

**Review for manuscript bg-2017-428:**

**“Changes in gross oxygen production, net oxygen production, and air-water gas exchange during seasonal ice melt in the Bras d’Or Lake, a Canadian estuary”** by Manning et al., 2017

In this manuscript the authors present net oxygen and gross oxygen productions (NOP and GOP) in a channel of Wycocomagh Bay, a semi-enclosed estuary in Nova Scotia, Canada.

The authors use the well known O<sub>2</sub>/Ar and triple oxygen isotopes technique for determination of aquatic primary production. This method has been widely used in oceanic environments in the past and in this context, the manuscript provides one of the first applications of this method in a brackish water system influenced by ice coverage during winter months.

Few studies to date have done dedicated studies to find out the gas transfer coefficient through ice. This parameter holds the largest uncertainty for the determination of net and gross oxygen productions through the methods applied. In this study by Manning et al., one of the main aims was to measure the gas transfer coefficient through a dual tracer release experiment in the study site, in the presence of and without ice cover. The study is complemented with a time series of O<sub>2</sub>/Ar measurements and discrete sampling for triple oxygen isotopes, however all these measurements are only taking place in an eastward channel (Little Narrows) that connects Wycocomagh Bay with the neighbor St. Patricks Channel and the rest of Bras d’Or Lake. This channel was mostly free of ice during their study period. Indeed is in Little Narrows where the exchange of waters in and out the Bay is taking place, however, the authors did not spend some samples to also collect water in the interior of Wycocomagh Bay nor in the vertical water column.

I was however attracted by the addition of the non-steady state term for the determination of NOP and GOP, which often is neglected, but due to, for example, the lack of vertical measurements. Manning et al., 2017 GRL, presented this in the recent past. However, in this manuscript, this term could not be finally fully explained and the conclusions lead to the typical speculations of potential heterotrophy or influence of vertical exchange.

The topic is of relevance and suitable for the journal Biogeosciences. The manuscript is mostly well written, but many typos were found and those are listed in the minor comments below. The figures and tables are clear, but can still be improved by the addition of missing information.

I was also particularly missing information regarding the water structure of Wycocomagh Bay where apparently, anoxic waters can prevail during some parts of the year and those certainly can influence the primary productivity of the estuary. Also, the authors make little effort to explain the horizontal exchange of water through the channel, despite they measure a tidal record through the study period.

Before this manuscript can be published, I encourage the authors to revise thoroughly the manuscript and consider the major and minor comments listed below.

## **Major comments:**

### **Abstract**

L3 – Here it is misleading, as in the title, to say that the measurements were done in the Bras d’Or Lake, those were actually in Little Narrows, a channel of the Bras d’Or Lake, and tracer measurements in Wycocomagh Bay. A clearer definition of the study site is required: the Bras d’Or Lake system is conformed by a number of estuaries and channels and also the Bras d’Or Lake itself. It is therefore misleading to say that the measurements were done IN the Bras d’Or Lake. This should be better written and even the title should be modified to reflect the real study site.

### **Introduction.**

Throughout the introduction I am missing more information regarding Whycomagh Bay (WB) for example: how relevant it is, based on previous literature reports, the lateral exchange of water through Little Narrows? How this impacts the biogeochemistry of WB? Is there a well-stratified water column there with little deep-water ventilation? Tides seem to be more important than what the authors suggest initially, more on this point is suggested in the following comment.

I am particularly surprised that the authors do not make any reference to previous works where a detailed description of the physical and biological characteristics in the waters of WB is presented. This is for example the case of Lambert, 2002 (see full reference at the end of this review) where the author presents a detailed description on the characteristics of the different components of the Bras d’Or Lake system. In that work is particularly mentioned that the waters of WB might become low oxygenated in some areas due to restricted circulation that is regulated by topography and coastal configuration. Also in that study, the author suggests that sewage might be an important source of nutrients to the waters of west WB, causing periods of eutrophication, hence, low oxygen conditions. This background information must be added to the introduction.

### **Method**

I am surprised that the authors did not make efforts to also sample for O<sub>2</sub>/Ar and triple oxygen isotope measurements for waters under the ice in the interior of Whycomagh Bay, or at least, before the release of tracer in the borehole they had to build. Also, it is well known that this method requires at best also the knowledge of the vertical distribution of O<sub>2</sub>/Ar and  $\delta^{17}\text{O}$  and  $\delta^{18}\text{O}$ , and the authors could also sample in the vertical water column for examination. I think the authors should sustain why this was not done.

P4, L22-24 - This sentence needs more explanation and evidence. From previous published works, how do the tides affect the exchange of waters in WB and influence its biology, oxygen content and physical water column structure?. A published work by Dupont et al., 2003, discusses thoroughly the role of tides in the Bras d’Or Lakes system.

