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Biogeosciences
Discussions

Interactive comment on “Climate-mediated changes to mixed-layer properties in the Southern Ocean: assessing the phytoplankton response” *by* P. W. Boyd et al.

P. W. Boyd et al.

Received and published: 14 March 2008

Authors comment

We thank both reviewers for their constructive comments. Our study represents a departure from virtually all of the literature published so far on climate change and ocean biogeochemistry: studies have either focused on modeling aspects of climate change (either globally or regionally), or have tried to examine experimentally how oceanic biota will respond to changes in one or more environmental properties. Our intention was to try to merge this two distinct approaches to better inform the community on a) how climate change will impact the Southern Ocean, b) how this rate of change compares to the magnitude of change that has been employed in biological perturbation

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experiments, and c) how we can design better experiments to investigate the effects of climate change on oceanic biota.

Both reviewers had some issues with this new integrative approach to this subject. Reviewer #2 reported that the relationship between the modeling and experimental results sections of the manuscript were not always clear, that we did not relate all of the results in the modeling section to the biological section, and that the latter was like a review in places. In our revisions we have made the relationship between these two sections clearer. However, due to the paucity of data in the second part of the paper (for example on regional trends in the responses of the S. Ocean biota to environmental perturbation) it was not always possible to make direct links between all of the sections. In the revised version we have addressed reviewer #2's criticism of not making more of some of the modeling sections, by adding several new paragraphs to discuss the wider implications of these model simulations. The reviewers comment that the second section of the manuscript was more like a review is readily addressed. Our response is that although we present a significant amount of previously unpublished data, from my lab, (on iron, light, iron and light, and CO_2 perturbation experiments), due to lack of data for this region (something commented on by reviewer #1 see below), for the sake of completeness we have supplemented table 1b with additional data from the few other perturbation studies that have been conducted.

Reviewer #1 states that we need to explain various sections of the paper more carefully and clearly. This we have done during the revision of the manuscript. They report that table 1b has very few data, we agree but this only serves to highlight the need to conduct more (and better designed) perturbation experiments in this region.

Response to the reviewers' substantive comments on Boyd et al.

(Note to the editor – although this authors comment is supposed to precede the submission of a revised manuscript – we have gone ahead and revised the manuscript in order to deal with some of the general issues raised by both reviewers (which required

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alteration of text throughout the Discussion section).

The two reviewers for the Boyd et al. manuscript provided constructive comments on the manuscript, and we have revised the manuscript accordingly. Below we list the reviewer's comments (*in italics*) followed by our responses (in plain text).

Anonymous Referee #2

Received and published: 15 January 2008

In this study, the authors analyze, using a coupled carbon-ocean-atmosphere model, the changes in the physical and biogeochemical conditions of the Southern Ocean due to climate change. They compare these changes to the natural variability. Then, in a second step, they interpret the model results using the current understanding of the environmental control of phytoplankton growth. Their model results confirm previous findings in that the secular changes induced by climate change will be very subtle in the next few decades relative to the natural variability. As a consequence, the authors suggest that climate change will not produce the conditions necessary to induce the adaptation of the resident phytoplankton, at least for the next few decades.

To be totally honest, I don't really know what to say about this study. In fact, this paper can be divided into two quite well distinct parts. In the first part, the authors present results from a coupled global model focusing on the Southern Ocean. This section is rather interesting, especially because this is, to my knowledge, the first study focusing on the comparison between the natural variability and the climate change in a biogeochemical perspective. Unfortunately, I find it too rapid. Many results are presented but not really analyzed, neither used. For instance, figure 3 displays the spatial 2-D patterns of the natural variability and the response to the anthropogenic forcing. But this information is not used in the rest of the manuscript. Another example is table 3 which presents the co-variation between physical and biogeochemical property anomalies. The analysis of this table is extremely short and superficial (some variables correlate well, some others don't) and the information from this table is not used anywhere else

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in the study. Thus I would suggest either to shorten this first part to keep only the information really useful for the rest of the study or to extend the analysis. If the authors choose the second option, my advice would be to more clearly highlight the usefulness of the model results.

