Biogeosciences Discuss., 9, C2773–C2779, 2012
www.biogeosciences-discuss.net/9/C2773/2012/

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.

J. D. Shutler et al.

jams@pml.ac.uk

Received and published: 27 July 2012

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.

1) The Takahashi climatologies are assumed to not include the effects on pCO2 from calcifiers. While there is no mention of this in either the Takahashi article or the current

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manuscript, there is the possibility that certain grid cells may in fact include coccolithophore effects on pco2 since blooms are regular annual features in specific regions. If pCO2 samples were made inside blooms, then the climatology would be impacted by coccolithophores, and the perturbation calculation would be compromised. This should either be fact-checked or discussed.

It should be noted that one of the conclusions from the work is that the temporal and spatial coverage of the blooms is highly variable (please see the abstract – ie -54/+84

Yes, the Takahashi climatology (Takahashi et al 2009) is assumed not to include the impact of coccolithophores. As the reviewer states, the original publication makes no mention of the effects of calcification. Since we have no a priori way of evaluating the effect of coccolithophores on the Takahashi climatology, we adopt the pragmatic approach of taking it as a baseline for our sensitivity study. We will highlight this assumption and add this information into the discussion.

2) The authors cite conservative numbers in their calculations - assuming a constant cell density of 2000 cells/ml and a CaCO3-C value of 0.065. Yet these are not the lower limits cited by the original authors of manuscripts they cite. In Balch et al (1991) - and more recently Holligan et al (2010), - bloom density was observed at cell concentrations ranging from 1000 -2000 cells/ml. Further, Tyrell and Merico (2004) adopt a minimum value of 1000 cells/ml for bloom criteria. The authors have also chosen the higher value of the CaCO3-C range (0.02 – 0.065 given in Brown and Yoder (1994b)). Shouldn't conservative estimates use the lower numbers, unless there is good reason (which would need some mentioning) of why the higher numbers were chosen.

We have used the word 'conservative' to mean 'reasonable', 'realistic' or 'typical' (not minimum), so the quantities used reflect the 'typical' conditions. Accordingly, the value of 2000 cells/ml is used here to represent the typical value across the bloom (so it is expected that concentrations in the early life of the bloom will be lower, and concentrations at its peak are also likely to be higher, similarly variations at the edges of the

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bloom will also exist). The maximum cell density in the literature is much larger at 115,000 cells/ml and as the reviewer states a minimum cell density previously quoted is around 1000 cells/ml. The Holligan et al (2010) paper states typical concentrations of 5000 cells/ml. Therefore, the use of 2000 cells/ml appears appropriate to represent the 'typical' conditions and is consistent with previous studies for our region of interest (e.g. Brown and Yoder 1994a). To clarify this we will remove the use of the word 'conservative' and explain that the values used represent the 'typical' conditions. We will also include the information above in the methods to explain how we reached this value.

A similar situation is true of the use of 0.065 g CaCO3-C m-3. Values in the literature of PIC content of coccolithophore cells range between 0.18-1.05 pg CaCO3-C. Assuming a cell density of 2000 cells/ml, then this translates into a range of 0.036 - 0.21 g CaCO3-C m-3. Values from field experiments account for the higher values and the theoretical value was estimated at 0.276 pg CaCO3-C (Young and Ziveri 1999) which (assuming 2000 cells/ml) equates to 0.054 g CaCO3-C m-3. Therefore we use 0.065 g CaCO3-C m-3 as this appears realistic and is based on the results from an in situ study in our region of interest (eg. Brown et al 1994a). We will add some additional text into the methods to explain this.

We thank the reviewer for identifying these issues and we will expand the text to explain the choices.

3) The authors assume that the bloom features remain for 30 days. Berelson et al (2007) and references therein cite shorter residence times (5-18 days). Could the authors comment or acknowledge this, perhaps in the Discussion?

The values stated in Berelson et al (2007) that the reviewer quotes are based on model simulations in conjunction with Earth observation data, rather than relying directly on observations. No mention of the impact of cloud on the Earth observation data that they use is apparent in the Berelson paper, so we have to assume that this issue was

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unaccounted for.

Our assumption of 30 days is based upon previous work (Brown and Yoder, 1994a) and our SeaWiFS Earth observation dataset. We looked at a number of blooms in a range of locations and on average they were visible in the same approximate location for 3 weeks (21 days). To include the period of growth prior to the blooms being visible in the EO data, and to cover the potential biasing due to cloud, the estimated period was extended to 30 days. The Earth observation data is only able to detect bloom conditions under cloud free conditions and is unable to detect the development of the bloom. Therefore, the period of initial growth will not be within the 21 days. Actual in situ observed residence values from the literature of >40 days exist (e.g. Harlay et al 2010). Brown and Yoder (1994a), using a similar approach, estimated mean and median bloom durations of 36 $\pm$ 25 days (std.dev.) and 31 days, respectively, for coccolithophore blooms observed in the western North Atlantic in CZCS imagery. Therefore, we feel justified in using 30 days. As before, the value is used to represent a reasonable or typical value, and so we will add a sentence in the methods to explain this.

As the reviewer suggests, we will also add some information on this into the discussion.

4) Could ocean color PIC maps be used as a better scalar for bloom PIC concentration, or for comparision? It seems a lot of effort has gone into identifying blooms in the methodology, but wouldn't using PIC directly alleviate the uncertainty with bloom detection by including all areas with PIC present, and also include areas that are at below-bloom concentrations, which are also likely to impact pCO2?

Yes, 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 advantage of the method that we have chosen 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

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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 from the original publication, as no regional estimate exists.

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.

5) I believe the satellite radiance error term (listed at 11%) is not necessary in the total uncertainty calculations as it is already included inherently as part of the accuracy of the bloom pixels, since the bloom pixels are identified with satellite radiances. Since satellite radiances are being used in the bloom detection, there doesn't need to be a separate term.

This is a good point. We will adjust the text and errors accordingly. This will have the effect of reducing the total uncertainty in the areal estimates to ± 22

6) The authors only report false negatives in their confusion matrix, but what about false positives?

The false alarm rate is 0.14 (a value of zero would be the ideal case). We will add this value into the text and explain its significance.

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