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Interactive comment on "Assessing net community production in a glaciated Alaska fjord" *by* S. C. Reisdorph and J. T. Mathis

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Reviewer 1 Comments: (RC) General comments

RC: The magnitude, spatial and temporal variability of net community production (NCP) in a glaciated fjord, was measured through different species: dissolved inorganic carbon (DIC), inorganic macronutrients, dissolved oxygen (DO) and particulate organic carbon (POC). Net community production varied with seasons. The work should concentrate on more general statements for the system and not treat the different sites separately.

AR: We treat the 4 sites separately because of the specific variation in natural influences and condition. Specific & technical comments

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Abstract RC: Avoid acronym for Glacier Bay (GLBA) throughout the ms. It is not much longer than Glacier Bay and much better to read, the same is true for Gulf of Alaska (GOA), Prince William Sound (PWS).

AR: Acronyms have been removed from the Abstract section. However, we did leave GLBA as an acronym within the main body since it is used often throughout the ms. Introduction

RC: P 4 Line 16 Definition of ecosystem functionality is essential since it is one of the objectives

AR: We have added examples for further clarification.

RC: P 5 all the NCP provided could be presented in a Table. I don't see how this data compared that are one order of magnitude different

AR: We feel the figure helps to remind the reader of the regions and that each region has different natural influences (i.e. glaciers in the north; oceanic water in the south).

RC: Authors propose that to understand the dynamics that drive NCP and the associated air-sea CO2 fluxes related to deglaciation processes is of great interest worldwide. It seems interesting to compare with Chilean Patagonia and fjords in Norway, I strongly suggest this to improve generallity. Expand on Aracena 2011 and fjords overall exchanges with the open ocean.

AR: We understand that these global comparisons are important. However, we do not feel it is within the primary focus of this ms. We touch on similar fjords from around the world, but the focus of this ms is to provide a first time estimate of NCP in GLBA. Including GLBA in a more detailed global comparison is out of the scope of this ms and may be more appropriate for a future publication.

RC: P 7 Keep focus in assessing net community production, in order to resolve about magnitude of sink for atmospheric CO2. To connect with fish community structures or endangered species may be a different objective.

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AR: We felt that with lower trophic level biological studies and species identification in GLBA is greatly lacking, that it was important to discuss some of the higher trophic biology to link to the biological community was necessary and may help suggest what lower trophic level organisms may live in the bay based on these higher level organisms. It also establishes the importance of understanding NCP within the bay and how it supports these endangered species.

Methods RC: Pag 10 Line 20 replace (prepared and distributed by Andrew Dickson, UCSD) by "measures were standardized using certificated reference material distributed by Scripps Institution of oceanography (Dr. Andrew Dickson laboratory)"

AR: Description has been edited.

Results and Discussion Results RC: Spatial and seasonal distributions of DIC and nitrate are well described but I suggest that "carbon overconsumption", the process in which more DIC is taken up than that inferred from the C:N Redfield ratio (Pag 15) should be clarified by means of a conceptual diagram (even qualitative) to take advantage of novel results, better understand results and facilitate certainties.

AR: We have listed carbon overconsumption as a possible explanation along with other possibilities, but do not feel it is part of this ms to go into depth about each one. This is a baseline study in GLBA and additional study would need to be done to flesh out these possible explanations.

RC: Figs 3 and 4 are not easy to follow, and this is linked to my final statement about Fig 7. Maybe the seasonality is less important than the overall spatial behaviour of the system. Seasonality in high latitudes is already a very well-known issue. Maybe the use of spatial statistic would contribute to better comprehend the behaviour of fjords in general.

AR: Our goal for this ms was to describe the system, including seasonality. Both are discussed in the text. However, we chose to present the data in a figure to remind the

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reader of the regional locations and potential influences (i.e. Marine influence in LB).

RC: Page 18 lines 6- 14 should be improved. Next 8 pages with only 3 references for discussion need to be shortened to arrive to the conclusions. I suggest the Results should be separated from the Discussion, I had to study the paper to be able to process all the text written data. Maybe another diagram should be included to show them.

AC: Results and discussion have been separated into 2 sections for better clarity.

RC: Figure 7. Authors should take advantage of these results. They should synthesize about ecosystem functionality, is it one? are the different sites, different ecosystems? Which is the scale?

AC: We've added some additional discussion regarding this plot and ecosystem functionality. However, this ms was focused on providing a baseline and first time estimates of NCP since peer-reviewed biological data within the bay is very limited at this time. It becomes difficult to analyze/discuss differing ecosystems within the bay without a more thorough understanding of biological composition.