To address the reviewer's concern we have expanded the presentation of the model results in section 3.1 and clarified our interpretations. Much of the revised text is listed below in response to the specific issues raised by Reviewer #1.

In the second part, the authors use some of the results analyzed in the first part to try to infer what the consequences would be for resident phytoplankton in the Southern Ocean. Rather than being strictly speaking results, the authors use the current knowledge on phytoplankton growth/physiology to discuss on the potential response of phytoplankton to climate change in the Southern Ocean. This part is, like the first one, quite interesting because it reviews important aspects of the current knowledge in the perspective of a changing environment. However, to my opinion, this part is more a review than a discussion. Furthermore, the relationship with the first part is not always obvious. The only result that is used from the first part is the small anthropogenic change relative to the natural climate variability in the Southern Ocean. This second part is also sometimes quite hard to follow. For instance, section 4.2 is quite hard to relate to the rest of the discussion and is thus distractive.

We have strengthened the links between these two parts of the manuscript, and have revised and clarified section 4.2, and parts of section 4.3 of the Discussion section (see track changes version of the revised manuscript). The Discussion section has also been updated to take into account several new papers that have been published since our initial submission (see track changes version of the revised manuscript).

To conclude, I think that this paper presents some interesting results. However, the model results are to my opinion unsufficiently exploited. Furthermore, the discussion section would need to refocus on some few ideas rather than exposing interesting but

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sometimes hard-to-follow general concepts.

We have added text to better describe the model results and their broader implications (detailed below), and have rewritten the discussion section (see track changes version of the revised manuscript) to clarify our main points as requested.

Anonymous Referee #1

Received and published: 17 January 2008

General Assessment of Boyd et al

In this paper the authors run a coupled carbon climate model for the period 1820 to 2100 for the A2 SRES scenario, and then compare the difference with a control run in the Southern Ocean of various physical and ecosystem properties. Time series of these differences are presented for the period 2000-2100, and then maps of standard deviation for 10 years of the control of several properties are plotted as well as their change over 20 years (2020-2029 minus 2000-2009), where the difference is greater than the sd. Changes are generally greater (both in magnitude and relative to 1 sd) in subpolar regions than in polar regions. These differences are then compared with results from phytoplankton perturbation experiments. The general conclusion is that expected changes over the first 20 years of this century are for the most part small relative to expected natural variability, and also too subtle to be represented experimentally at present. Finally, our knowledge of the 'plasticity' of most organisms to slow change is inadequate. I agree with the general conclusions of the paper, but the case is not made very forcefully, mostly because the different parts of the paper need to be explained more carefully and more clearly. Especially the second part needs to be presented more clearly. Table 1b give little quantitative information (lots of 'No data' entries), and the discussion is very dense and needs more careful explanation. For example, writing out what 'CCM' means in words is inadequate; a few sentences clearly explaining the concept are needed.

Specific Science Issues

p 4290, l 1-5; I don't understand the 2x and 4x protocol. I think it means multiplying the thermal change by 2x and 4x, but it is stated that the change in CO2 for the A2 scenario is multiplied by 2x and 4x. This is clearly not the case in Figure 1, 4th panel on the left.

We have revised the text to clarify the experimental protocol.

page 4289 line 26-28 and page 4290, lines 1-5 expanded to:

"Therefore, we have also conducted two additional experiments for the period 2000-2100 where we have artificially increased the climate sensitivity of the CSM1.4-carbon simulation to span the range observed in other COAM's. In these experiments, the atmospheric radiation calculations see a higher effective CO_2 concentration than seen by the land or ocean biogeochemistry. Specifically, an atmospheric CO_2 perturbation above pre-industrial levels, DCO_2, is computed as:

$DCO_2 = CO_2^{model} - CO_2^{preind}$

The anthropogenic CO_2 perturbation is multiplied by a factor of 2 and 4 for cases A2-x2 and A2-x4, respectively, and then add back in the preindustrial concentration to find the CO_2 concentration fed to the model atmospheric radiation code:

