

Interactive comment on “The seasonal cycle of $\delta^{13}\text{C}_{DIC}$ in the North Atlantic Subpolar Gyre” by V. Racapé et al.

Anonymous Referee #1

Received and published: 2 October 2013

General comments

The stated aim of this paper, to quantify the $\delta^{13}\text{C}_{DIC}$ seasonal cycle in the North Atlantic Subpolar Gyre (NASPG), is valuable, and the data collected and end result would provide a useful reference for other studies. However, there are several flaws in the interpretation, mostly due to making over-confident claims which are not fully justified by the data, and enough that the paper should not be permitted for publication in its current form.

Specific comments

Temporal distribution of data

The phrase describing the temporal distribution of your data, ‘between 2005 and 2012,’

C5611

which appears in the Abstract and throughout the paper, is misleading. It implies that there is an at least semi-continuous set of data during this period, when in fact there is only data at the start and at the end of it. Initially, I would have been happy with changing this to something along the lines of ‘in 2005-6 and 2010-2,’ and I wouldn’t have considered this too serious an issue. However, delving into the detail of the Results and discussion section we find (I am 95% sure, because the wording is not completely clear – p. 14522, lines 25-6) that in fact the main numerical result of the paper – the amplitude of the $\delta^{13}\text{C}_{DIC}$ seasonal cycle – is derived only from the 2005-6 data. It may well have been shown that it agrees with some of the other data set (only some of it – see later paragraph re. data chosen for exclusion) from 2010-2, but the implication in the Abstract that the value given for the seasonal cycle amplitude is derived from data from “between 2005 and 2012” is unacceptable.

Coccolithophore bloom hypothesis

To explain the ‘anomalous’ values of $\delta^{13}\text{C}_{DIC}$ measured in August 2010, a coccolithophore bloom is invoked. There are many problems with this idea, and it certainly should not be in the presented in the Abstract. Firstly, I am not convinced that there even is enough data to be sure what is ‘normal’ and therefore identify something as an anomaly. That aside, let us assume that this data is indeed anomalous. It is admitted that there is no evidence of coccolithophores because of cloudy conditions occluding satellite imagery (‘it is difficult to confirm this hypothesis’ should be stronger: ‘it is impossible. . .’). It is stated that the shallow MLD and high temperature would promote a coccolithophore bloom in August and Raitso et al. (2006) is referenced to support this idea. In fact, that paper does not really support this hypothesis, for several reasons: (1) the timing is not right – the anomaly is in August, while Raitso et al. say ‘coccolithophores in the subpolar North Atlantic occur at their highest abundance during late spring/early summer and peak in June’; (2) as the timing is not right, a different explanation is needed for the initiation of the bloom – Raitso et al. say ‘it must be noted that whenever high SST anomalies occur it does not mean that a bloom will

C5612

be formed, but when it co-occurs with the timing of coccolithophores it will probably benefit the bloom'; and (3) it was too cloudy for any satellite imagery, which implies that there was not high solar radiation at the sea surface, but Raitsos et al. say 'the combined effect of high solar radiation, shallow MLD, and increased SST were highly correlated with coccolithophore abundance'. Perhaps it is meant that there was a big bloom earlier in the summer and the low $\delta^{13}C_{DIC}$ has persisted, but that is not what is stated in this paper. Next, even accepting for the sake of argument that there was a coccolithophore bloom, no evidence is provided that it would actually reduce DIC without affecting $\delta^{13}C_{DIC}$. They may well produce calcium carbonate shells, which do not fractionate carbon much, but they also produce organic matter, which presumably does. Finally, there are other, more parsimonious explanations for the 'anomaly' which are not considered. For example: is there also a low salinity anomaly – would an increase in freshwater input reduce DIC without affecting $\delta^{13}C_{DIC}$? Or: relatively, there was a very deep winter MLD at the start of 2009 – could this have lowered $\delta^{13}C_{DIC}$ at the surface, and persisted through to August 2010? $\delta^{13}C_{DIC}$ should re-equilibrate with the atmosphere much more slowly than DIC after such an event. In conclusion, the coccolithophore bloom hypothesis is probably worth including, but (1) with more caveats given, (2) not as the only option, and (3) not in the Abstract.

Data selected for exclusion

This leads on to the data selected for exclusion. The 'anomaly' from August 2010 has been discussed at length. As I mentioned, I am not convinced there is enough data to establish 'normal' conditions, and justify its exclusion. It is claimed the data from June 2006 were dropped because there are only 4 measurements; how is this a valid criteria for exclusion? From Figure 3(a) it looks like these data have actually been ignored for convenience, because one of them falls in the range of the August 2010 'anomaly'. The calculated amplitude of seasonal $\delta^{13}C_{DIC}$ variation should either (1) include all of the data and have a larger error (there is no error estimate reported at all in the Abstract alongside the amplitude value, which needs fixing); or (2) be reported

C5613

as separate values for different years with different conditions. Reporting a single value in the Abstract implies that all of the data agrees with it.

Technical corrections

Abstract, line 8: Would be useful to state the polarity of 'strong linear relationship' here

p. 14517, lines 3-10: Might be clearer to put these numbers in a table?

- Lines 13, 17 + 19: d^{-1} is an unclear unit. Please write out decade⁻¹ (I assume)

- Line 28: 'between 2005 and 2012' – see Specific comments

p. 14518, line 13: were => are

- Lines 21-22: remove 'depending on season with high' (typo)

p. 14519, line 10: accuracy 'close to' ± 0.02 per mille? What was the actual value?

- Line 25: 'around' -24 per mille – why 'around'? Did you not use precisely -24 in the calculation?

p. 14520, line 3: to => with

- Line 15: 'the DIC gradient . . . which increases northward' – the DIC increases northward, the DIC gradient does not (in fact, the DIC gradient with latitude decreases northward according to Fig. 2(b)+(d))

p. 14521, line 8: 'seems to be lower' – are they or aren't they lower? Why only seems to be?

p. 14522, line 1: 'seem to be' – see previous comment

- Lines 1-2: '[some data is] organized linearly above [other data]' – I know what you are trying to say but this statement is meaningless

p. 14523, lines 13-14: 'Minima are observed . . . whereas maxima are estimated' – 'observed' and 'estimated' are not synonymous – did you observe the minimum and estimate the maximum? Observe both? Estimate both?

p. 14524, lines 10-11: 'the NASPG experienced a significant warming (+2°C) during

C5614

2010.' This is ambiguous – did it get 2°C warmer during that year, or was the maximum temperature 2°C than normal, or the average temperature?

p. 14526, line 4: soft tissu => soft tissue

p. 14528, line 28: Raitsos et al. (2006) reference is out of alphabetical order

Table 1, caption: 'over the period 2005-2012' – see Specific comments

- It is not clear what you mean by n_{tot} and n
- The units are out of column alignment
- Would be better to have an indication here of which data you chose to exclude

Figure 1, caption: 'over the period 2005-2012' – see Specific comments

- Dashed lines indicate surface currents?
- You mention specific lat/lon points for Reykjavik and Newfoundland in the text – would be good to see these marked on this map
- Show 2005-6 and 2010-2 cruises in different colours?
- Use of an arrow to show data used in study is very imprecise and leaves ambiguity as to which data are used. Use a (shaded?) box?

Figure 2. As for Figure 1, arrows are imprecise, although less of a problem here (because data selection is perpendicular to the arrows)

Interactive comment on Biogeosciences Discuss., 10, 14515, 2013.