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Biogeosciences
Discussions

Interactive comment on "Glacial meltwater and primary production as drivers for strong CO_2 uptake in fjord and coastal waters adjacent to the Greenland Ice Sheet" *by* L. Meire et al.

Anonymous Referee #2

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GENERAL COMMENTS

The authors report a nice data-set of DIC, TA and pCO2 in a Greenland fjord that covers seasonality, and show that this system acts as a sink for atmospheric CO2. The drivers of the CO2 dynamics are investigated with a model and the authors conclude that part of the CO2 sink is related to mixing of meltwater and seawater (a purely thermodynamic effect) but that the larger part is related to NEP (although this is somewhat understated, while the effect of meltwater could be overstated).

MAJOR COMMENTS

In their model, the authors used data of TA and DIC measured on melted glacier ice as

the freshwater (FW) endmember. There are some problems in this approach :

* As stated in page 17929 only 60% of the freshwater relates to glacier meltwater and 34% relates to surface runoff. It is unlikely that TA and DIC values for surface runoff are the same as those from the melted glacier ice. Hence, the model should have used two FW end-members.

* 6% of the freshwater comes from precipitation. Is this precipitation directly on the fjord surface ? or is this precipitation on the watershed ? How can it be distinguished from surface runoff ? Isn't possible to account this in model (adding an addition flux in the mass balance for direct rain on the fjord surface) ?

* Shouldn't groundwater inputs somehow also be included in the model ?

* The glacier ice does not simply melt and mix with seawater. There's in fact some quite complex water infiltration dynamics (e.g. Olichwer et al. 2012) and weathering below the glacier (Wadham et al. 2010; Graly et al. 2014), so that the FW end-members of TA and DIC are in really different from those used in the model. This in fact appears somehow in the Y-intercept of the linear regressions of DIC and TA vs salinity in the fjord (page 17938) that are different from the FW values (50 vs 159 for TA and 80 vs 61 for DIC). In the end, the CO2 content of Greenland "glaciers" is in fact quite high (Ryu & Jacobson 2012) and not below saturation as stated in page 17942.

P 17935 L 11 : so no seasonality was accounted for the SW end-member ? I expect that in such a shallow system DIC increases in bottom shelf waters after the bloom organic matter sinks.

I would also expect an enrichment of bottom water DIC in the fjord from remineralization of organic matter that sediments from the surface to the bottom of the fjord. So that DIC concentration transported from bottom to surface (F flux) should be different in each box and increase from Zone 1 to Zone 3.

Is there a possibility that coccolithophores also bloomed in the studied fjord ? Coccolithophores are common in high-latitude fjords (Schei 1975; Kristiansen et al. 1994) and this affects CO2 dynamics (Purdie & Finch, 1994).

I suggest that the authors bank their raw data at CDIAC and mention this at the end of the material & methods section.

P 17943 : The effect of sea-ice meltwater on CO2 dynamics has been shown before in several studies (I picked a recent one by Bates et al. 2014 but there are others), so this is not entirely a complete conceptual revolution of CO2 ocean dynamics, as suggested. Although this might be the first time it is shown for glacier meltwater in a fjord.

Did the surface freeze at some point in the fjord during the annual cycle (sea-ice formation) ? If so, shouldn't this be somehow accounted in the mass balance and the air-sea CO2 fluxes ?

SPECIFIC COMMENTS

P 17927 L 7 : add the months and years of the cruises.

P 17927 L 20 : is this the global uptake in oceans or oceans+land ?

P 17928 L 5 : might be useful to cite some papers that show this fingerprint on other biogeochemical variables in principle more studied than CO2. Salinity, Nutrients, chlorophyll-a.

P 17928 + 17941 : There's a mix of units that should all be uniform : tC/month/km2, gC/m2/yr, gC/m2/d

P 17928 L 11 : "exact mechanisms (. . .) not well understood". Please rephrase: ocean CO2 dynamics isn't rocket science or quantum physics. There are a handful of thermodynamic and biological processes that control CO2 in the oceans. Whether they've been evaluated and quantified in every single part of the ocean is another matter, and indeed there's some work left.

P 17928 : defined DIC and pCO2 abbreviations first time they're used.

P 17930 : please provide some information on the precision and accuracy of the pCO2 measurements, and comment on the calibration of the instrument (how and how often ?). Paper by Fietzek et al. 2014 might be useful.

P 17930 : define SD

P 17931 : define ICES

P 17931 L 23 : in the dark ?

P 17931 L 23 : Incubations are quite long. Is this standard procedure ? I thought incubations for bacterial production were shorter, e.g. 2 h

P 17932 L 10 : There's increasing evidence that the Wanninkhof & McGillis (1999) parameterization provides over-estimates at high wind speeds, e.g. Ho et al. (2006). This is likely due to strong errors at high wind speeds when the eddy-covariance apparatus is thrown around with ship movement. The use of the Wanninkhof & McGillis (1999) parametrization is a problem in the present study since the fluxes computed at wind speeds of 30 m/s (as stated) will be sky high.

P 17934 L 2 : this equation should be added in Table 1. As it stands, in table 1, there are 7 unknowns (Q1, Q2, Q3, F1, F2, F3, Qg) for 6 equations which is puzzling if you do not carefully read all the M&M, and you'll miss how this was solved.

P 17937 : "strikingly" is a self-evaluation. Let the reader decide what' striking in your work.

P 17937 L 5 : Here and in numerous other places in the ms (17939, 17942, 17943, 17944). The correct phrasing is : "surface waters are undersaturated in CO2 with respect to atmospheric equilibrium" or "pCO2 in surface waters is below atmospheric pCO2". The mix of both "pCO2 undersaturation" or "pCO2 is undersaturated" is not correct.

P 17930 L L 7 : Are these relations for all of the data (all stations and all cruises) ?

if not please specify. Also, these plots might be included in the paper. Finally, isn't the fact that DIC is conservative contradictory with the fact that NEP seems to control DIC dynamic (Fig. 10) and lead to large temporal changes of DIC (Fig. 10) ? This is probably related to the large salinity variation that obscures the NEP effect on DIC. But some rewording could be useful. Plotting DIC & TA versus Salinity will help to discuss this.

P 17942 : In page 17935 TAFW is 50, and here it's 54 μM

P 17943 : One way to also demonstrate this is to plot DIC, TA and pCO2 in surface waters as a function salinity, and compute the theoretical pCO2 change along the salinity gradient using the two extreme salinity values (DIC and TA end-members) (this gives the the thermodynamic effect of salinity change on constants and on pCO2 in a closed system, e.i. no air-sea CO2 exchange).

L 17944 : At such low salinities (8) the choice of dissociation constants becomes a problem, as well as the total boron value (computed from salinity), so this should be detailed in the Material and methods.

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