$CO_2^{radiation} = 2 \text{ x } DCO_2 + CO_2^{preind}$; $CO_2^{radiation} = 4 \text{ x } DCO_2 + CO_2^{preind}$

Thus the climate sensitivity to the anthropogenic CO2 perturbation is effectively enhanced while not directly altering the atmospheric CO_2 used for air-sea exchange, CO_2^{model}."

p 4290, l 12; it would be good somewhere either to give the approximate latitude of the 130 Sv boundary, or to plot it on 1 panel of Fig. 3. Did the boundary change with time? There is evidence that atmospheric southern annular mode has moved poleward and will continue to do so in this century. Maybe the boundary between polar and subpolar

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waters will follow (see various papers by John Fyfe of CCCMA/University of Victoria, Canada and also AR4).

The following text replaces the text on page 4290, line 9-13:

“The climate change signals in the S. Ocean of the CSM1.4-carbon simulation are approximately (though not exactly) zonal, and therefore we have partitioned the S. Ocean into polar and subpolar waters based on frontal structure. We set the boundary as the simulated 130 Sv stream function that approximates well the boundary between the two water masses in the control simulation. The boundary is prescribed from the control simulation stream function and varies from 60-65 deg. S in the Pacific sector to 45-50 deg. S in the Atlantic sector downstream of the Drake Passage. The model ocean stream function evolves with time under climate change because of two factors, an increase in strength and a poleward contraction of the zonal surface wind maximum in the Southern Ocean associated with a shift toward more positive phase of the Southern Annular Mode (Russell et al., 2006; Le Quere et al., 2007; Lovenduski et al., 2007). The effects of the two processes partially counteract and the lateral shift for the 130 Sv streamline is minimal. In our analysis the subpolar/polar boundary is not allowed to vary through time, which would complicate the comparison with time because it would confound temporal and geographical variations.”

Three references are added on Southern Annular Mode variations:

Le Qu’er’e, C., R¨odenbeck, C., Buitenhuis, E. T. Conway, T. J. Langenfelds, R. Gomez, A. Labuschagne, C. Ramonet, M. Nakazawa, T. Metzl N., Gillett, N. and Heimann M.: Saturation of the Southern Ocean CO2 Sink Due to Recent Climate Change, Science, 316, 1735–1738, doi:10.1126/science.1136188, 2007.

Lovenduski, N.S., Gruber, N., Doney S.C., and Lima, I.D.: Enhanced CO_2 outgassing in the Southern Ocean from a positive phase of the Southern Annular Mode, Global Biogeochem. Cy., 21, GB2026, doi:10.1029/2006GB002900, 2007.

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Russell, J.L., Dixon, K.W., Gnanadesikan, A., Stouffer, R.J., and J.R. Toggweiler, J.R.: The Southern Hemisphere westerlies in a warming world: Propping open the door to the deep ocean, J. Climate, 19, 6382–6390, 2006.

Figs 1, 2 vs Fig. 3; From Fig. 1 the polar MLD is expected to shoal and stratification to increase over this century. But from Fig 3, much of the change in MLD appears to be orange, i.e. positive, and stratification decreasing. Are the signs correct, or is this a short term deviation in the model results? If the signs are correct then the authors need to comment.

To address the reviewer's comments, the results section has been expanded to clarify our findings and interpretation:

page 4291, line 9-20 we revised and expanded the text as follows:

“The CSM1.4-carbon results are broadly similar to other COAM simulations that exhibit surface warming of Southern Ocean waters in response to anthropogenic climate change. While the anthropogenic warming signal is quite clear in the CSM1.4-carbon A2 case by the end of the 21st century, the warming trend earlier in the century is less apparent because of natural variability, particularly over shorter time-spans of a couple of decades relevant to the establishment of ocean observing systems. In the low climate sensitivity A2 case, the decadal mean sea surface temperature (SST) in the subpolar region increases by +0.10 K/decade for a 20-year period (model years 2020–2029 minus 2000–2009) (Figs. 1–3; Table 1a). For comparison the root mean squared (rms) variability of the subpolar mean SST on interannual scales is 0.11 K. Given a sampling duration of two to three decades, therefore, the anthropogenic SST warming trend is thus detectable, though somewhat marginally, above the natural interannual variability for the subpolar region. The secular warming trend is less pronounced in polar waters closer to Antarctica in the A2 case, about +0.03 K/decade, and the climate change temperature signal does not clearly exceed natural variability (rms 0.13 K) until roughly model year 2060.

