Review of Manning et al **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"**

The study of Manning et al presents 1) estimates of gas transfer velocity under almost complete ice cover and ice-free waters; and 2) measurements of Net and Gross Oxygen production rates under ice, during and immediately after the spring ice melt, in Whycocomagh Bay, a semienclosed estuary in Nova Scotia. The study described in the manuscript is novel and has important implications for studies of the polar regions. I am very enthusiastic about the first part of the paper, focused on the gas exchange, but have more reservations about several aspects of the 2nd part of the paper (estimates of NOP and GOP). Thus, I have just a few comments on the former, and outline more detailed concerned on the latter. Overall, I recommend the paper for publication after major revisions.

I. Gas exchange estimates

Estimating rates of gas exchange in ice-covered seas is of great importance for biogeochemical studies in polar and subpolar regions, yet gas piston velocities have been notoriously difficult terms to estimate, and remained poorly constrained. The authors used a clever approach, where they released a dual tracer (3He/SF6) and monitored the change in the two tracers ratio through time. All physical processes (mixing, dilution within the water column) except for gas exchange with atmosphere affected both tracers equally, while the difference in solubilities of the two gases controlled the changes in the tracer ratio thus allowing for robust determination of piston velocities. I believe this part of the study is of a substantial scientific value and is a great contribution to future studies of polar regions.

Specific comments

1) The ice edge on April 7th was located at approx. 1 km from the sampling site (based on Fig. 1a). I wonder what was the influence of this open water patch on the estimated k600? From Figure 4, it looks like the 3He/SF6 ratio dropped substantially on Day 10. What would be k value if this last point is excluded from the fit?
2) It would be useful to give the actual values of molecular diffisitivities for 3He and SF6 (e.g. somewhere in Section 3.2.1)
3) Is there a reason for the 3He/SF6 increase in the first 3 data points after Injection 1? Or this is likely a "noise" signal? This issue is addressed on p. 10, line 23-25, but some clarification would good. What was the tidal status when these 3 data points were taken (other than "visual observations" mentioned in the subsequent lines on the same page)?

Typos:
There is a typo in Fig. 4 (k600 of the 2nd injection should be 0.7, not 0.07)
P. 6, line 30 should read " The Lott and Jenkins solubility is ~2% higher"

II. Estimates of NOP and GOP rates

The 2nd part of the study was devoted to estimates of NOP and GOP at the Little Narrows sampling location. The productivity terms have been poorly constrained in ice covered and partially ice covered high latitude seas. Thus, the value of this part of the study is in expanding our (currently very limited) knowledge of these terms in the polar regions, despite some limitations (in setup, calculations and interpretation), which I address next.

Specific comments

1. Setup of the study:

O2/Ar monitored and samples for $^{17}\Delta$ were collected at the Little Narrows, which has been ice free all through the length of the time series (based on Figure 1a). How valid it is to apply the piston velocity determined for the ice covered conditions at the ice free Little Narrow study site?

To address this issue, a more through description of the Little Narrows study site is needed: what is residence time of surface water in this channel? Current velocities? In other words, how well measured here O2/Ar and $^{17}\Delta$ signals represent the conditions within the Bay? While the authors do state that spatial variability within the Bay is likely to be small, the issue at hand here is – are the reported NOP and GOP rates really the rates under ice (for the time period between March 31 and April 18th) or are they more representative of the very local ice free waters in the Little Narrows?

2. Calculations:

Equation (5) modified from Prokopenko et al 2011 (equation S8 in that paper, would be good to give a citation) contains two terms, O2 and h (in the NSS term). In Prokopenko et al, O2 and h were assumed constant, while the $^{17}\Delta$ was time-dependent. However, in the study of Manning et al, this is clearly not the case, particularly for the O2 term. In fact, there appears to be a discrepancy between Equation (5) and (11). In the former, O2 is treated as if it in a steady state, while in Equation 11 O2 (as O2/Ar) is treated as a time-variable term.

So, one question is what were the actual values of O2 that the authors used to calculate the reported GOP terms for every time point? Similarly, what were the values of h (though the latter is probably less important, at least for the period between March 30-April 10, when mixed layer depth remained more or less constant, however the changes in h after April 10th are more substantial)?

Based on the above, I am not sure that the approach chosen by the authors to calculate the NSS term is fully correct. I would suggest that the authors redo the calculations using the approach presented in Haskell et al (2017), published in Global Biogeochemical Cycles, which presents an alternative treatment of the NSS term in NOP and GOP calculations under non-steady state conditions.

3. Interpretation:

An obvious problem of the study is the lack of information about the water column below the mixed layer. Are there any published studies on the water column winter conditions?

Clearly, TOI was presented by Manning is the fist of this kind in the Bay. But it would be very important to know the degree of oxygenation of the water column during winter months. If the Bay goes anoxic (or very low O2), estimates of NOP are impossible to make without knowing how much of this winter low O2 signal contributes to the mixed layer.
The problem is likely less acute for GOP, as the absolute difference between the water column and the mixed layer $^{17}\Delta$ is likely to be smaller than for O2/Ar (and one could assume $^{17}\Delta$ being as low as at atmospheric equilibrium value). However, the drop in GOP observed after the ice melt is really difficult to explain: obviously, increased ventilation should not lower GOP, thus the drop of $^{17}\Delta$ is driven, in addition to increased ventilation, by dilution of the mixed layer $^{17}\Delta$ with waters carrying lower 17O-excess. GOP does not require new nutrients (as the authors themselves point out), thus even when all the nutrients are consumed, GOP should not be affected.

The problem with NOP estimates is likely be more acute, but since the O2 concentration below the mixed layer is not known, it is more difficult to assess quantitatively.

On the positive side, this study provides a very interesting and novel example of using the 17O-excess method in waters different from VSMOW, and this part of work is of great value.

Smaller comments:
P. 15, line 22 - Pls, add original reference for "published relationship between d17O-H2O and d18O-H2O , where only the reference to Manning et al (2017) is currently given).
P. 17, line 15-17 – what was the actual values for the fractionation factor used in calculations (as based on Luz and Barkan, 2011)?
P. 20, line 20 should read " the mixed layer-integrate GOP"

In summary, my recommendation for Interpretation part would be to re-write the discussion of the NOP and GOP estimates, clearly stating the above limitations first and substantially shorten the discussion of zooplankton and heterotrophy. It appears that the estimates of NOP rates are likely strongly affected by mixing of the O2-depleted signal and it would not be possible to evaluate the magnitude of this effect). As to GOP – I wonder if using the approach of Haskell would modify the estimated GOP trend through the time series (and some estimates of the degree of dilution of $^{17}\Delta$ signal could be made).