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Discussions

Interactive comment on "Imminent ocean acidification projected with the NCAR global coupled carbon cycle-climate model" *by* M. Steinacher et al.

M. Steinacher et al.

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We would like to thank both reviewers for their constructive comments. We have followed their suggestions in most cases. Please find below the detailed response (bold/italic font) to the comments of anonymous referee 1 (normal font).

- Arctic publication. Three of the authors also co-author a submitted paper focussing on the Arctic Ocean. Though I do trust the authors that no duplicate material is used in the present work it would be good that similarities and differences between the two studies are explicited.

The following two text passages in the introduction have been modified to clarify the similarities and differences between this study and Orr et al. (subm.):

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Page 4355, lines 17-22:
The emphasis here is on the Arctic Ocean, where large changes in $CaCO_3$ *saturation (Gangstø et al., 2008; Steinacher, 2007; McNeil and Matear, 2007; Orr et al., subm.), freshwater balance, and sea ice (Stroeve et al., 2007; Holland et al., 2006) are expected under rising* CO_2*.*
Page 4358, lines 9-17:
In a recent assessment, Orr et al. (subm.) show by combining observations and model results that surface waters in the Arctic will become undersaturated within decades. They present an overview on the evolution of the saturation state in the Arctic with respect to both aragonite and calcite as inferred from three coupled models, including the NCAR CSM1.4-carbon.

Here, we complement earlier studies by using the fully coupled NCAR CSM1.4-carbon model to investigate the evolution of Ω_{arag} *over the 21st century for the SRES A2 and B1 scenarios. In comparison to earlier work, we present a detailed analysis of the changes in the Arctic, including a quantification of underlying mechanisms, assess the global evolution of the ocean volume for different saturation regimes, and analyse spatio-temporal variability in saturation.*

Further, references to Orr et al. (subm.) have been added on lines 8 and 24 at page 4358.

- Carbonate chemistry. There is no description of the method used for the computation of K'_{sp} and of the pressure dependency of chemical constants. Incidentally the appropriate reference for OCMIP routines is not Orr(subm.).

Another issue concerning carbonate chemistry is the way SiO_4 is treated. Silica intervenes in the expression of alkalinity but its role is not essential. Since the model does not include silica it would be better not to consider it (neither in the model-derived nor in the data-based formulations). By mixing the WOA01 SIO_4 climatology with model results the authors introduce more bias in their chemistry than they eliminate. The reasons are twofold. First any tracer distribution that would be coherent with the model hydrodynamics most probably differ from that provided by WOA01 (the correlation and relative standard deviation would be very different from 1). Second, considering that SIO_4 keeps its present-day distribution despite the profound changes that could occur throughout the 21st century is not appropriate. One could argue that the impact of the authors method on the actual values of chemical variables is small but this method is nevertheless not scientifically sound.

References for the calculation of K'_{sp} and the pressure dependency have been included. Also, the reference for OCMIP routines has been changed.
Regarding the treatment of silica, we don't fully agree with the referee. Silica is part of total alkalinity and setting $Si(OH)_4$ concentrations to a constant value everywhere would lead to a systematic bias as well. Further, the changes in ocean circulation throughout the 21st century are relatively weak. Therefore, it is not clear which method would be better and we believe that using the present-day silica distribution is justifiable. In any case, as shown by Steinacher (2007), the effect of changes in $Si(OH)_4$ concentrations on the calculation of carbonate ion concentration and saturation state is small. In a sensitivity analysis, silicate concentrations have been either set to zero, increased uniformly by 20 μmol/l (which corresponds to 10-90% do the original concentration), or doubled. These changes lead to local differences in [CO_3^{2-}] and Ω_{arag} of 0.4±0.2%, 0.5±0.1% and 0.6±0.3% respectively; maximum values are around 1%. This is now mentioned in the text. Further, the second part of the paragraph has been reformulated:

