Februar 22, 2013

Biogeosciences Discussions

Manuscript: “**Oxygen and indicators of stress for marine life in multi-model global warming projections**”
by V. Cocco et al.

Author Reply to Referees #1 and #2:

We would like to thank the two referees for their time and care in providing comments on our manuscript. We will answer each in turn beginning with Referee #1. Our comments are presented in blue font, the Referee’s original comments are in black. *Italics* are used for the quotations of changed or added text in the manuscript.

Anonymous Referee #1

This manuscript presents predictions of the current (1990-1999) and future (2090-2099) state of the mesopelagic layer (100–600 m) of the World Ocean based on simulations performed with seven Earth System Models. The worst greenhouse gas emission IPCC scenario (SRES A2) was tested.
The effort to coordinate this multi-model simulation experiment, compare the outputs, and produce consensus predictions for the end of the 21th century is commendable. Furthermore, the manuscript covers the lack of joint predictions of the expected decline of O_2 and increase of CO_2, both affecting negatively the respiration of aerobic organisms. I find the manuscript very appropriate for a journal as Biogeosciences and have only a few minor comments and corrections detailed below.
We are pleased to hear that the referee feels our project to be of value and will discuss in the following the minor comments and the changes we will make.

MINOR COMMENTS

Page 10787, Lines 8–9. Please, state clearly in the abstract what you mean by “upper ocean”. It would be much better to define it as the upper mesopelagic layer (100–600m).
Page 10788, Line 29. Please, define “surface ocean” (0-100 m?) and “thermocline” (100– 600 m).
We agree on these and changed the terms accordingly in the text, e.g.: Pag. 10788 line 20
The analysis focuses on how anthropogenic carbon emissions and climate change affect decadal-to-century scale changes in CO_2 and O_2 within the upper mesopelagic layer (UML, from 100 to 600 m of depth)
Page 10790, Lines 27–29. I cannot see why a larger C/N ratio of the sinking organic matter leads to a enhancement of O_2 consumption at depth. To my understanding, a larger C/N ratio means a larger proportion of carbohydrates, with a respiration ratio ($-O_2/C$) of 1.0, which is lower than the respiration quotient of 1.4, characteristic of a material of Redfield’s composition. Therefore, a higher C/N ratio would lead to lower oxygen consumption at depth.
Thank you for the remark. We changed the sentence as follows:

As long as the O2/C ratio is not adjusted, this effect would lead to an expansion of the suboxic water volume in particular in the tropical oceans (Oschlies et al., 2008).

Page 10791, 1st paragraph. Maybe you could also comment on the positive effect of increased CO_2 levels on the dark incorporation of CO_2 by prokaryotic organisms inhabiting the dark ocean.
We agree with the reviewer that the dark lithoautotrophic and methanotrophic communities are a fascinating part of biogeochemical cycles. However, as these communities are not represented in the current generation of models, we feel that speculation on their responses falls outside the scope of this work.

Page 10794, Line 11. Replace "calcite cycle" by "calcium carbonate cycle"
Changed accordingly.

Page 10795, Line 13. I'm not familiarized with the Taylor diagrams and would acknowledge a brief introductory explanation of them. I think it would be useful for most reader of the ms.
Added the following explanation:
First, the skill of the different models in representing the observation-based fields for the world ocean between 100 and 600 m of depth is assessed by Taylor diagrams [Taylor2001]. The Taylor diagrams allow us to visualize the correspondence between model results and observation-based variables. The polar coordinates represent the correlation coefficient R (polar angle) and the normalized standard deviation sigma_model/sigma_obs (radius). Hence, in such a diagram the points corresponding to the observation-based variables would lie all at (R =1, sigma_model/sigma_obs = 1).

Page 10798, Line 27. Please, check the units of the delta fO_2 plot in Figure 4.
We corrected the mistake in the plot. Many thanks.
Page 10801, Line 17. Maybe you refer to "Fig. 6" rather than "Fig. 8".
We meant Fig. 8b.
Page 10803, Line 3. Please, replace "Fig. 11a" by "Fig. 11".
Many thanks.

Page 10803, Lines 9–12. On one hand, Why did you use a C:P ratio of 117 rather than the standard Redfield's ration of 106? On another hand, the marine biological cycle consist not only of organic carbon but also of $CaCO_3$. Depending on the effect of increasing CO_2 levels on the dissolution of $CaCO_3$, considering only the degradation of organic matter could be an oversimplification.
The reviewer brings up an excellent point that the present analysis described here does not include the carbonate pump component of the biological cycle, and have changed "biological cycle" to "the biological pump". Some models (e.g. BCM-C) also consider the degradation of particulate inorganic carbon, but it is considerably smaller than the contribution from particulate organic carbon counterpart. Although in the models actually different choices for C:P (and other elements) are made, the choice of 117:1 for C:P (Anderson and Sarmiento, 1994) was made here for practical reasons as it is in the middle of the range given by Redfield et al. (1963) and Takahashi et al. (1985).
We therefore rephrased:
[…] with the typical C:P Redfield ratio of 117 after Anderson and Sarmiento, 1994 (a value in the middle of the range given by Redfield et al. (1963) and Takahashi et al. (1985)).

Anonymous Referee #2

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.

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 largescale 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.

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.

