large estimates error on it.

pg 3642, lines 15-30: Worth pointing out that in some regions of the biggest changes in model-mean NPP (e.g. Equatorial Pacific), the models do not agree even in the sign – this is contrast to the O2.

» Agreed. Text revised.

pg 3644, line 23: I think the use of the word "highly" here is misleading. Taucher+Oeschlies looked at either a temperature dependence or not – there was not

several "levels" of temperature dependence studied as the word "highly" implies.

» Agreed. Text revised.

pg 3645, line 1-2: The authors state that "....it is likely that that they use very different parameter values..."; but since the authors know what these parameters are from their various models, they might want to be more definitive on this point. See point 3 above

» Agreed. Text revised.

pg 3649, line 1: I am more stuck by how large the ranges are in the different models results than that the results are "...distinct and relatively robust across the range of models.." . You might want to change this sentence.

» Agreed. Text revised.

pg 3649, lines 20-23: It would be nice to see how the overturning changes for each model, even plotted against pH and/or O2 changes: i.e. does the difference in overturning really explain the differences in the model results?

» We agree with the reviewer that adding this new analysis would be nice. This additional work may however be beyond the scope of our paper. In addition, some work with the same set of models is now published or submitted and focus on the role of water masses distribution and changes on ocean acidification (Resplandy et al; GRL 2013) and the role of changes in overturning on ocean carbon uptake (Schwinger et al. submitted).

Technical Corrections: pg 3628, lines 14-17: It would be good to put the ranges (or standard deviations) of the percent changes to SST, pH, PP, and O2. To me, the range uncertainty is a main point of this paper. pg 3635: line 17: "but" missing between "components" and "differ"? pg 3641, line 25: "Oceans" has a space in it. Figure 2: Symbols are very small. Might be worth either putting both figures on same scale, or note on the caption that they are different. Figure 11: Caption needs to state that all 4 RCP's are shown Figure 12: Caption needs to state that these are for RCP8.5 Figure

13: This shows where model mean changes are the biggest. And yet in some of the locations where the biggest mean is predicted the models do not even agree on sign of PP or O2 change. I think this figure should be redone with the regions where models do not agree masked in some way. Otherwise this can be mis-interpreted.

» All technical corrections taken into account.

Reviewer 2 : General Comments The title of the manuscript had me expecting to find a discussion of the combine impacts of climate change and ocean acidification on marine ecosystems. The title is misleading and the study really only presents the environmental changes projected by 10 CMIP5 models for a range of RCP scenarios for 4 key environmental variables (T,pH, mid-depth Dissolved Oxygen (DO) and Primary Productivity (PP)). The study sets out to investigate multiple stressors for marine ecosystems. However, the chosen fields (like pH) are not ideally suited for demonstrating marine ecosystem impacts. Second, many of the stressors are not coincident (located in different parts of the water column). If the study wants to discuss multiple stressors of environmental change they should present the stressors in coincident domains (e.g. for: 1)ocean surface and 2) ocean interior between 200-600m). In the latter domain, a case must be made for why this depth range can be considered one habitat, Further, in the ocean interior the coincident DO, CO2 and Temperature changes should be given.

In choosing the fields to present, the authors need to clearly motivate why these environmental changes are ecosystem stressors. Further, they must provides some context on what is a significant change (may be a threshold change) and discuss in a more quantitative manner why RCP26 significantly less stressful than RCP85. To address the questions identified, the paper needs to discuss environmental stres- sors relevant to marine ecosystems and in a manner that makes it clear the changes are coincident thus justifying calling them multi-stressors.

» We thank the reviewer for his/her comments and remarks. As stated above, we have clarified the fact that we do not address the combined effects of multiple stressors

on ecosystems but present how these stressors may evolve, coincidently or not, over the course of the 21st century. We discuss in more details the rationale to focus on the chosen fields. In addition to pH, we now present projections of carbonate ion concentrations.

Specifics (I'm working of the discussion paper and I have attached it to make the link to my comments clear. While the pdf has notes all the important issues are listed below)

pg 3629 line 9 - cite the original references or preferably use some more recent references line 10 - 11 - add citation for the first part of this sentence line 13 - Thomas reference seems out of place since the example you give more reflective of high latitude ecosystems line 23 - their several more recent studies of anthropogenic CO2 concentrations (e.g. Khatiwala et al., 2009)

» Agreed. More recent and original references have been added to the introduction.

pg3630 line 8 - add citation for 50-80 threshold and what organisms have such a high oxygen threshold?