"Carbonate chemistry, pH, carbonate ion concentration, and the saturation state have been calculated offline from modeled or observation-based quantities using the standard OCMIP carbonate chemistry routines (http://www.ipsl.jussieu.fr /OCMIP/phase3/simulations/ NOCES/HOWTO-NOCES-3.html). In these routines, based on work by Dickson (2002), Millero (1995), and Mehrbach et al. (1973), the

total pH scale (Lueker et al., 2000) is utilized. The apparent solubility product K'_{sp} *is calculated after Mucci (1983) and the pressure dependency of chemical constants after Millero (1995). The observation-based carbonate variables were computed from monthly, seasonal, or annual means, depending on data availability. Annual means have been used for Alk and DIC, as well as for* PO_4^{3-} *and* $Si(OH)_4$ *below 500 m. Seasonal means of T and S have been used below 1500 m. For the data-model comparison, annual averages from the simulated monthly carbonate data from 1990 to 1999 were used. Because the CSM model does not include* $Si(OH)_4$*, a seasonal cycle of observation-based* $[Si(OH)_4]$ *from WOA01 has been used in all calculations of simulated pH and calcium carbonate saturation state. Uncertainties in* $[CO_3^{2-}]$ *and* Ω *arising from this treatment of* $Si(OH)_4$ *have been estimated to be less than 1% (Steinacher, 2007)."*

- The difference in distributions of $[CO_3^{2-}]$ and Ω_{arag} in the Taylor diagram (Fig. 2) is striking. I suppose this is a consequence of the model performances at reproducing the T and DIC (and may be S) fields. It would be worth investigating the reasons for such a difference. If available such an analysis would prove useful for model evaluation as well as provide indications of confidence levels in the predicted changes. For a better understanding similar plots for DIC, T and S should be provided. They would help the reader appreciate the model performances.

The difference between $[CO_3^{2-}]$ and Ω_{arag} in the Taylor diagram is mainly a consequence of the pressure-dependent solubility product K'_{sp}. The relationship between data-model differences in Ω and in $[CO_3^{2-}]$ is approximately

$$\Omega_{\mathrm{mod}} - \Omega_{\mathrm{obs}} \approx \frac{[CO_3^{2-}]_{\mathrm{mod}} - [CO_3^{2-}]_{\mathrm{obs}}}{K'_{\mathrm{sp}}}. \quad (1)$$

K'_{sp} increases with depth. This implies that data-model differences in Ω are less heavily weighted at depth than at the surface relative to data-model differences

in [CO_3^{2-}]. Consequently, the model-data comparison is more favorable for Ω than for [CO_3^{2-}] when data from the entire water column are compared with observations as model-data deviations in [CO_3^{2-}] are larger at depth than at the surface as shown in Fig. 3. Almost no difference is found in the Taylor diagram statistics between Ω and [CO_3^{2-}] when only surface values are considered where variations in K'_{sp} are small (dark blue symbols in Fig. 2a). This is now mentioned in the text:

"Correlation coefficients are somewhat smaller (0.86 to 0.91) for $[\mathrm{CO}_3^{2-}]$ *than for* Ω_{arag} *because the data-model differences in* Ω_{arag} *decrease with depth relative to the differences in* $[\mathrm{CO}_3^{2-}]$*. This results because the increase in the pressure-dependent solubility product* K'_{sp} *with depth decreases the weighting of the larger errors in deep-water* $[\mathrm{CO}_3^{2-}]$ *(Fig. 3)."*

Further, a similar Taylor diagram has been added for DIC, T and S fields (Fig. 2b).

- Global evolution of pH and Ω_{arag} arag. Previous works (Orr et al., 2005; Cao et al., 2007; McNeil and Matear, 2007) conclude in a weaker effect of climate on pH than on Ω_{arag} by 2100 A.D. This is in contrast with the present study in which the impact on both pH and Ω_{arag} is of the same order of magnitude (page 4363, lines 26-28). On page 4370 the authors suggest that one possible explanation is that McNeil and Matear (2007) used a prescribed CO_2 concentration scenario rather than a CO_2 emission scenario. This is in contradition with the results of Cao et al. (2007). Indeed Cao et al. (2007) performed experiments with both constrained and prognostic atmospheric CO_2. In both cases the pH relative changes do remain smaller than the relative changes of Ω_{arag} (Table 1 in Cao et al. (2007)). I do not see any reason why an emission scenario rather than a concentration scenario would lead to different relative behavior in pH and Ω. The reason for the differences among the above-mentionned studies must lie in the ocean processes. One exploratory path could be to reproduce Fig. 6 from Mcneil and Matear (2007) with the present model results and look for differences.