In pag. 10 L27, the authors mention that “visually” the tides seem to play an important role. If ADCP measurements were done, it will be good to add some more analysis on this direction and try to find out the effect on the time change

in tides to the observations in O<sub>2</sub>/Ar, for example. Please also explain Figure 1 of the supplementary material.

Section 2.2 - Can the authors spend some lines at the end of this section, explaining why the concentration of the tracers between injections is different, and also the bubbling period? Why during injection 1 in the presence of ice the tracer was more diluted? Due to expected less ventilation of the tracer?

P17, L10-11 – The non-steady state term of Eq. 9, assumes that there are changes over time of  $^{17}\Delta$ , and those are related to changes in  $h$  due to potential entrainment into or export from the mixed layer. These are physical processes that are only discussed later. The authors should spend some lines here to introduce these explanations here, as to which processes would influence the non-steady state term.

– Besides of addressing my comment of why the selection of the first period to calculate  $r^2$  in the  $^{17}\Delta$  results until 19 April (mentioned below), will be good to plot a correlation between mixed layer depth and  $^{17}\Delta$  to figure out if there is some influence on the  $^{17}\Delta$  due to shoaling or deepening events.

### **Specific comments:**

#### **Suggestions of text modifications or additions are written in bold text**

##### P1, abstract

- Consider introducing the acronym NOP and GOP already in the abstract  
- Here and throughout the manuscript, the convention to write the mean value and the standard deviation as: “mean(std. dev.)” should be avoided, and use the traditional “mean±std. dev.” format.

##### P2,

L9 – Add a space between “waters” and “(Ducklow et al.”

L10 - “**The** parameterization of gas exchange...”

L11-12 – repetitive sentence to same page L15-16, re-arrange so it does not sound repetitive.

L20-25 – These lines are misleading, by reading them, it gives the impression that the work was done in the Bras d’Or Lake itself. It is until line 25 when reference to Whycomomagh Bay (WB) was made. I would try to move these lines to a new section about “site description”, it could be a first section of the methods part or even a sub-section in the introduction. I would start this paragraph by introducing first WB (L25-29) and briefly mention where it is located, complementing with more background information (previous works) on the physical and biogeochemical properties of the water of WB (mentioned on major comments above).

L22 – “...and has a **total** surface area of ...”

L29-30 – Introduce and rephrase: “**The annual** maximum ice cover is typically reached in early March. Ice disappears rapidly during April until its total melt by the first week of May”

L31-33 – These lines should have less detail, and leave it to the methods section, it is enough if it says: “To determine the gas transfer coefficient during the period of study, we performed a two dual-tracer release experiment ” or something similar.

P4

L1 – “The **NOP/GOP** ratio, provides ...”

L2 – “ ... production available for export **to waters below** the mixed layer.”

L5 - I think here it is fair to still refer to Luz et al., 1999 and Luz and Barkan, 2000 as the initiators of the triple oxygen isotopes method for ocean productivity, rather than the review of Juranek and Quay, 2013 only. The last can still be referenced because it includes also the O<sub>2</sub>/Ar ratio method for NOP.

L12 - “ (Figure 1) during a **33-day time series** (25 March-28 April 2013)”.

It also is important to mention here that all the measurements were done at the water within the surface mixed layer or simply in waters below the surface.

L33 – “...from a thermocouple **located** in the **sampling** bucket, because it had ...”

P5

L2 – “ ... to correct for warming **effects.**”

L6 – How far in cm or m was located the CT sensor from the cable ferry?

L16 – Freshwater inputs to the bay from melting ice and snow, precipitation?

L20 – As I read the manuscript, I am looking for the information on sea-ice thickness in the site of study during the period of analysis; I only found this information in Pag. 21 L11, will be good to move this information to this line for example, ice thickness encountered during Injection 1.

L21 – “ ... **were** diluted by a factor of ...”

L23 – how far was the “separate hole” from the injection site?

P6

L6 – flask evacuation meaning when the flasks were prepared (evacuated) in the laboratory previous to the study?

L12 – which value the authors obtained for <sup>17</sup>Δ in equilibrated waters?

L14 – “ ... 20 mL **of** water in 50 mL glass ...”

L22 – how did the authors detected when the tracer was flowing with this system?

L31 – “The Lott and Jenkins’ **solubility value** is ~2 % higher...”

P7

L9 – “ ... **fully covered** by ice, and completed ...”

L15 – Here the ice volume and thickness during the sampling period should also be provided.

L19 - what was the vertical resolution of the CTD profiles? Is the data in Figure 3 the result of some vertical interpolation to the CTD measurements?

L20 – how the authors determined the depth of the mixed layer (also dashed line in Figure 3)? Here it should be stated.

