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Biogeosciences Discussions

Interactive comment on "Coccolithophore surface distributions in the North Atlantic and their modulation of the air-sea flux of CO_2 from 10 years of satellite Earth observation data" *by* J. D. Shutler et al.

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We thank the reviewer for his helpful review and positive comments. The issues that they have raised are all relatively minor. We have addressed each of the reviewer's specific comments below. The reviewer comments are in *italics* and are followed by our responses.

The generalisation of the impact of coccolithophore calcification on the pCO2 air-sea gradient to the entire basin based on remote sensing is a step for the understanding of *inorganic C dynamics in the context of Global Change.*

Thankyou for this positive comment. Yes we agree that the work provides a step forward in our understanding of inorganic carbon dynamics.

Since the developed algorithm is shown to also capture shelf coccolithophore blooms, why restrain the coverage to the open ocean and not include shallow blooms?

Unfortunately the Takahashi climatology (required for analysis as it provides the in-water pCO2 data) does not cover the majority of the shelf seas, therefore to provide a clear focus (ie open ocean) we used bathymetry (ie >200m depth) to constrain the region of study.

1) It is now accepted that water-leaving radiance is proportional to the seawater concentration of suspended calcite and more precisely to surface densities of detached coccoliths and not only coccolithophore cells (Balch et al.2005). The coccolith-to-coccolithophore ratio is highly variable and increases towards the end of the bloom (Holligan et al. 1993). Reducing the estimate of CaCO3 standing stocks to a layer depth of 20m, a life span of 30 days, a coccolithophore concentration of 2000 cells/ml leading to a PIC concentration of 65 mg/m3, is probably the weakest part of the manuscript. The determination of CaCO3 standing stocks could benefit from the 2-band approach that Balch et al. (2005) (used with MODIS images), with the added value of providing also the organic carbon estimates.

The use of the PIC maps (Balch et al 2005) would be equally as valid as the method that we have used in our study.

The advantages of the method that we have chosen to use is that it allowed us to evaluate the uncertainties using in situ (CPR) data that covered the same spatial and temporal extent as the Earth observation data time series. The CPR database is the largest in situ plankton database in the world. Our approach meant that we were able to estimate the regional uncertainty consistent with the time series of interest.

Unfortunately, these in situ data would be unsuitable for evaluating the uncertainties in the PIC approach as they provide a snap shot of the cell numbers, rather than a quantity of PIC. Consequently, if we had used the PIC maps we would have needed to rely on the global uncertainty estimates (as no regional estimate exists) from the original publication.

It is important to remember that the work presented is a broad scale sensitivity analysis and so we are interested in the mean and maximum impacts of the blooms, rather than trying to specifically characterise all of the variations and impacts.

Clearly future work could compare and contrast a similar analysis using the PIC map data and we will add some text to this effect into the discussion.

2) Holligan et al. (1993) also suggest that the duration of the bloom averages 30-40 days and that it disappears from satellite images over 1 or 2 weeks, too rapid for the sinking of individual particles and little indication for lateral dispersion. Some other reports indicate shorter or longer periods of high reflectance associated to coccolithophores. The approach A + BY is probably, depending of the time frequency of satellite snapshots, suitable for determining and study the life span of the different features and their dispersion. Can such a study be incorporated to the present study?

We performed a simple such analysis with our SeaWiFS satellite Earth observation data to determine our typical bloom length of 30 days. So, yes, the approach could be used for a detailed study to look at the life span and dispersion of the different features. However, this is beyond the scope of the current paper. It would likely involve a number of additional techniques (e.g. particle dispersion modeling).

3) Takahashi's climatology that relies upon in situ measurements probably already accounts for the effect of calcification on pCO2 in some areas and needs to be modulated in the text.

Yes, we assume that the Takahashi climatology does not include the impact of coc-

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colithophores and we adopt the pragmatic approach of taking it as a baseline for our sensitivity study.

As suggested we will highlight this assumption and add this information into the discussion.

4) As suggested by A.V. Borges in his previous comment, the effect of coccolithophore blooms on pCO2 and the dissolved inorganic carbon system has been documented in the cited papers (Harlay et al.2010, 2011 and Suykens et al. 2010). Previous studies by Purdie and Finch (1994) or Buitenhuis et al. (1996), for example, have also illustrated the potential reduction by calcification of the air-sea gradient in pCO2, based on in situ measurements. The reference to these paper should be made in the discussion.

Yes we agree and we will reference them in the discussion (please also see the response to A.V. Borges' short comment).

Minor comments: Are (NASA, 2010) and (NOAA, 2011) some bibliographic references? If yes, they are not listed in the reference list.

Yes they are references and they were mistakenly omitted from the reference list. We will correct this.

Interactive comment on Biogeosciences Discuss., 9, 5823, 2012.

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