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Comment

Interactive comment on “Quantifying the influence of CO₂ seasonality on future ocean acidification”

by T. P. Sasse et al.

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This paper presents an assessment of how seasonality in surface ocean carbonate chemistry will affect future saturation states of the calcium carbonate aragonite. The authors combine a monthly climatology for omega (from DIC and TA) with decadal trends from 6 ESM projections. The authors extend in scope earlier studies by e.g. McNeil and Matear (2008). The topic is of considerable scientific interest. The article is well written. The authors conclude that seasonality will bring forward the initial onset of month-long aragonite undersaturation by 17 years on average for the global oceans. I recommend this article for publication in Biogeosciences, provided that the authors address the comments below.

Scope for improvement:

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Section 2 Methods

P 5913 L10-11: The text should state CLEARLY that the uncertainties of 10.9 and 9.2 $\mu\text{mol/kg}$ for DIC and TA are for the global ocean south of 70°N and exclude coastal waters (as is stated on page 4332, Figure 12 and Table 6 of Sasse et al., 2013a). The implication is that the results presented in this paper apply only for the global OPEN ocean south of 70°N. The authors might consider blanking areas for which the results are less accurate.

P5912. The error analysis of the monthly climatology for omega should be much more thorough.

P5912. L2. Figure 1 is not very informative. More useful would be a difference plot between in situ measurements and predicted values. Better use might be made of the colour bar if it were to range from 1.0 to e.g. 4.5.

P5912. L13 and Figure 2. The comparison of 'our zonal mean predictions to in situ measurements' is not comparing like with like. It would be instructive to 1) have a comparison of in situ predictions with in situ measurements for winter and summer and 2) zonal mean predictions for winter and summer (Figure 2).

P5912 L16. The wintertime minimum is clear for the northern hemisphere, but less so for the southern hemisphere.

Section 3

L5913. L13. The explanation of the independent predictions is far too short to be understandable. The reader should not have to read another paper to get the basics of this method.

P5913. L23. This analysis does not provide any insight in to spatial biases in the approach, contrary to what the authors state. Extra information on spatial biases (or the absence thereof) would be beneficial.

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Conclusions

The conclusions are a summary of the text. This is not what conclusions should be like. The current conclusions are not very inspiring (as the article was very clear and informative). There is no need for the conclusions to repeat the introduction, nor to describe the methods. The conclusions should not repeat the main text and might be shortened substantially.

Minor comments

Abstract

L5908 L16, L19, L21 Repetition: 'Our results suggest' (3x).

Introduction

P5908 L25. Consider adding a more recent reference.

P5909. L2. Correct 'ocean's'

P5910. L14. Consider adding a reference to Newton et al. (2014), the GOA-ON report to be found on this page.

P5910 L15-16. It is not clear what the authors want to say: such a large-scale initiative throughout the global ocean'. Does the 2010 study really comment on the GOA-ON initiative (which started in or after 2010)?

P5911. L5, also P5917 and elsewhere. The authors refer to their TA and DIC climatologies as 'new global CO2 climatologies'. This is a little confusing.

Section 2.

P5911. L12. Correct 'ocean's'.

P5912. L23. Is Popova et al (2014) a 'data-based' study? While glancing over it, I could not find much evidence of data being the basis of the (model?) predictions.

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Section 3

P5913. L21. The figures present a near-normal distribution, not a normal distribution as stated in the text.

Section 5

P5914. L25. Diurnal variation can play a role in open ocean areas with shallow mixed layers, such as the tropics. There is some older work on this (e.g. Robertson et al., 1993; Bakker et al., 2001; Boutin et al., 1998). Recent studies on surface salinity in a SMOS context are also looking into this (talks at 2014 ESA-SOLAS-EGU conference).