L21 – “ ... the length of the CTD **instrument** and obtaining ... “

P9

L11 – Add here what *h* in Eq. 2 means, and NOT until P10, L17.

P10

L25 – “ ... m d<sup>-1</sup>, **in the presence of ice and shallow mixed layers, with the standard deviation of the distribution of *k*<sub>600</sub> from the Monte Carlo simulation as the uncertainty value** (Figure 4).”

L26-27 – here refer to Figure 1 of supplementary material and interpret these observations based on the ADCP data if possible, rather than referring to visual observations. Waters flowing into WB can be seen in the eastward velocities panel?. Also, Figure 4 should appear before Table 1, or refer to table 1 earlier in the manuscript.

L29 – “..., and it is possible **that** the...”

L30 – why the authors assume here that the mixed layer was deeper than 0.8±0.3 m during 9-10 April?

P11

L6 – “ m d<sup>-1</sup> **for open waters** (Figure 4).”

L8 – “... lower **in the presence of ice** and 0.4 % lower **in open waters.**”

P12

L5-14 - these lines should be moved to the introduction section, e.g., to pag. 2 and after L19.

L16 – “During **visual** surveys along the shoreline of Whycomagh Bay ...”

P13

L5 – what is the surface area of WB? You only gave the total surface area of Bras d’Or Lake in P2, L22. Maybe add this info on WB also in that same paragraph in the intro.

L7 – “... for both injections (**k=0.71**), then the results yield ...”

L20 – Here it is better to write “time-series until the end of 15<sup>th</sup> of April” isn’t? It would be better to write it like this if the linear interpolation to obtain *k* in the period where there were no measurements was done from 16 April (from 00:00 h) until end of 20 April.

L22 – “midnight 20 April” means the start of 21 April? Then I would write instead “21 April” to keep consistency with the periods above.

L25 – So which *k* value was used for the period between 16 and 19 April? Would it make sense to use an average *k* value between that period of time?. This might be OK since for the period of injection 1 and injection 2, one single *k* value was used in each case. Please complete here what approach was taken.

L31 – “(Figure **1b**)”

L32 – “**After** injection 2, between 20 April ... “

L33 – “... 2.2 m s<sup>-1</sup>. The calculated *k*<sub>600</sub> over this time period ... “

P14

L11 – “... short-term **wind speed** measurements...”

L12 – “... the gas transfer than wind speed **values** averaged over ...”

L20 – “... with other studies showing **that** gas transfer velocity ... “

L24 – remove “polynyas”. Here it only applies “ice leads”

L25 – “**ice leads** that are common in Arctic and Antarctic **sea ice**”.

L26 – here I am also missing more information on the ice thickness found during the study. Elaborate further on the statement of “differences in gas transfer behavior are expected based on the nature of the ice pack”

L31 – “dissolved O<sub>2</sub> derived from air-water exchange” and remove the text “(from the atmosphere)”.

L32- Remove the text “(from H<sub>2</sub>O)”. Later you explain with detail these differences but here it is not explicit enough and just confusing.

P15

L5 – “O<sub>2</sub>, and **for** H<sub>2</sub>O is referenced to the ...”

L7 – “... quantifies the <sup>17</sup>**O-excess** in dissolved O<sub>2</sub>.”

L13 – 294 per meg should be 249 per meg

L25 – a period is missing between “salinity” and “A similar ... “

L30 – remove units for salinity

L31 – “... meteoric **water** endmember ... “

L32 – “... study area, **and** 40 m elevation) ... “

P16

L5 - What is actually the mean salinity in WB? Please give also this information and not only for Little Narrows, I suspect there is a strong seasonal gradient, but how far from Little Narrows this is actually taking place?

L11 - I don't actually understand why you call  $^{17}\Delta$  and  $^{17}\text{O}$ -excess for the dissolved oxygen with atmospheric signal and the one from the water.  $^{17}\Delta$  is the  $^{17}\text{O}$ -excess. If you want to make this difference I would then call it  $^{17}\Delta_{\text{H}_2\text{O}}$  or  $^{17}\Delta_{\text{w}}$  as you did for the slope value, however is still think there is no need to have two separate equations for that, you can simply explain each term what would they imply for case of the atm.  $\text{O}_2$  and  $\text{O}_2$  from  $\text{H}_2\text{O}$  but only using a general equation.

P17

L6- and what about  $[\text{O}_2]_{\text{eq}}$ ?

L9 - the steady state GOP term is also the equation of Prokopenko et al., 2011, please cite her here after the comma.

L15 - How much was the  $^{17}\Delta_{\text{eq}}$  value for equilibrated water at  $< 5^\circ\text{C}$ ?

L20 - Is not until 28 April?

L20 - How many samples were collected in average/day?

L22 - refer here to Eq. 9 after "GOP term."