We agree that our results are not fully consistent with the results of Cao et al. (2007) in terms of magnitude. However, the opposite behavior of pH decrease (enhanced by climate change) and Ω_{arag} decrease (reduced by climate change) is consistent. This has been clarified in the text on page 4370, line 9:

"This opposite behavior is consistent with the analysis of Cao et al. (2007) for carbon emission scenarios, although they found a weaker effect on pH than on Ω_{arag} *in their study with an Earth System Model of Intermediate Complexity."*

The difference between using an emission scenario rather than a concentration scenario has already been discussed by Cao et al. (2007, paragraph 13): "McNeil and Matear [2006] reports that the effects of climate change on surface ocean pH are negligible from a coupled climate-carbon cycle simulation driven by the IS92a atmospheric CO_2 concentration pathway. With prescribed CO_2 concentrations we project negligible climatic effects on surface pH (Table 1), consistent with their study. In this case, the indirect DIC effect almost cancels the direct temperature effect (not shown), leading to a negligible net climatic effect on pH. However, with prescribed CO_2 emissions, we find that consideration of climate change has a pronounced effect on surface pH (namely, to cause a greater decrease in pH, as seen from Figure 2c); the direct temperature effect dominates the indirect DIC effect (as explained above) (Figure 3a)."
A reference to Cao et al. (2007) has been added on page 4370, line 13:

Mcneil and Matear (2007) found a climate-change feedback reducing the change in Ω_{arag} *by about 15% but almost no feedback on pH; they applied a scenario with prescribed* CO_2 *concentration and not a scenario with prescribed carbon emission as done here (see Cao et al. (2007) for a discussion of differences between emission and concentration scenarios).*

- Changes in the Arctic (pages 4366 and 4367). The combination of model and data

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such as performed here implies that the model bias is and would remain linear. Isn't this assertion at odds with the non-linearity of the carbonate chemistry? I would surely not state as the authors do on page 4367 that "... the emerging undersaturation of the surface Arctic Ocean is a robust feature and independent of these model biases". Since the reasons for the bias are not clearly elucidated there are no reasons to believe that the evolution of the aragonite saturation would be that predicted by the model. The reasons for the model bias could result in non-homogeneous bias to occur with time.

We have compared the effect of adjusting the individual components (T, S, DIC, Alk, PO$_4$) instead of correcting Ω directly and we have found a small effect ($<$1%). This is mentioned in the text on page 4366, line 21 and rules out problems with the non-linearity of the carbonate chemistry. We agree that a potential non-homogeneous bias with time can't be entirely eliminated by this method. However, while it is true that we do not fully understand the reason for the model bias, we do well understand the driving forces behind the decreasing trend in saturation. This is the increase in atmospheric CO$_2$ by anthropogenic emissions and the penetration of this perturbation into the ocean. The statement "...is a robust feature and independent of these model biases" on page 4367 refers to the fact, that the emerging undersaturation of the surface Arctic Ocean is projectd with and without model-data correction.

- Results presentation. A first remark is that the titles of subsubsections 3.3.1 and 3.3.2 contradict that of the parent subsection 3.3. Also subsubsection 3.3.2 is quite important in size and subject. It should deserve to be discussed in a subsection of its own. Further some global aspects discussed on page 4363 are again addressed at the end of section 3.3.2. Lines 7-20 on page 4370 should be gathered with lines 22-28 on page 4363. Some reorganization of section 3. would improve the readability of the manuscript. The discussion about the Arctic ocean should be separated and material from 3.3.1 and 3.3 should be merged with that in 3.2. I suggest something like this:
3.1 Comparison of modeled aragonite saturation and CO3 concentration with

observation-based estimates
3.2 Projected global and regional changes
3.3 Seasonal and interannual variability of Ω_{arag}
3.4 Changes in the Arctic Ocean and climate feedbacks

There was a mistake in the subsection numbering. We apologize for the confusion. The numbering is now as follows:

3.1 Comparison of modeled aragonite saturation and CO_3^{2-} concentration with observation-based estimates
3.2 Projected global mean changes
3.3 Regional changes in saturation at the surface
3.4 Changes at depth and in the volume of supersaturated waters
3.5 Changes in the Arctic Ocean and climate feedbacks
3.6 Seasonal and interannual variability of Ω_{arag}

As suggested by the referee, the global aspects on page 4363 (last two paragraphs) have been merged with the text in section 3.2 (second paragraph) and section 3.3 (last paragraph).

- Abstract, lines 11-12. I am in favour of reporting pH changes in pH units rather than in hydrogen ion concentration changes. pH units usually carry more meaning for the reader.

The corresponding pH change (ΔpH = -0.45) has been added.

-Abstract, lines 15-16. I do not understand the sentence "Aragonite undersaturation in Arctic surface waters is projected to occur locally soon and to become more widespread as atmospheric CO_2 continues to grow."