The anthropogenic warming trends are larger for the A2-x2 and A2-x4 higher climate sensitivity cases. The decadal subpolar warming trend in A2-x4 (+0.36 K/decade), for example, is more than 3 times the trend in the A2 case. For the A2-x4 case, the polar temperature rise (+0.14 K/decade) becomes significant relative to natural variability on the decadal scale, and the secular warming signal becomes detectable by model year 2020."

Page 4992, line 25, added the following text:

"Both anthropogenic warming and interannual climate variability influence the spatial pattern of regional surface ocean anomalies. The right-hand column of Figure 3 displays spatial maps for difference in simulated surface property between model decade 2020-2029 minus decade 2000-2009 for the A2 case. In the model simulation a zonal band of warmer SSTs occurs in the subtropics and subpolar waters, and a large area of cooling is found in the polar Atlantic sector. The Atlantic regional cooling illustrates an important point that on sub-basin scales interannual climate can slow or even reverse for individual sampling periods the anthropogenic warming impacts that are apparent at larger basin and global scales. To focus on the longer term trends in the decadal difference maps in the right-hand column, areas are masked with white if the observed temporal differences are small relative to rms interannual variability.

The magnitude of the interannual variability (rms of annual means) in model surface properties is displayed in the left-hand column of Figure 3. For SST, the regions of maximum interannual variability occur along frontal boundaries, in particular for the example shown in Figure 3 in Pacific sector along roughly 60 deg. S and in the Atlantic sector east of South Georgia Island near 30 deg. W. The Southern Annular Mode (SAM) is a significant contributor to ocean interannual variability in the Southern Ocean that reflects the strength of the atmospheric pressure low over the Antarctic continent and the high in the subtropics and subpolar region. A positive SAM occurs with an intensification of both the Antarctic low and subtropical high, resulting in field observations and models in stronger westerly winds, increased upwelling, cooler SSTs

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in polar waters and subpolar central Pacific and warmer SSTs elsewhere in the Southern Ocean (e.g., Lovenduski et al., 2007). The CSM1.4 model decadal SST anomaly patterns with warming of the subpolar zone and neutral or cooling of the polar zone are similar to those observed for a shift to a more positive SAM. The SST patterns along with the increased surface winds and upwelling in the simulation are consistent with the argument that anthropogenic warming in the Southern Ocean will be expressed, in part, through a projection onto an increasing SAM (Le Quere et al., 2007)."

page 4293, lines 4-13 we modified the text as follows:

"For most of the other physical and biogeochemical factors in the CSM1.4 A2 case, however, the climate change signal is smaller than or comparable to natural variability on the decadal time-scale (2020–2029 minus 2000–2009), making it difficult to identify anthropogenic signatures. Further, on the sub-basin and basin scale there are regions exhibiting both positive and negative changes over decadal time periods. This is illustrated in the spatial difference maps (decade 2020-2029 minus decade 2000-2009) for case A2 displayed in Figure 3. For example, the differences maps of mixed layer depth and stratification show no distinct patterns of the climate warming signal evident later in the century—reduced mixed layer depth in polar waters due to freshening, increased mixed layer depth in subpolar waters due to stronger winds and increased vertical stratification in both polar and subpolar waters due to surface freshening and warming, respectively (see temporal trends in Figures 1 and 2).