RC: Please discuss Martiny et al. (2013), they suggested that the coupling between oceanic carbon, nitrogen and phosphorus cycles may vary systematically by ecosystem and proposes a C:N:P ratio of 78:13:1 in cold, nutrient-rich high-latitude regions.

AR: We plotted this new ratio (78:13:1) and found it to have little to no significant difference than the ratio used. The greatest difference occurs at high concentrations of DIC and NO3, but these are not the samples of concern to this section. RC: I recommend making major revision to the ms (synthesise results and then discuss) for reconsideration. Results are worth publishing and only poorly presented. AC: Results and discussion have been rewritten into separate sections for better clarity.

Anonymous referee #2 General comments

RC: The regions where glaciers meet the sea are of considerable interest. This paper describes results from an extensive sampling project covering all seasons. The pre-

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sentation is, however, such that an evaluation of the scientific merits of this work is not feasible and publication is not recommended. I will mention some, but not all, issues behind coming to this conclusion. The description of the study area lacks numerical information on bathymetry, areas and salinity distribution. The presentation of the results has to be raised to a level of overview and synthesis from the tedious rounds of descriptive text. Graphics and tables might improve the presentation in this respect. The primary subject of the manuscript, net community production, is assessed on the basis of salinity normalized DIC data. The details of the calculations are not sufficiently described but this reviewer recalls the paper by Friis et al, (1999) on the errors which may be introduced by conventional salinity normalization when the low salinity end-members have significant inorganic carbon concentrations (Friis et al., 2003).

AR: We have split the Results and Discussion into 2 separate sections for making it easier to read, understand and follow. We have also added a section (4.1) on the seasonal and spatial distribution of salinity. Regarding Friis et al., 2003, this paper refers to errors within alkalinity estimate as a result of salinity normalization. However, we did not normalize alkalinity, only DIC as stated in the Methods section. We used the carbonate correction, also described in the Methods section, to account for freshwater influences for our NCP estimates.

Specific comments

RC: In section 2 on methods one would expect to see the name of the vessel used on cruises, and a reference to the protocol for the oxygen determination, and an explanation for using 0.8_m filters for filtering nutrient samples, and what “muffled” means for glass fibre filters used for POC samples, and a mention of the type of the 13 mm glass fibre filters. And is the protocol for nutrient analyses really according to a reference from 1981? Since then quality control awareness has had a large and positive influence towards making nutrient results more reliable.

AR: These comments have all been addressed/added to the Methods section. The

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ship's name has been added. We elaborated further on what is involved in 'muffling' of filters. We also addressed the use of filters during nutrient sampling, as well as updated the nutrient protocol reference. We have also added a reference for DO protocols and the type of 13 mm gffs used.

RC: Glacial flour is one of the characteristics of glacial waters. Are there any carbonate minerals in the glacial flour that could affect the DIC determinations?

AR: Added text to the Methods section to address this comment: "While glacial flour may supply some carbonate minerals to the marine system, influencing DIC and CaCO2 concentrations, we were not able to quantify the amount of glacial flour deposited in the Bay or analyze its composition for this study.

RC: There are two errors in equation 2 on page 13038 and there is no explanation for choosing to use the cubic wind relationship of Wanninkhof and McGillis (1999). Nor is there an explanation for using one number in all seasons for atmospheric CO2, 395 uatm.

AR: Equation 2 is correctly written and further explanation of U3 has been added: "Wind speeds were cubed using the methods of Wanninkhof and McGillis (1999) to account for the retardation of gas transfer at low to moderate wind speeds by surfactants and the bubble-enhanced gas transfer that occurs at higher wind speeds." We also recalculated fluxes using seasonally averaged atmos. pCO2, rather than an annual average and edited this in the Methods section. This change, however, did not have any significant effect of flux values and flux numbers remained as they were.

RC: In section 4.2 the results of the NCP calculations are expressed both as mmolC/m2/d, and as g C/season, which is not intelligible as the time unit used here "seasons" is either 2 or 3 months and it is not clear how NCP is integrated over areas. The sums of NCP per season, e.g. page 13044 line 24, come to very large numbers, incomprehensible to this reviewer.

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AR: The use and definition of each “season” is already referenced in the Methods section. The use of ‘seasons’ as a unit has been used in previous NCP publications. We also used the units we did so that they would be in the same units, and thus comparable, to other published NCP values. We understand that they are not the cleanest numbers, but wished to keep these units as our other option would be decigrams and not easily compared with previous studies. Further explanation on regional estimates of area and NCP was also added to the Methods section.