The sentence has been changed to include a more precise statement on the time

of the onset of undersaturation:

"Aragonite undersaturation in Arctic surface waters is projected to occur locally within a decade and to become more widespread as atmospheric CO_2 *continues to grow."*

In addition, we have changed the manuscript title to: *"Imminent ocean acidification in the Arctic projected with the NCAR global coupled carbon cycle-climate model"*

- Use of adjective "alkaline" (p. 4354, line 23 and p. 4356, line 17): in the everyday language the word alkaline is often used as a synonymous for base. I would recommend not to use this word in the present context mainly because it may confuse the reader and let her believe the authors refer to an alkalinity change rather than to a pH modification but also because of the fact that not all bases are alkali.

The two sentences have been changed to:

"The continued emissions of CO_2 *by human activities cause atmospheric* CO_2 *to rise, climate to warm, and the ocean pH to decrease."* **and** *"The hydrolysis of* CO_2 *in seawater lowers ocean pH, making the oceans less basic."*

- p. 4356, line 9. Wouldn't fertility be more appropriate than fertilization?

Yes. This has been changed.

- p. 4356, line 10. "... life stages".
- p. 4357, line 8. "... biogenic production and dissolution of CaCO3 are mainly controlled..."

Done.

- p. 4357, line 22. It is often written, as the authors do, that "coccolithophores are a major contributor to the open-ocean carbonate pump",but what is the actual percentage of the rain attributable to coccolithophores?

This statement has been removed. The sentence now reads "Such changes

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could have important implications for...".

- p. 4359, line 15. Is the too extensive ice cover in NP and NA a cause or a consequence of the model shortcoming? As formulated the sentence implies the first!

This text passage has been reformulated (see answer to Referee 3).

- p. 4361, 1st paragraph. Couldn't this paragraph be reformulated in a more concise way?

This paragraph has been reformulated (see answer to specific comment above).

- p. 4361, line 23. What is it meant by "this specific section"?
- p. 4361, lines 23-24. A more proper formulation would be "..., the Artic Ocean is defined to be waters north of 65°N, except ..." (the basin index of CSM1.4 model is of no interest for the reader).

This sentence has been changed to:

"When not referring to the specific transect where observations are available, the Arctic Ocean is defined to be waters north of 65° N, except the Labrador Sea and the Greenland, Iceland, and Norwegian (GIN) Seas (<80° N and 35° W-18° E)."

- p. 4362, lines 14-15. There is an incoherency in the formulation: "Surface [CO_3^{2-}] is approximately proportional to the difference Alk-DIC. Consequently, the nutrient and carbon rich water of the North Pacific thermocline ... ".

The word 'Surface' has been removed.

- p. 4364, line 19. The years given here do not correspond to the values in Fig. 6.

This is correct. The years given on page 4364, line 19 refer to the time when mean Ω_{arag} (averaged over the entire Arctic) becomes less than unity, whereas the dotted line in Fig. 6 marks the onset of surface undersaturation (zonal average at some latitude), which occurs earlier. We have clarified the caption of Fig.

6 (see comment below) and the sentence on page 4364, line 17:

Averaged over the entire Arctic, surface annual mean Ω_{arag} *becomes less than unity in the model when atmospheric* CO_2 *exceeds 490 ppm (2040 A.D. in A2 and 2050 A.D. in B1).*

- p. 4364, lines 22-23. "..., high latitude surface waters poleward of about 50° are projected to be undersaturated under the A2 scenario".
- p. 4365, line 20. decades, not deceades.
- p. 4366, line 25. "... the model-only projection also shows ..."
- p. 4369, line 7. "Considering all fluxes, (Alk-DIC) ..."
- p. 4374, line 2. "Our model predicts that water with a saturation ..."
- p. 4374, line 4. "... and will be gone ..."

All done.

- p. 4374. Lines 14 to 19 should be reformulated. The meaning of the sentence referring to the time series station is not clear.

This passage has been reformulated: *"Interannual variability in saturation is found to be small. These findings are consistent with results obtained at time series stations (HOT, BATS), which show that the anthropogenic signal is clearly distinguishable from seasonal and interannual variability."*

- Figure 4. Wouldn't it be possible to organize this figure so that pannels (a) to (d) would be wider? This would facilitate the reading.

The map showing the transsect has been moved to a separate figure (new Fig. 2) to improve the layout.