The large white regions in both maps indicate model points where the decadal changes in mixed layer depth or stratification are so small that they are not statistically significant relative to the model interannual variability. The simulation also exhibits nearly as many areas of decreasing stratification (green/blue) as increasing stratification (yellow/orange). In fact, based on the time-series plots averaged over the subpolar and polar regions, a definitive basin-scale climate warming signal or elevated stratification for the A2 case would not be observable until roughly 2040 (polar) and 2070 (subpolar); the threshold for detecting shallower mixed layer depth is also about 2040 for polar waters and is not found by the end of the 21^{st} century in subpolar waters.

The anthropogenic climate signals become more distinct for the in the A2-x2 and A2-x4 cases, with increased climate sensitivity for the following properties: decreasing polar sea surface salinity, increased subpolar and polar stratification, decreased subpolar and polar surface dissolved iron concentrations, poleward shift and increased strength in polar upwelling, and increased polar surface ocean irradiance due to decreased ice fraction. In higher climate sensitivity cases, the date at which the anthropogenic signal is detectable from the natural background also occurs earlier in time, often by decades.

Page 4295, line 13-24, text modified to:

“The climate-change driven trends in surface water properties in the Southern Ocean do not occur independently, and synergistic effects need to be accounted for in phytoplankton responses. One approach that has been advocated is to use biological responses to interannual physical/chemical variability as an analogue for climate change (Boyd and Doney, 2002); but this line of reasoning depends upon the character of the interannual physical/chemical variability being similar to that of climate change, including property-property relationships. Table 3 presents property-property linear regressions (regression slopes and correlation coefficients) for surface ocean temperature, mixed layer depth and nutrient anomalies for both interannual variability (annual means from control simulation) and anthropogenic climate change (A2 case, decadal means of 2020-2029 minus 2000–2009; Fig. 3). For natural interannual variability, there are statistically significant correlations of warmer SSTs, shallower mixed layers and lower surface nutrient concentrations; surface iron and phosphate also are well correlated. The patterns differs considerably for climate change. The signs of the climate change and interannual variability SST-surface nutrient correlations are the same, but the regression slopes (∂ nutrient/ ∂ SST) tend to be lower for climate change (except for polar ∂ Fe/ ∂ SST). Under climate change, the correlations of subpolar mixed layer anomalies to other properties are weak and not statistically significant. In polar waters, the variations of mixed layer depth to temperature are larger, and there is a factor of

4 reduction in the magnitude of the ∂ PO$_4$/ ∂ Fe slope relative to the same value for interannual variability. Overall, the property-property correlation analysis suggested that interannual variability is only a weak analogue for climate change with respect to synergistic and non-linear interactions across multiply variables.”

p 4292, l 22; missing word(s) ’ by other coupled simulated, the model ’

text has been added to rectify this issue

p 4292, l 24 ’for the next 20 years’ seems vague. Maybe ’over the next 20 year period from present’.

Amended

p 4294, l 4: if the Kfe in the model is 30 pmol/L, then for the most part, there will be little iron limitation in the model if iron levels in the model don’t drop below 96 pmol/L for even the 4xA2 scenario as indicated in Figs. 1 and 2. According to Table 1b, for the most cases, the Kfe used is also lower than in laboratory studies. The authors need to comment here.

We concur with the reviewer - that in this simulation with the KFe used here (0.03 nmol/l) may not entirely reflect the degree of iron limitation that occurs, on average, in the Southern Ocean. For example, at the [Fe] of 0.11 nmol/l for the average of the control simulation in the subpolar zone, the limitation term is 0.79 (or a 21% reduction in net community production). In the strongest climate change simulation, [Fe] is reduced to $\sim$0.096, resulting in a limitation term of 0.76. We now make these points in the Discussion section (evident from track changes).

We disagree with the reviewer’s interpretation of Table 1b, where the “free iron” levels from laboratory experiments are orders of magnitude lower that that used in the model to set KFe. However, there is considerable difficultly in comparing free iron concentrations with the model “bioavailable” iron. We have added text to detail this issue in the Discussion (evident from the track changes version of the revised manuscript).