Reviewer 3 Comments General Comments

RC: The manuscript “Assessing net community production in a glaciated Alaska fjord” by Reisdorph & Mathis addresses the important question of how deglaciation, impact the marine biogeochemistry of fjord ecosystems. The research work was undertaken in Glacier Bay, a high latitude fjord that lies within the Gulf of Alaska. Specifically, the work presented by Reisdorph & Mathis seeks to contribute to the better understanding of ecosystem production in a glacially dominated environment representative of much of the southern coastal Alaskan region. The methodological approach used in the work presented relies on the water column determination of seasonally averaged data on dissolved inorganic carbon (DIC), in- organic macronutrients, dissolved oxygen (DO) and particulate organic carbon (POC).

RC: These parameters were used to determine Net Primary Production (NPP), air-sea CO2 exchange and community respiration between July 2011 and July 2012 in Glacier Bay. The data was presented on a regional basis to account for spatial differences within the fjord an important aspect to consider as fjord ecosystems usually generate distinct gradients in water column conditions running from head to mouth. The data presented reflect the expected seasonal changes in NPP making a positive contribution to the understanding of carbon biogeochemistry and food web conditions that may have an impact on key marine species within Glacier Bay. The paper how ever makes cumbersome reading and there are important aspects/issues that have not been treated with enough depth. Seasonal water column DIC concentration changes

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can be a good approximation to determine seasonal NPP (especially in open ocean). This methodological approach has however important limitations mainly because it is difficult to constrain several processes that can add or take out inorganic carbon from the water column (besides the air-sea exchange of CO2 that has been properly addressed in this paper). Boundary conditions in a highly dynamic environment such a fjord are difficult to constrain. The respiration of allochthonous organic carbon from terrestrial (and maybe to a lesser extent oceanic) origin can severely distort in situ NPP estimations hence its implications need to be better addressed (at least the caveats that need to be considered). Another important flaw of the paper is the poor consideration of physical processes that drive NPP within Glacier Bay. The interplay between seasonal freshwater fluxes, influence of nutrient laden more oceanic waters and wind, tidal and other type of water column mixing/stratification processes (including internal waves, the impact of constrictions etc.) have been poorly treated.

AR: We discuss the influence of wind mixing, as well as glacial flour, on our NCP estimates throughout the ms and within the new Discussion section. Influences on stratification are discussed near the beginning of the Background section. Internal waves and constrictions are discussed near the beginning of the new Discussion section. Stratification (primarily salinity-driven) is also discussed in this new section. The influence of winds, turbulent and tidal mixing are also mentioned throughout the Discussion in places where these mechanisms are identified to impact DIC, TA, NCP and nutrient concentrations. We have also added additional text to address other caveats and assumptions to consider in regards to our NCP estimates. These additions are throughout the text and can be viewed via the tracked changes.

Specific comments Introduction

RC: The introduction and background need to be shorten and it should focus on more relevant aspects that i) influence NPP fluxes within Glacier Bay and ii) that better explain the caveats that underlay the methodological approach used (see the general comments above). The justification of the work is poorly presented (one phrase at the

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end of the background section)

AC: Assumptions and caveats underlying the methodologies used have been added within the Methods section when discussing the methodologies used. Additional text describing of justification of this work were included within various sections (e.g. Intro, Conclusion) and can be identified by the traced changes.

Methods

RC: While the number of cruises is specified the duration of the sampling at each geographical station has not been informed. Conventional naming of the vessel or type of research vessels is lacking. The depth used provide information on the whole water column nonetheless the choice of water depth is not fully justified (photic layer or mixed layer depth considerations for instance??). The term “surface” water is used but it is difficult to ascertain what portion of the water column are the authors talking about. Number of replicates and indication of precision of the analysis are lacking

AR: The vessel name and affiliation have been added to the Methods section. Station sampling regime and sample depth justification has also been added to the Methods Section. The term “surface water” has been addressed in this same section of the Methods. Precision of analysis is listed within the Methods where necessary. Additional information can be ascertained from references listed within the Methods.

Results and discussion

RC: This section is very difficult to follow. I suggest that an improved version of the manuscript should separate the result from the discussion section. The use of tabulated results is encouraged

AR: We have split the Results and Discussion into 2 separate sections for better clarity.

Interactive comment on Biogeosciences Discuss., 11, 13029, 2014.