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***Reviewer #1 (Remarks to the Author):  
  
This manuscript summarises projected changes in sea-surface temperatures and oxygen concentrations in the global network of MPAs, with a specific focus on the year of "emergence" of trends beyond background noise.  
  
At its heart, this is a simply, yet useful paper that would likely be well cited. I do have a three fundamental concerns. All can be reasonably easily addressed, I think.  
  
First, the utility of the entire manuscript is contingent on the premise that environmental change will have detrimental consequences for biodiversity. Whist I agree with this idea in principle, ecological impact is not modelled here in any way. Instead, environmental change is modelled, with impacts assumed. In many cases, this assumption is hidden behind some very "certain" language (concrete assertions). I think this needs attention. Where the authors refer to assumptions/projections, they should not imply certainty, but should rather use language that reflects at least some uncertainty.***

We understand the reviewer’s argument and concern. We spent a lot of time discussing what the projected amount of warming would mean for populations, species, and communities and how to estimate or model this empirically. It is certainly challenging and we can’t be sure we aren’t overestimating or underestimating the potential effects. In part because we have little ability to project rates of adaptation and acclimatization. We also know little about the thermal optima and the upper thermal tolerances of most species.

That said, we believe the ecological effects of ~3 °C of warming (by 2100 via RCP 8.5) are more likely than not to be substantial; primarily because we are already seeing large effects after just 0.5-1.0 °C of warming in the oceans. The already observed warming effects on species ranges, fitness, and abundance have been synthesized in numerous studies (e.g., Harley et al. 2006, Poloczanska et al. 2013), and by the IPCC in ARV (Hoegh-Guldberg et al. 2014). And there are hundreds of papers describing substantial, already observed impacts on ecosystems around the world, e.g., on coral reefs (Selig et al. 2012, Heron et al. 2016, Hughes et al. 2017).

However, we agree that the language on effects may be too strong and that we don’t sufficiently articulate the substantial uncertainty about how physical changes would translate into ecological impacts. We have altered the text to make this uncertainty clearer.

***Where the authors refer to assumptions/projections, they should not imply certainty, but should rather use language that reflects at least some uncertainty.***

We agree with this and have revised the manuscript accordingly.

***Second, although two RCPs are analysed, results and discussion focus heavily on RCP 8.5. It would be useful to provide regular contrasts with projections from RCP 4.5 to indicate what might be achieved in the event that global action is taken to mitigate climate change. This is particularly important, given the final conclusions of the manuscript.***

We agree. And several of the co-authors pointed this out to me before submission. I worried discussing RCP 4.5 outcomes, comparing them to RCP 8.5, etc. would make the paper too descriptive. Additionally, we had to cut a lot of text to stay within Nature’s word limit. We have added this text and now more fully explore the contrasting outcomes of RCP 4.5 and 8.5, however, we note that this information is also presented in the graphics and table.   
***Third, the origin and novelty of the data and trends are unclear in the main text (and in some cases, also in the Methods). Specifically, it seems to me that year of emergence was not computed for this analysis, but rather extracted from a recent paper; similarly CTSM was extracted from a separate paper. Even the SST and [O2] projections were extracted from non-native grids whose origins remain unclear to me. I have no problems with use of data extracted from various sources, but the origins should be more clear in the main text, and should be explicitly stated in the Methods.***

Understood. We have added text to the main text and the Methods clarifying the sources of the data and where it can all be obtained.   
***Beyond this, there are several minor issues that that Authors should consider addressing. They are listed below by line number.*  
  
*Title: The title implies threats to biodiversity, but the link between the projected warming and threats to biodiversity are not strongly developed in the manuscript, which focuses instead on trends in environmental change.***

We disagree that the effects of warming on marine biodiversity are not strongly developed. Nevertheless, we changed the title to “Climate Change Threatens the World’s Marine Protected Areas”.

***15-16: I’m not convinced that the analysis shows that there will be species or habitat loss.***

Based on what’s already happened (substantial species declines and habitat losses due to warming), niche theory, modeling by other studies (discussed in our paper), and our original analysis of how key physical attributes would change, we argue the RCP 8.5 scenario would likely result in altered patterns of biodiversity. Namely losses in the tropics where temperature and oxygen conditions would likely exceed the tolerances of most species. We have clarified the underlying assumptions that the mechanistic link between such environmental change and biodiversity change is dependent on. Namely that: 1) temperature, pH and oxygen concentration influence the geographic distributions of species, and 2) forecasted changes in these variables beyond the natural variability experienced by species are likely to cause a shift in geographic distributions of some constituent species, thereby affecting local biodiversity. We argue in the text (more clearly now) that there is ample empirical support for both assumptions and that this (biodiversity shifts) is already happening. We do, however, recognize that we cannot be certain what the effects will be and we now emphasize the inherent uncertainty in such ecological forecasting.