P5914. L25. Not sure whether (Aze et al. 2014) is the correct way of citing the CBD report. I suspect that the 3 editors played a key role in this report. The report itself provides this citation (page 2): Secretariat of the Convention on Biological Diversity (2014). An Updated Synthesis of the Impacts of Ocean Acidification on Marine Biodiversity (Eds: S. Hennige, J.M. Roberts & P. Williamson). Montreal, Technical Series No. 75, 99 pages.

P5915. L8. Did you explain SD?

P5915. L14. Clarify that this is 'seasonality is the dominant mode of variability throughout the global ocean in the models'. (add 'in the models'.)

P5915. L24. 'consistent' appears to overstate this pattern. Consider replacing 'consistent' by 'similar'.

P5915. L27. What is the standard deviation (or the range) of the ratio of 1.3 ?

P5916. L1. Add 'This suggests ESM on average under-predict'.

Section 6

P5916. L13. The authors casually state that surface ocean pCO₂ would track the increase in atmospheric CO₂. What is this statement based on? Clarify which implicit

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assumptions this statement relies on. Add a reference.

P5916. L13-14. The authors state that this was 'likely adequate for most of the 20th century'. What do the authors mean with 'likely adequate'? What do the authors base this statement on? Is this a model result? If so, say so. Only in recent decades is data coverage of ocean carbon parameters sufficiently large for trend analysis of surface ocean CO₂ (e.g. Takahashi et al., 2009).

P5917. L2. The authors mention an increase in the CO₂ disequilibrium. What is this based on? A reference would be appropriate.

Section 7

P5918. L8. Why are these sites 'unique'? Consider removing the word 'unique'. Presumably you mean that these are 'single' sites, but that these sites are somewhat representative for the wider region.

P5918. L21-23. The statement on a reduction in seasonal amplitudes of omega as a result of changes in the Revelle factor needs better explanation and possibly a reference to earlier studies observing something similar. The current text is rather cryptic.

Section 7.1.

P5919. L3-4 and L11. Add 'the year', in 'by the year 2086' and 'as early as the year 2030'.

P5919. L5. Correct 'century's'. (??)

P5920. L2. Remove 'before this occurs' as this overlaps with 'before' later in the sentence.

Section 7.2.

P5920. L21. Add 'the' in 'the Southern Ocean'.

Section 8.

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P5921. L8. Do you mean $\sim 3.5^\circ$ latitude? Clarify.

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P5921 L10. Remove 'additional'. This is not really additional.

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P5921. L13. 'Much earlier than anticipated'. What is this based on? Clarify.

Conclusions

P5921 L20. Remove or clarify 'at the base' and 'at the top'.

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P5921 L21-L22. Saturation states and minerals are plural, however, you only provide the symbol for omega aragonite. Correct.

References

Bakker, D.C.E., J. Etcheto, J. Boutin, Y. Dandonneau, and L. Merlivat (2001) Variability of surface water $f\text{CO}_2$ during seasonal upwelling in the equatorial Atlantic Ocean as observed by a drifting buoy. *Journal of Geophysical Research*, 106: 9241-9253, doi:10.1029/1999JC000275.

Boutin, J., J. Etcheto, Y. Dandonneau, D.C.E. Bakker, R.A. Feely, H.Y. Inoue, M. Ishii, R.D. Ling, P.D. Nightingale, N. Metzl, and R. Wanninkhof (1999) Satellite sea surface temperature: a powerful tool for interpreting in situ pCO_2 measurements in the equatorial Pacific Ocean. *Tellus*, 51B (2): 490-508.

Newton, J.A., Feely, R. A., Jewett, E. B., Williamson, P., Mathis, J. (2014) Global Ocean Acidification Observing Network: Requirements and Governance Plan. First Edition, 60 pp. http://goa-on.org/docs/GOA-ON_plan_print.pdf.

Robertson, J. E., Watson, A. J., Langdon, C., Ling, R. D. (1993) Diurnal variations in surface pCO_2 chemical properties, and oxygen at 60N, 20W in the N.E. Atlantic. *Deep-Sea Res. II* 40, 409–422.

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