» Citation to Keeling et al. (2010) and original references have been added.

line 28 - add citation for importance of T, DO, pH and NPP and it possible give key threshold changes

» Giving key tresholds is a difficult task. We now discuss for each of the stressor potential and meaningfull tresholds.

line 29 - the sentence is not clear - will they be discussed here or later

» Acknowledged. Text revised.

pg 3631 line 3-8 - should comment on the difference between is92a and RCP8.5, since for CO2 the latter has much greater concentrations by 2100 then IS92a.

» Differences between is92a and RCP85 now acknowledged.

line 6 - a better reference for the emergence of under-saturation in the Southern Ocean in the McNeil and Matear (2008).

» Reference added.

line 9-14, The overall decline in DO as you stated earlier in the introduction is probably not important, rather it is whether the volume of low oxygen water (you stated 50 -80 umol/l or less) changes. You should discuss this feature in previous modelling studies since there are many studies which have look at this issue (e.g. Cocco et al 2012, Keeling et al 2010, Schmittner et al 2008, Oschlies et al 2008, Matear and Hirst 2003). For this ocean field there are important physical, solubility and biological interactions

» Most of these references are cited and discussed later in the text.

line 20, why do you focus on pH rather than saturation state? there is a much clearer biological link with aragonite saturation state than pH

» See discussion above. Projections of CO3 are now included (new figure and revised text).

line 23 - The statement is confusing As I read it, it sounds like from a biological perspective there a debate whether ocean acidification and deoxygenation have a synergistic impact on marine biota (if this what you are trying to say then add citations for this debate). What you go on to discuss is the change in C/N of exported organic matter with rising CO2 ,which is a feature that is also being debated. The synergist impacts are on BGC cycling rather than marine ecosystems.

» Agreed. The discussion here has been revised.

pg 3636 line 12 - this statement is provocative and make me ask what is the point of the study. I suggest you just state what you will focus on.

» Agreed. Text revised.

line 24-25 - the ability of the models to represent the surface pH and NPP variability

is poor - why should I believe the projections of such poor models. Justify using these models for looking at pH and NPP changes.

» Ability of the models to represent 3D pH is now added (much better skills than for surface pH). We also explain why the models have such low skills for surface pH. Low skills for NPP have already been reported and discussed in Schneider et al. (2008, BG) and Steinacher et al; (2010, BG) for a previous set of Earth System Models.

pg 3638 line 4-8 - Confusing you start by mentioning differences in radiative forcing but then give an example of different climate sensitivity - what causes the differences?

» Text revised.

line 20-24 - Aragonite would be a better biological stress variable particularly in the upper ocean where the undersaturation provides a nice threshold to discuss. Is92a scenario has less co2 atmosphere in 2100 than rcp 8.5. This difference should be discussed because it changes the acidification of the ocean. Further, I don't follow the range given for pH (pH range should be 012.x4 = 0 .003). Where do you get 0.1 from?

» Agreed. Projections of CO3 are now discussed. (new figure, and additional text).

pg 3639 line 24 - I don't see why global decline is relevant for the discussion of multi-stressors on the marine ecosystem.

» We maintain a focus on global evolution of each stressor. Regional deviations are discussed from a global perspective.

pg 3644 line 10 -14 - explain why you think they are decoupled

» Text revised.

pg 3647 line 4 - Looking for synergies at a global scale is inconsistent with the concept multiple stressors impacting ecosystems line 9 - why is global DO changes important? you need to focus on biologically relevant variables

» Agreed. Discussion on synergies has been revised.

For the results section I also included my comments on the figures figure 2 -you could just use numbers for model and colours for variable to make the figure easier to read - Models have no skill for ph. Why? - What are the values for RMS error lines? - what is the bias between obs and models? - Include in the plot the multi model mean perhaps a shown as a circle to represent the variance in the multi-model mean - what years are used for models? - for the PP observations they are really just another model. This should be clearly stated.

» The Taylor diagrams have been redrawn to include additional variables and interior fields. Caption has been revised accordingly. Model skills for surface pH now discussed.

