

Interactive comment on “Insights into oxygen transport and net community production in sea ice from oxygen, nitrogen and argon concentrations”

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General Comments

This manuscript presents measurements of gas concentrations within sea-ice cores across a seasonal gradient that are used to estimate the net metabolism within sea ice. The method uses the ratios between oxygen and argon and oxygen and nitrogen to separate the physical dynamics of oxygen concentrations from the biological consumption and production of oxygen. The method was adapted from techniques used in the open ocean, however, a key assumption is overlooked in this application to sea ice. The manuscript discusses sea-ice as a closed system, and therefore no corrections for the exchange of gases across the ice-water and ice-air interface is included, but the sea-ice was found to be permeable over at least part of the ice during all peri-

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ods when this methodology was used. Further, the manuscript lacks clarity throughout and consistency in terminology, which make it very difficult to evaluate the methods and results which appear to be very interesting. The discussion could be shortened significantly to highlight the significant results. Related to this, there seem to be no replicate samples, error estimates, or statistics with which to weigh the significance of the results. Overall, addressing these issues will likely constitute a re-analysis of the data and an entirely new manuscript.

P2052 L25-3. This is a big assumption and is contradictory with statements in the MS. During all periods the sea-ice was permeable in the lower portions (Figure 5) and this region is where a majority of ice metabolism occurs due to this permeability and exchange of metabolic products, nutrients, and carbon with the seawater. Further, it is unclear if the authors are suggesting sea ice is a closed system? Ice is described as porous (1st sentence of Intro) and both permeable and impermeable at different times of the year / different depths of the ice core. Further, I would argue that the sea-ice system is more complicated with the source of O₂ at the center, so exchange across both the air-ice and ice-water interface would need to be corrected for. Since this was modeled after the oceanic O₂/Ar method and NCP estimation was only applicable under permeable ice conditions, I find it very troubling that no corrections are included for the transfer across the air-ice and water-ice interface. This is in direct contradiction with the explanation in the methods (P 2052 L25-3) “In a closed system, NCP then corresponds to the changes of O₂NCP between sampling events. Note that in seawater studies, because of the open system, additional coefficients and equations are required to take into account the diffusion at air–sea interface, the changes in the mixed layer depth and the diffusion of O₂ across the base of the mixed layer (Castro-Morales et al., 2013).” Further, without measurements of below-ice seawater O₂ concentrations and the fact that the sea-ice surface is a region of very high productivity, it is very difficult to evaluate the profile suggested for the permeable ice (Figure 6, P2065 L 20-25).

The methods also lack detail and make it hard to evaluate the results. What time of

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day where the ice cores taken, was this consistent across periods? Diel O₂ production / respiration could heavily influence the results. Over what timescales are the O₂ rates determined? From core to core? Can it be assumed across these long time periods that NO exchange between the air – ice – water occurs? This is unlikely. Where any replicate samples taken? Error estimate? Statistics comparing results?

In general, I think that some terms used in the MS are misleading and limit the clarity of the MS; some examples: (1) “We present the evolution of O₂ standing stocks, saturation levels and concentrations” – oxygen evolution primarily relates to molecular oxygen production by photosynthesis; the saturation levels are not “evolved”. This is especially important when the physical processes are considered (which are a large component here). I would suggest the use of oxygen exchange or production and respiration or metabolism. Explicitly providing terms for the abiotic and biotic processes would significantly clarify this point. (2) P2046 L 17 – “Sea ice is a porous material”, this suggested that fluid gas exchange readily happens in sea ice, but the MS talks at length about impermeable ice layers. The point about impermeable and permeable ice layers should be made explicitly and consistently throughout the MS (3) P2051 L3 Equation 1 is described as “Gas saturation levels is described following Craig and Hayward (1987) in seawater” but the equation does not provide the gas saturation “level”; instead it describes the ratio of the difference between the two concentrations (e.g. (110/100)-1 = .1). This is another example of a place where clarity can be increased; what does “level” represent here? How do you get to the “saturation levels (%)” show in Figure 4?

Detailed comments

P 2046 L 5 “dynamics” instead of “dynamic”; L8 NCP defined as “biological contribution”, not as defined in line 20; L19 “to survive to” to survive at. P2047 L 2 “evolution”, I would suggest variability or change; L10 “privileged” seems like the wrong word here. P2048 L7-8 “to discuss on the feasibility to decipher” awkward. L20-25 What time of the day were the ice cores collected?, was this consistent for all periods? With O₂

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production maximal at midday and the potential build-up of O₂ across the daytime (dependent upon the air-ice and ice-water exchanges which are ignored here) this could significantly alter the calculated NCP rates and the O₂/Ar ratios. P2049 L 4, again, the use of evolution here is misleading / confusing. I suggest variation or dynamics. P2051 L2 missing colon L10-14 Much stronger support for this would be to use the maximum rates present in Rysgaard 2008 to calculate the maximum change in the N₂ signal that can be provided by denitrification (and annamox). A quick back of the envelope give 0.2 umol N L⁻¹ d⁻¹ per m of ice thickness, which is a small percentage of the signal. This could also be a source of variability in N₂, especially for the impermeable ice layers? P2052 L 5 and 7, Consistency in significant figures; L23-24 This statement is unclear “in which, [O₂]_{eq}/[Ar]_{eq} is the solubility ratio in seawater at equilibrium in Castro-Morales et al. (2013), but the solubility ratio in ice at equilibrium in the present study (Sect. 2.4).” P2053 L9 “thickness” = thickens? P2056 – L9-16, Would it not make more sense to normalize your standing stock by ice thickness for comparison purposes? P 2056 L23-25 Saying gas exchanges with the atmosphere (as well as the water below) suggests that a correction is required for both of these losses of gas before NCP can be calculated. Therefore, whenever the brine volume is above 5% (Golden et al. 1998) (if assuming gas is behaving the same as a fluid), these exchanges need to be accounted for, as in the air-water flux in the open ocean (Castro-Morales et al., 2013). The much lower NCP rates reported here (P2063 L 29) compared to other measurements suggest some of these losses may be significant. P2058 L5 awkward. L12 “reminds” awkward. P2062 L22 “Therefore, to test the feasibility of using O₂/Ar to estimate NCP ice, we calculated the changes of standing stocks of O₂NCP” confusing, again consistency in terms would clarify. O₂NCP was defined as “NCP-related O₂ concentrations” earlier. Should it read: changes of O₂ standing stocks due to O₂NCP? P2063 L2 delete “much”. P2065 L11 – does not seem to follow above. P2065 L24 It is very confusing to me to present NCP rates per m⁻² while separating this over different heights in the ice. The m⁻² suggests two dimensions in a planar surface, while a third dimension is including by comparing different heights of

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ice (3 dimensions). I would suggest a per volume (m⁻³ or L) would be clear and more accurate here, as is commonly done with ice core incubations. P2066 L 24 "from the atmosphere" carbon uptake is not examined here / occurring in the atmosphere, should read "from uptake rates determined across the ice-air interface." L23-27 This suggest a significant transfer across the ice-water interface which needs to be accounted for – see earlier comments. Figure 5 – Unreadable, unfortunately the automatic journal format makes this figure very small, but even at a full page size this figure would still be too small (I had to blow it up to 300% to read). This is the most important figure in the MS and needs to be redone, or possibly split to make this very important figure useful. Figure 6 – Carbon "uptake" incorrect (you cannot have a negative uptake), should read carbon exchange or flux.

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