- Figure 5. Could be suppressed.

We would like to keep that figure for completeness.

- Figure 6. Pannel (b) of Fig. 6 is not really discussed in the text. I would suggest that

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pannel (a) be also drawn for scenario B1 in replacement of Fig. 5 and pannel (b) of Fig. 6.

Since the figure shows Ω_{arag} as a function of atmospheric CO_2 and there is virtually no difference between A2 and B1 at the same CO_2 level, we see no reason to add a second plot for scenario B1. Instead, the corresponding years in B1 have been added at the top of the plots. The following sentence refering to Fig. 6b has been added to the text on page 4364:

"Depending on the seasonal amplitude, short-time undersaturation during at least one month is reached several years earlier in many regions (Fig. 6b)."

Further, seasonality and annual minumum values of Ω_{arag} are discussed later in the text, therefore Fig. 6b could be interesting as a reference.

-Figure 6. An explanation of the meaning of the dotted lines is missing in the caption.

The caption has been modified to:

*"Projected evolution of the **(a)** annual-mean and of the **(b)** lowest monthly mean zonally-averaged aragonite saturation Ω_{arag} for the SRES A2 scenario (model only). The evolution is plotted as a function of the annual-mean atmospheric CO_2 mixing ratio at the ocean surface. The corresponding years in the A2 and B1 scenarios are given at the top. The dotted line indicates the transition from supersaturation to undersaturation in zonal average Ω_{arag} at 77° N by 2032 and 2016, respectively."*

- Figure 7. The global average is dominated by the Pacific Ocean. I would suggest that four pannels be drawn : Atlantic, Pacific, Arctic and Southern Ocean.

We agree. The global average has been replaced with plots of the Atlantic and Pacific.

- Figure 7. The caption says "... annual mean aragonite supersaturation ΔCO_3^{2-}"; shouldn't it be "... annual mean ΔCO_3^{2-}". Why not produce time-depth diagrams of Ω

which is mostly discussed in the text rather than ΔCO_3^{2-}?

It is important to state that we refer to aragonite saturation, because ΔCO_3^{2-} is different for the several mineral phases. The caption has been modified to:

"... annual mean $\Delta[CO_3^{2-}]$ (μmol/l) with respect to aragonite ..."

We intentionally show ΔCO_3^{2-} in all figures with meridional sections to provide an alternative to Ω for those who are more interested in (excess) concentrations.

- Figure 8, caption line 6. " ... decrease to 25% and 70% with respect to the preindustrial values by 2100."

Done.

- Figure 12. Wouldn't it be possible to re-organize the figure into 2 rows (and 3 columns) rather than 3 for better readability?

We will re-arrange this figure during the production process (and possibly others too) to fit the final layout of the paper as well as possible.

References:

Cao, L., Caldeira, K., and Jain, A. K.: Effects of carbon dioxide and climate change on ocean acidification and carbonate mineral saturation, Geophys. Res. Lett., 34, L05607, doi:10.1029/2006GL028605, 2007.

McNeil, B.I., and Matear, R.J.: Climate Change Feedbacks on Oceanic pH. Tellus, 59B, 191-198, 2007.

Orr, J. C., Fabry, V. J., Aumont, O., Bopp, L., Doney, S. C., Feely, R. A., Gnanadesikan, A., Gruber, N., Ishida, A., Joos, F., Key, R. M., Lindsay, K., Maier-Reimer, E., Matear, R., Monfray, P., Mouchet, A., Najjar, R. G., Plattner, G.-K., Rodgers, K. B., Sabine, C. L., Sarmiento, J. L., Schlitzer, R., Slater, R. D., Totterdell, I. J., Weirig, M. F., Yamanaka, Y., and Yool, A.: Anthropogenic ocean acidification over the twenty-first century and its

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impact on calcifying organisms, Nature, 437, 6818211;686, 2005.

Orr, J. C., Jutterström, S., Bopp, L., Anderson, L. G., Fabry, V. J., Frölicher, T. L., Jones, P., Joos, F., Maier-Reimer, E., Segschneider, J., Steinacher, M., and Swingedouw, D.: Arctic ocean acidification, Nature, submitted.

Steinacher, M.: Ocean acidification and changes in marine productivity in simulations with the fully coupled 3-D climate model CSM1.4-carbon, Master8217;s thesis, University of Bern, Bern, Switzerland, 2007.

Interactive comment on Biogeosciences Discuss., 5, 4353, 2008.

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