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Biogeosciences
Discussions

Interactive comment on "Oxygen and indicators of stress for marine life in multi-model global warming projections" *by* V. Cocco et al.

Anonymous Referee #2

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1 Summary

Cocco and co-authors investigate future changes in oceanic CO_2 and oxygen using a suite of coupled earth system models that have been forced with the SRES A2 emission scenario. A particular focus of this paper is the analysis of the CO_2 and O_2 changes in terms of the respiration index. While the models tend to simulate relatively similar warming magnitudes and patterns, the simulated changes in oxygen differ widely, particularly in the oxygen minimum zones. These changes are projected to remain small, however, while the invasion of anthropogenic CO_2 impacts the thermocline waters globally, dominating the overall changes of the respiration index.

2 Evaluation

The potential for major long-term changes in the marine oxygen content as a result of global warming is becoming increasingly recognized as yet another dimension of anthropogenic climate change. So far, no systematic and coordinated multi-model comparison has been undertaken to see which changes are robust, and which ones are highly sensitive to particular model setups and idiosyncrasies. Furthermore, no large-scale multi-model assessments have been undertaken so far with regard to the issue of multi-stressors, i.e., the notion that many global ocean O_2 changes do not occur in isolation, but together with major changes in other properties, such as temperature and CO_2. Thus, this paper addresses a clear gap in our understanding and therefore represents an important and much welcomed contribution to the field. Furthermore, the study reports on the results of a very careful data-based evaluation of the performance of the models with regard to their simulated fields for the present-day ocean.

The paper is overall well crafted, clearly organized, and generally well illustrated. It addresses an important topic, and the method and results are of interest to a wider community. The approach of combining output from Earth system models to form a joint perspective is not particularly novel, and model intercomparison papers always leave you with the somewhat unsatisfactory impression that you learned much about where the models tend to simulate things incorrectly, but you usually learn very little about why this is the case.

Nevertheless, I fundamentally support the publication of this manuscript in Biogeosciences. But there are two major comments that need to be considered by the authors, in my opinion, before this manuscript can be published.

1. *fO_2 and fCO_2*: While I follow some of the arguments for why it could be relevant to analyze the CO_2 and O_2 distributions in terms of their fugacity, I am not really convinced. In fact, I rather find it distracing. I therefore strongly argue for a more

concentration based approach. My concrete suggestion would be to strengthen the concentration based analyses and to reduce the sections dealing with fCO_2 and fO_2, and ultimately also the respiration index. One of the reasons for why I opt for a stronger focus on the concentrations is because they are a much more direct measure of the changes in O_2 and CO_2 compared to fO_2 or fCO_2. Furthermore, the physiological relevance of the Respiratory Index is anything but well established, so it is given too much prominence in the text, in my opinion.

2. *Length*: The paper is on the long side with a rather large number of figures. In my opinion, the paper has a substantial potential for a careful editorial pruning in order to make the manuscript easier to digest and follow. The introduction is a good example as it introduces many findings and concepts that are interesting but not relevant for this paper.

3 Recommendation

I recommend acceptance of this manuscript after a minor to moderate revision. I particularly recommend that the authors emphasize more the actual chemical changes rather than those expressed in terms of fCO_2 and fO_2.

4 Minor comments

p10788, line 14, "the goals of this study...". I found the presented arguments for why we should focus on fCO_2 and fO_2 marginally convincing. I find the downsides, e.g., highly non-linear response of fCO_2 and fO_2 to the underlying chemical (and physical changes), non-conservative nature, high sensitivity in the deep ocean rather than in the main thermocline, and lack of strong experimental data that support the respiration

index, etc. too condemning. As a result, I recommend a substantial extension of the classical chemical analyses, at the expense of the discussion of the fugacities and also of the respiratory index.

p10790, line 5ff: This paragraph is a good example of how the text could be substantially streamlined. In my opinion, it is relatively irrelevant for this paper to know whether or not global warming is anthropogenic or not.

p10789, lines 26ff: "negative trends". I would change this to "mostly". A closer look at most papers reveal a rather rich pattern of increases and decreases in O_2, reflecting the complex processes that govern the distribution of this dissolved gas.

p10793, line 7, "GLODAP and WOA gridded data sets for comparison". I recommend to be careful with the use of these gridded products, particularly when it comes to the computation of derived properties, as is done here. I would feel much more comfortable if the authors used the non-gridded products, computed the quantities there, and then extrapolated these new quantities to the globe, The problems in the gridded products include excessive smoothing, and potentially overly large extrapolation/interpolations. One region where this may be particularly relevant are the oxygen minimum zones, where Bianchi et al. already showed some of the problems with the WOA oxygen values. These regions also have relatively few carbon measurements, so that the computed fCO_2 from gridded GLODAP could be rather far off from the true value. I recommend to at least check a few places with independent in-situ data.

p10797, line 10, "using averaged values over the 100-600 m depth range". It is unclear to me why this was done that way. Please elaborate.

p10798, line 7ff, "...can be large ... even if deviations are modest". In what way is this statement supported by the presented evidence? This remained unclear to me.

p10806, section 4 Discussion: This is more a summary than a discussion section. The discussion could indeed be strengthened.

p10807, 3rd paragraph: This paragraph (starting with "changes in dissolved..." felt repetitive.

p10809, line 5, "exceed natural variability". Where was this shown? Has this indeed be demonstrated in this manuscript?

Interactive comment on Biogeosciences Discuss., 9, 10785, 2012.