Figure 3 -No variability in ph - state why

» Text revised.

Figure 4 -Why show individual models? The only new info is in c) where one model has strange increase in DO in first 50 y drift? What causes this?

» We do not have explanations for the 50yr drift seen in one model. Showing the individual models illustrates how uncertain are some of the projections. It also enables to see where one individual model stands (especially for those using only one single model to study impacts of multiple stressors on marine ecosystems.

Figure 5 -Define stress intensity - Missing arctic in figs a and b -Missing shade key -pH change is not a significant stress? All the models have the same ph change but present day pattern was shown to be bad for all models. Why should I believe the projected change?

» Figure has been corrected (Arctic now included). “Bad” model skills for surface pH now discussed.

Figure 6 -define stress intensity

» Stress intensity now defined. Figure 7 -Need to convince me the pH projections can be used when they so poorly represented in the observed fields. - why show pH since it affect as a marine ecosystem stressor is less clear - what is a significant pH change for marine biota? » Again, “bad” skills for surface pH now discussed. And CO3 projections added. Figure 8 -Would be nice to know the actual change too

» We did not attempt to evaluate our models against changes (only model-mean fields are assessed). This is beyond the scope of our study.

Figure 9 -D. Only one model? But it is not shown in b and c? Therefore, no model meets the good criteria for 3 selected volumes! Justify the use of model projections.

» The reviewer is right. None of the models meets the criterium for all low-oxygen zones definitions. This demonstrates how these projections (for low oxygen zones) are uncertain.

Figure 10 - panels a) and d) Useful comparison? Why should they be related? Good correlation does mean causality. This analysis would be more useful is done regionally rather than globally- b) and c) How do the model compare to the observed heat content changes?

» Agreed, but the interesting point is on the distinct temporal evolution of these fields.

Figure 11 -Should assess models present day representation of these fields before looking at changes - Why the multiple plots per water mass? - By 2100 you can see and detectable changes in the averaged deep and bottom water properties?

» Now done.

Figure 12 -As stated in previous fig I’m surprise you can see a detectable change in deep and bottom water - for marine ecosystems why are these change important - they are small - Need to convince me that the pH and DO changes are believable for these

water masses

» Again, the discussion of relevant thresholds is difficult. The text has been modified.

Figure 13 - selection of the thresholds - should assess the model less than 50 mmol/m3 area in the present-day - the DO decline in the intermediate water is not located where the SST and surface pH change occur - how can referred to as multiple stresses?

» Again, we maintain that we discuss multiple stressors and show how they can evolve coincidentally or not. This is a major message of our manuscript.

pg 3649 line 24-27 - you really only presented and assess SST. There are no clear analysis of the change in Temperature within the ocean. For example how does the observed change in heat content compare to the modelled fields? For marine biota, why should I care about T changes in the deep water?

» We now asses also interior fields (temperature, pH, O2). We do not present comparison of simulated and observed ocean heat content changes over the last decade – this is beyond the scope of our study. And has been the target of other publications (see for instance Gleckler et al. 2012 Nature Geosciences with the CMIP3 models).

pg 3650 line 15-17 DO values are high, are the DO changes in the Southern Ocean really a stressor for marine ecosystems?

» Agreed. Text will be modified.

line 18- next page - As you stated you did not do attribution analysis therefore you needto cite the previous work to back up your statements about the mechanisms responsible for the projected changes

» Agreed. This was partially done only. Text revised.

pg 3651 line 3-5 - How do pH biases affect the projections? This is especially important for regional and water masses changes.

» Difficult to say how biases affect projections. Again, 3D pH is now assessed.

line 6-15 Why is this important ? For most of the variables chosen (e.g. T, pH, DO) the biological parameterization are of second order importance. The base state is where you would expect to see the largest impact of the chosen parameterization and for fields like pH and DO there are problems with most of the models. The paper switches between surface and interior, but for the interior fields (other than DO) there was no assessment of present day fields. For ocean interior changes in heat content and anthropogenic carbon are two fields that could be used to assess the simulations.

» Agreed. Interior fields are now assessed (Evaluation Section and additional Taylor diagrams). Changes in heat content and anthropogenic carbon are however not used: this is beyond the scope of our study.

Interactive comment on Biogeosciences Discuss., 10, 3627, 2013.