We are pleased to hear that the referee recommend the acceptance of our manuscript. We will discuss in the following major and minor comments and the changes we suggest.

MAJOR COMMENTS

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 distracting. 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.

We changed the following figures giving more prominence to O_2 and CO_2:
Fig. 2 and 3: now O_2 and CO_2 instead of fO_2 or fCO_2 are illustrated.
Fig. 4: replaced fCO_2 and fO_2 with CO_2 and O_2.
Fig. B1 and B2: now O_2 and CO_2 instead of fO_2 or fCO_2 are illustrated.

Abstract: CO_2 and O_2 replace the fugacities. RI not mentioned anymore.
Pag. 10788, from line 2:
The goals of this study are to provide multi-model estimates of decadal-to-century scale trends in <u>*O_2, and CO_2*</u> *(together with fO_2, fCO_2, and other biologically relevant variables), as well as to identify underlying mechanisms using global warming simulations from six fully coupled atmosphere-ocean general circulation models and one model of intermediate complexity. […]*
Pag. 10789 line 2:
The concentrations of dissolved inorganic carbon (DIC, i.e. the sum of CO_2, HCO_3^{-} and CO_3^{2-} concentrations)
Pag. 10789 line 2:
Higher dissolved CO_2 concentrations (i.e. the sum of the $CO_2(aq)$ and H_2CO_3 concentrations)
Pag 10796 lines 13-22:
Similarly to the observation-based O_2 frequency distribution, the modeled O_2 frequencies range between 0 and 400 mmol m^{-3}, indicating the ability of the models to reproduce the correct range of variability of O_2 in the upper pelagic layer. The highest peak in the observations is at ~300 mmol m^{-3}. Most models tend to overestimate the magnitude of this peak and/or exhibit it between 200 and 240 mmol m^{-3}. The observation-based and modeled CO_2 *values lie mostly between 0.01 and 0.06 mol m^{-3}, with the highest peak at ~0.02 mol m^{-3}. The magnitude of this peak is largely overestimated by CSM1.4, MPIM and, to a smaller extend, CCSM3 and GFDL.*

From Pag. 10797, line 9 to Pag. 10798 line 10: replaced fCO_2 (fO_2) with CO_2 (O_2).

Pag 10814 line 20 – 10815 line 25: Section in the appendix replaced with *"Projected changes in RI - additional figure"*
*The largest reductions are projected by BCM-C in the Southern Ocean, the North Atlantic and Pacific. Most of the decrease is related to the increase in fCO_2 in many areas. RI is projected to increase in the Eastern South Atlantic in most models. This is consistent with the projected changes in fO_2 in this region. The increase in fO_2 dominates over the projected increase in fCO_2 in this low oxygen region. The projected changes in RI in the eastern tropical and Southern Pacific and in the Bay of Bengal and the Arabian Sea are not consistent across the range of models, similarly as found for projected changes in fO_2 in these low-O_2 regions. (*text previously at Pag. 10804 line 17-25*)*

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.

We agree and deleted these paragraphs:
Pag. 10789 lines 10-16
Pag. 10790 lines 13-19
Pag.10800: deleted line 23 (subsection: *Attribution to mechanisms*)
Pag. 10790, line 2: deleted *"and indicates increased stratification as the main driver of this decline."*

Fig. 6 and 10 are not presented anymore.
Fig. 14 is now in the appendix (as Figs. B3)

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.
We decided to consider the fugacities (together with O_2 and DIC concentrations) because of the lack of modeling studies in this direction and because of their physiological relevance.
To motivate this point we suggest to include the following sentence at p.10788:
*The fugacity of carbon dioxide (fCO_2) and oxygen (fO_2) are two key thermodynamical variables for reporting the physiological state of aerobic marine animals [Hofmann2011,Seibel2012]***.**
Pag. 10794 line 20: *the potential influence of CO_2 on respiration ...*
Pag. 10795, line 3-7: *Criticisms of the concept were raised and its practical usefulness has been questioned (Seibel2012,Poertner2011).*
Pag. 10805, lines 5 to 8: deleted

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.
We rephrased the paragraph:
The observed O_2 variations are relatively small, although they can result in changes up to 50% in the low-O_2 regions, and trends can therefore be difficult to detect. There is no consensus in considering anthropogenic global warming as the main driver of the observed O_2 changes because of the relatively short and sparse observational records. Natural variability may tend to mask the anthropogenically-induced trends in dissolved O_2, as suggested from both modeling [Frolicher2009] and observational [Mecking2008,Deutsch2011] studies.

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.
We agree and added “mostly" in the text.

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.
We checked a few locations before performing the analysis, using bottle data from individual

cruises.

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.

We added a sentence at p.10788, line 27:

Here, we focus our analysis on the upper mesopelagic layer (UML, from 100 to 600m of depth) . Within this depth range O2 is depleted the most – as a consequence of the O2 consumption associated with the remineralization of organic matter.

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.

We refer to Fig 1.b (grey and black symbols) and to the fact that the calculation of fCO2 involves different variables which are used to derive the different dissociation constant of the carbon system in seawater, which are combined in non linear ways during the calculation.

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

We renamed the section: “*Summary and discussion*”

Due to the length of the manuscript we prefer to avoid a further extension of the discussion.

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

For clarity we prefer to leave the paragraph as it is.

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

We deleted the statement.