***18: When MEAN SST & O2 exceed these ranges?***

No. When the values exceed the natural range.

***19: “Natural variability” has no real context here. When was the variability natural?***

Before anthropogenic carbon emissions altered ocean environmental conditions.

***20 (and elsewhere): I’m not sure that “factor” is the right word here and elsewhere. Would “Stressor” not be better? Or just “environmental variable”?***

We agree and changed to “environmental variable” or “variable” throughout, except on line 26 where we used “stressor”.

***24: CTSM is an undefined abbreviation, albeit that its definition is vaguely implied.***

Term is defined in lines 22-23.

***25-26: Yes, but this will often be the case. The real question is whether one stressor is more influential than another.***

Fair point

***29: How many such species really “depend” on marine reserves?***

Fair point. Probably very few. Text was edited to: “Species largely restricted to marine reserves could be especially sensitive to anthropogenic climate change because of their typically small populations and low genetic diversities (Peters 1985)”

***39: Insert comma: “…perturbations, including…”***

Done

***43-45: An assertion often made, but see Llovel et al. (2014) Deep-ocean contribution to sea level and energy budget not detectable over the past decade. Nature Clim. Change 4 (11), 1031-1035.***

Interesting, thank you.

***47-49: This needs a citation. Maybe Pörtner H-O, Bock C, Mark FC (2017) Oxygen- and capacity-limited thermal tolerance: bridging ecology and physiology. Journal of Experimental Biology 220:2685–2696?***

GREAT paper that we hadn’t seen. Citation added.

***52: Possible issue with citation: “(ref. 2)”?***

We believe this is the Nature format for citations following numerical values.

***54-55: Are the no-take reserves in addition to the 8236 MPAs, or a subset. Minor point, but not clear from the writing.***

They are a subset. Made clear in the text.

***Figure 1: No explanation of panels in the figure caption, and there is an inconsistency between panel identifiers (lower case letters) and in-text citation (capital letters)***

See response to this point below.

***57-59: Either “/“ or “per”, but not both...***

Done

***Table 1: There is a case of analysing minimum temperatures, also, since these often control leading range-edge expansions. Ranges expanding into MPAs with vulnerable (small?) populations might introduce new competitors, for example.***

Agreed

***71: Projected responses, not responses…and they are driven by the assumption that species will track their projected thermal niches. Perhaps this language is too concrete.***

Agreed and edited. But note there is quite good evidence that they do, e.g., Pinsky et al 2013, DOI: 10.1126/science.1239352. We have added a citation for evidence of this (Poloczanska et al 2013) but not Pinsky et al since we are already at the publsihers limit of 30 citations.

***77-78: Again “will” is too concrete…these are projections, so certainty should not be implied****.*

Agreed. Wording softened.

***80-81: Insert comma: “…processes, including…”***

Done

***83: Since the authors are writing about temperature changes, perhaps a word other than “degree” would be more appropriate here?***

Done

***87-88: It is not explicit whether this emergence refers to any one of these three stressors, a combination of two, or only to the case where all three exceed natural variability. Adjust wording to clarify.***

Re, “Under RCP 8.5, by 2050 trends in warming and deoxygenation, as well as declining pH, exceed background variability over 86% of the ocean (Henson et al. 2017)”

It is meant to indicate all three individually exceed natural variability. Wording adjusted.

***88-91: This*** [“Assuming organisms are adapted to local environmental conditions, this degree of change of multiple factors that strongly affect their metabolism and fitness, and largely define their fundamental niches, would likely cause local extinctions and changes in species composition.”] ***is a strong statement that requires a citation. If there is no support in the literature, this is a bit jump to make, and should probably be expressed more tentatively as an expectation?***

To us, this statement doesn’t seem especially strong or definitive. First, we are merely stating the assumptions underlying the interpretation of our analysis. Second, we included the qualifier “likely” – meant to indicate (explicitly) that there is uncertainty about the effects, even if the assumptions are met.

There are countless citations that could be added, but again, we note NCCs citation limit of 30. We would love to add more, but since we’re already at the limit, for every citation we add, another has to be removed. We recognize there are far more than 30 statements in the text that require citations, but we don’t believe we can ignore the formatting instructions.