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Section 3.2 is dense (many refs, little exposition) and needs clearer explanation. Probably 'Q10' needs to be explained the first usage, as does 'CCM'.

We have extensively modified section 3.2 with a view to clarity. We have added the following text to define these terms " Q10 is defined as a temperature coefficient which provides a measure of the rate of change of a biological or chemical system due to a 10 ˚C increase in temperature.

"A carbon concentrating mechanism (CCM) permits phytoplankton to compensate, via active intracellular accumulation, for the large difference (ranging from 5- to 20-fold) between the CO_2 concentration in the surrounding waters with that within their cellular machinery (Kaplan et al., 1991)."

Kaplan, A., Schwarz, R., Lieman-Hurwitz, J., and Reinhold L.: Physiological and Molecular Aspects of the Inorganic Carbon-Concentrating Mechanism in Cyanobacteria, Plant Physiol., 97, 851–855, 1991.

p 4296, l 13 need an 'e.g.' before 'icemelt, Q10'

amended

p 4298, l 4 ' Polar diatoms generally 8230;. than for P. antarctica'. Need to add 'a species of Phaecystis'

amended

p 4299, l 4: here the authors emphasize "the largest climate change signal" CO2 but Table 1b only mentions 'CCM', and a reference to Riebesell et al. 2000. What about Riebesell, 2007 in Nature, where they found basically a CO2 fertilization effect for mixed communities of phytoplankton grown in enclosures in Bergen harbour, presumably also (sub)polar species?

We have removed the reference here to Table 1, and added some text here that identifies the magnitude of CO_2 perturbations commonly used in bottle (Tortell et al., 2008) and mesocosm (Riebesell et al., 2007) experiments.

The Riebesell et al. (2007) study, which we now cite, is indeed pertinent, however as the focus of the present study is on S. Ocean phytoplankton and climate change it is difficult to relate their findings (at a community level) to the polar species we discuss (i.e. no sub-polar data were presented in Table 1b). However, we do point the reader to this study and one by Hutchins et al. in JGR which are two of the very few studies on the perturbation of environmental conditions in experiments using phytoplankton from subpolar waters.

p 4300, l 9: write out 'SOI'

it is now written in full "Southern Oscillation Index"

p 4300, l 19: it is true that natural interannual and seasonal variability is comparable with expected secular climate trends, but the point is missed here that extreme values will change because the natural variability will be superimposed on the secular climate trend. In terms of temperature, if the secular change is say 0.5C, and the expected 1sd deviation is 0.6C, then that deviation will be 0.5C higher, to the extent that the changes are linear and superposable.

We have altered the wording in this paragraph, since using the phrase:

"the magnitude of change in oceanic properties due to climate variability is comparable to that by climate change," (page 4300, line 13-14)

understates the problem. Our revised text (see track changes) now points out that for many of the variables, the magnitude of interannual variability is much larger than the long-term, secular climate change trend. In some cases, particularly for the low climate sensitivity A2 case, there is not a noticeable shift in the extremes for several more decades.

Table 3: what is 'Hmix'? change in MLD? Also the structure of the Table is inconsistent. 'Interannual Variability' should be moved from being a heading to be the first line of

the body of the table, like ’Climate Change’. Maybe ’Subpolar’ and ’Polar’ should be removed from the line starting with ’Climate Change’.

The variable “Hmix” is indeed Mixed Layer Depth “MLD”, and we have modified the table accordingly. The headings are clarified in the revised table as well.

The following additional changes have been made:

p. 4307, line 6-7: modify text in the acknowledgements, “WHOI Ocean and Climate Change Institute and a grant from the National Science Foundation (NSF ATM06-28582).

Table 1a:

Change Temperature range in “rate of change subpolar waters” from “+0.10 to +0.31” to “+0.10 to +0.36”

Change Temperature range in “rate of change polar waters” from “+0.03 to +0.17” to “+0.03 to +0.14”

Interactive comment on Biogeosciences Discuss., 4, 4283, 2007.

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