Biogeosciences Discuss., 12, C2356–C2359, 2015
www.biogeosciences-discuss.net/12/C2356/2015/

Biogeosciences
Discussions
Open Access

Interactive comment on "Quantifying the influence of CO_2 seasonality on future ocean acidification" *by* T. P. Sasse et al.

M. Hagens (Referee)

m.hagens@uu.nl

Received and published: 22 May 2015

General comments

This paper addresses the importance of seasonality in the carbonate system for predicting the onset of aragonite undersaturation during the 21st century. Specifically, contemporary seasonality in Ωar was quantified using earlier reconstructed CT and AT climatologies and projected onto results of an ESM ensemble using several of the most recent emission scenarios. The first occurrence of month-long undersaturation, being defined here as a critical time period, was compared with the first occurrence of annual mean and permanent undersaturation for various regions.

I fully agree with the authors that including seasonality is key to projecting future im-

pacts of OA, given the assumed response time of marine biota to aragonite undersaturation. The impact of seasonality on future Ωar has been subject of previous work but not on the global scale, making this manuscript a good addition to existing literature. Data analysis has been carried out with care, confirming the validity of the approach in determining current seasonality in Ωar. I would therefore recommend the manuscript for publication in Biogeosciences, provided that the comments below are taken into account.

In my opinion, the manuscript would benefit from exploring changes in future Ωar seasonality. Previous work has shown that Ω will become more sensitive to changes in CT and AT, as the CT/AT ratio gets closer to unity due to enhanced atmospheric CO2 uptake (Egleston et al, 2010). As a result of this, Ωar seasonality is expected to increase during the 21st century until the point where CT equals AT, which, according to Egleston et al (2010), is reached at high latitudes around 3x the preindustrial pCO2 level (in this context, I could not follow the reasoning on p. 5918, lines 21-23). I understand the difficulties associated with predicting seasonality in carbonate system parameters using ESM and the choice of the authors to use decadal trends in CT and AT from the ESM to predict future Ωar. I would however encourage the authors to add some sensitivity analyses showing the potential effect of a shift in the phase and/or magnitude of CO2 seasonality, especially given that seasonality has been shown to be the dominant mode of contemporary Ωar variability in the majority of oceanic waters (Fig. 5).

Specific comments

Manuscript title: the manuscript really focuses on the effects of CO2 seasonality on Ωar, not other impacts of OA. I would therefore suggest mentioning this more clearly in the title, e.g.: "Quantifying the influence of CO2 seasonality on future aragonite saturation"

p. 5911, line 18: given that the CT climatologies represent the nominal year of 2000, why have decadal averages of temperature, salinity and nutrients been used rather

than the 1995-2004 years?

p. 5912, lines 16-18: I agree that the winter pattern is well captured, with the possible exception of the winter zonal mean around 70°S (is this also where the comment on p. 5914, lines 18-21, refers to?), but the summer zonal mean seems to be at the lower edge of the range of measurements around e.g. 60°N and 40°S. Do you have an explanation for this?

p. 5915, line 6: it says here that the annual-mean values between 2006 and 2100 are used, but of which emission scenario? Were different results for IAV obtained when another scenario was used?

p. 5915, lines 26-28: is there a marked difference between the various ESM here? What is the spread in the various model-based relative to data-based seasonal amplitudes?

p. 5917, lines 19-26: see general comment above.

p. 5918, lines 13-18: it is briefly mentioned that by including seasonality the onset of aragonite undersaturation in the Southern Ocean will be brought forward by ca. 8 years relative to the annual mean, while the situation of permanent undersaturation is delayed by ca. 15 years. I think this difference, resulting from the specific seasonal curve of Ωar at this location, is quite interesting and I'm wondering if this non-symmetrical pattern is also observed at other locations. Perhaps the authors could elaborate on this.

p. 5918, lines 21-23: see general comment above. With the projected greater sensitivity of Ωar to changes in CT and AT I would expect larger rather than smaller amplitudes. I would be interested in seeing the changes in both the Revelle factor and the amplitude of the Ωar seasonality, as it is not obvious from Fig. 6.

p. 5920, lines 5-29: The results of RCP2.6 are presented in Table 2 and Fig. 8 but not discussed at all in this section. I would therefore include a short discussion on this scenario here.

p. 5920, lines 27-29: in my opinion this is an interesting conclusion that could be stressed more, e.g. in the abstract.

References

Egleston, E. S., Sabine, C. L., and Morel, F. M. M.: Revelle revisited: Buffer factors that quantify the response of ocean chemistry to changes in DIC and alkalinity, Global Biogeochem. Cy., 24, GB1002, doi:10.1029/2008GB003407, 2010

Interactive comment on Biogeosciences Discuss., 12, 5907, 2015.