***Figure 2: “a” & “b” on figures, but “A” & “B” in legend.***

Done

***Fig 2: I don’t understand what is meant here. Blue dots have already exceeded ranges, but “emerge” well after 2017? How?***

The legend was incorrect. Should read: “Red circles are fully protected reserves in which thresholds have already been exceeded (in 2017), Blue circles are reserves that have not”. It has been corrected.

***Fig. 2A/B: Why would you expect these relationships to be linear? Visual inspection suggests that they are not, with a trough (A)/peak (B) at the equator.***

The non-linear fit was not significant.

***95: This statement in parentheses could be moved earlier and given more prominence...it is an interesting point***

Done.

***106-107: This is a strong statement of theory that needs support from the literature. Alternatively, phrase in a less concrete manner.***

Agreed. We added citations and also softened the text.

***114: Insert comma: “…(Table S1), but…”***

Done

***247: What is a “mean maximum”? Could this wording be clearer?***

The mean maximum is the mean of maximum values. And we do understand that this is confusing and we clarified the language in the legend: “Mean values are the means annual changes in the mean temperature across units (e.g., no-take reserves or all MPAs). Maximum values are the means of the maximum projected values across all units.”

***255: By “sample size”, do you mean number of grid squares?***

Yes, and we clarified in the legend.

***Table 2 and associated analysis: did you adjust for changing area of grid squares (i.e., use weighted mean, etc.)?***

No, we did not.

***Figure 3: This is an interesting colour palette, with red indicating essentially the best possible outcome. The authors might want to think about the intuitive interpretation. Also, the panels are not identified by letter.***

Apparently, this palette is recommended by graphical display experts. The primary reason for including the lettering was to be able to refer to specific panels in the text. And it seemed unnecessary to explain what region each panel covered, but we could add that to the figure legend if the editor prefers.

***120: 3.5% by number or area? Please clarify.***

Number

***131-134: Most analyses focus on RCP 8.5, so the data presented do not necessarily support the contention made here. A more thorough treatment of results from RCP 4.5 would allay this concern.***

Very good point. We have made a greater effort to describe RCP 4.5 outcomes.

***274/428: The M in GAM is model, so “model” is unnecessary.***

Done (good point)

***296-300: Where are these 1 x 1 degree data archived (CMIP5 ocean realm data are generally not on a 1 x 1 º grid). Ensembles of which models and why?***

The original model data is archived at <http://pcmdi9.llnl.gov>. To minimize errors an ensemble was created of all models, all models were first regridded to a 1x1 grid then the average of all available models was taken.

***How were GCM estimates from varying geographic projections combined?***

They were not combined. They were compared to assess how different the projections were. Ideally, we would have used only the downscaled projections, however, they only cover from 45oN to 45oS. The analyses presented in the paper are all based on the original, larger-grained projections. We tested the validity of this choice (in a qualitative sense) by comparing the two projections in several ways, including via mean values in Table M1, in the map in Fig. M2 and in the density plots (Fig. M1).

***Methods: Throughout this section, the authors mix and match “degree” and “º”. Consistency would be better.***

Done

***311: Insert comma: “…hemisphere, while…”***

Done

***312: Missing degrees.***

Corrected

***333-335: Readers will be familiar with the concept of anomalies. Use this terminology to rewrite the figure caption, thereby making it more intelligible.***

What is plotted is not anomalies. It is the difference between the two model projections (the downscale and the original, larger-grained). The plot shows how the differences between these projections vary geographically, being greatest in northern latitude. And that the downscaled projections from somewhat cooler in the northern hemisphere and warmer in the southern hemisphere.

***The final sentence is unnecessary and should be deleted.***

Done

***382-383: Hyphenate “community-weighted”***Done***Reviewer #2 (Remarks to the Author):  
  
The authors raise an interesting argument – that the protections already provided to MPAs might not be sufficient to protect them from changes driven by a changing global climate. The authors focus on the patterns of concomitant warming and deoxygenation using a modeling approach. The core observation here is that they provide predictions of the emergence of biologically-significant high temperature combined with low dissolved oxygen as they appear where MPAs are found. For this modeling effort, the authors use a “business as usual” scenario RCP 8.5, and a mitigation strategy, RCP 4.5, where emissions stabilize around 525ppm in 2100. Although this might be a minor point, it might interesting to defend this choice in the manuscript. In addition, to add the value of the study, have the authors considered including an ocean acidification component in the study. For many coastal marine ecosystems, that include many MPAs, the emergence of future OA is quite drastic.***

We did initially include OA in the analyses, however, because pH had already “emerged” (current values exceed historical variation), we obviously couldn’t compare the timing of future emergence, e.g., among latitudes. We did however, include one of the original key pH plots as a supplementary graphic (Fig. S1) and we now make this point in the text.

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