L28-29 - Why the chosen period to end was 19 of April instead of 16 of April to keep consistency with the  $k$  estimates? I see that from the ML- and volumetric-GOP (Figure 6 c and d), would make sense to connect all the first period until 19 of April, but could have also be done until 16 April and this can be clearly seen in the  $^{17}\Delta$  values (Figure 6 a and b). Did the authors test that combination? Actually by looking later to the NOP results, the NOP starts to decrease from 16 April, so it would make sense to consider the first period of GOP also until before 16 April.

P20

L3 - instead of "intermediate ice cover" write "varying ice cover"

L8 - "... 19 April), **because is the time period when the ice retreated rapidly and there was more light availability**, despite the increasing gas transfer velocity."

P21

L6 - "... especially **under thin** first-year ice..."

L11 - this information on ice thickness becomes too late in the manuscript, move it to Pag. 5.

P22

L19 - title of section 3.4.2 has twice "of"

L26 - instead of "decreased" use "retreated"

P24

L3 - "... appears to decrease during ..."

L3 - as mentioned earlier, the changes in NOP appear to be contrasting when the transition of sea-ice melting period starts in 16 April, I would still strongly suggest to modify the GOP first period also to 16 April instead to until 19 April, so during that transition NOP decreases and GOP increases, and the only way that this can take place is if at the same time that GOP increases also respiration increases (net heterotrophy) as discussed below. Also the entrainment of deeper waters as the mixed layer deepens, contributes to changes in GOP during the transition period.

L6-7 - what about the entrainment of low oxygenated waters as the mixed layer deepens?

L13-14 – the fact that there were no measurements below the mixed layer does not imply that vertical mixing is not an important process and should still be discussed.

L16 – In Castro-Morales et al., 2013, the influence of entrainment and vertical mixing in mixed layer GOP and NOP are thoroughly discussed. This citation should be included here.

L34 – what about nutrients sources from land (eutrophication), and also through Little Narrows during tidal exchange? How about a correlation between ice coverage and tides? Horizontal exchange in this region seems more relevant, warmer water entering WB influencing ice melt.

P25

L23-26 – Residence time calculations might help supporting this statement ( $t=h/k$ ), please add this information considering the changes in  $k$  and  $h$  through the time series.

L26 – I agree in this potential explanation, considering that there might be low oxygenated water below the mixed layer in this region.

P26

L12 – “ ~ 0) **on a daily basis.**”

P27

L5 – “ ... but the salinity **in sea ice** is substantially lower ... “

P28

L4 & 10 – add also to the list of references Castro-Morales et al., 2013

L29 – “ ... also from rivers, **or due to the entrainment of poor oxygenated waters.**”

## **Tables**

- Include a small table (or embed this information in Table 1) where includes the following data: full experiment period with dates from start of the experiment (25 March) until the end (28 April), also when the injections took place, and not only the dates for determination of  $k$ . Include in the table also the ice coverage/fraction. It is hard now to follow all these changes between beginning of tracers experiments, beginning of the full experiment, evolution of ice coverage, etc.

## **Figures**

**Figure 1** – In panel b, add latitude and longitude values in axis, as in panel a. In panel c, add some labels, e.g., Greenland, Canada. The colors in this panel are too subtle and hard to see.

**Figure 3** – Why the uppermost measurement did not start from the same depth ? I understand that maybe in the presence of ice this was not possible, but what about in open water?

Is there also a dashed vertical line in the last day of sampling (28 April)? Was a last CTD cast conducted that day?

At best, salinity “units” should be removed everywhere.

Wasn't 25 of March the first day of sampling? Why there is data from even earlier than 24 March? Refer in the caption to Table 1 for specific dates of the vertical dashed lines.

**Figure 4** – There is a typo in the legend for Injection 2, it should be 0.71 and not 0.071. In the line corresponding to Injection 1, the second to fourth measurements point (between 8<sup>th</sup> and 10<sup>th</sup> day) it looks actually an increasing tracers ratio. How can the authors explain this? In the end, the line for injection 1 was drawn by three points, initial, day 9 and day 11 (or end), would this still be valid?

**Figure 6** – In the caption, it should be written at the end “ ... GOP term are most uncertain **due to melting of sea-ice cover.**”

**Figure 7** – Would be good to also add in panels a and b the mixed layer depth using a secondary y-axis to contrast the time evolution of temperature and O<sub>2</sub>/Ar with MLD. In the caption it should be changed “yellow diamonds and pink triangles” to “green diamonds and blue squares” isn’t?

### References

- P1, L16 - Here Loose and Schlosser, 2011 should be “2011b”. Loose et al., 2011b should become 2011c.
- Add the doi’s to all the references where it is missing
- in Eveleth et al., 2016, change to full citation (not in press anymore), vol. 139, 89-102, 10.1016/j.dsr2.2016.07.016.

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