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Ocean Science Discussions

Interactive comment on "Upper ocean stratification and sea ice growth rates during the summer-fall transition, as revealed by Elephant seal foraging in the Adélie Depression, East Antarctica" *by* G. D. Williams et al.

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GENERAL COMMENTS

This paper presents results from Adelie and George V Land coastal waters, based on hydrographic data from instrumented seals. The principal result is summarized in Figures 7 and 8: The change in upper-ocean hydrography in Commonwealth Bay polynya is consistent with sea-ice growth in the Fall transition season of ∼10 cm/day. Ancillary

results include seal transects across the Antarctic Slope Front; however, these don't provide much information beyond those known already from traditional ship-based transects.

The paper is generally well written, and the primary result (and the methodology) will be interesting to many polar scientists. Once my specific comments and technical corrections below are addressed, the paper should be accepted for full publication in Ocean Sciences. However, I urge the authors to consider whether the paper can be simplified or restructured to focus more closely on the Commonwealth Bay polynya during the Fall transition. Alternatively, much more work is required to make the figures more user-friendly. I will explain concerns with figures after Specific Comments.

SPECIFIC COMMENTS

1917/11-12: Did the pre-calving MGT really make it all the way to the continental shelf break? My understanding is the MGT was shorter than that, but that grounded icebergs and "land-fast" sea ice extended it seaward.

1917/17: The regional geography is very important (here, you name three bays). You need to ensure that a version of Figure 1 contains all the features you refer to in the text. (More on figures later.)

1923/14-1924/7: We don't really need such a comprehensive "road-map" to section 4. You told us earlier what to expect.

1924/10 and MANY other places: FIGURE REFERENCING. Settle on ALWAYS referring to the figure or specific figure panel (Fig. 5b). Don't refer to "Panel E" (for example) but to "Fig. 5e". That is, don't assume that the reader wants to look back and make sure he/she is looking at the right last-mentioned figure number. Also, settle on either lower-case or upper-case a, b, c, ... (May be a journal requirement; I don't know). Also, check carefully that all figure callouts are right. Call-outs to different panels of Figure 5 are out of sync, for example, because you have added a DO plot in there, seemingly

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late in the paper prep.

1925/2-3: You can't really talk about "numerous mCDW signatures". I think what you mean is "numerous profiles with evidence of mCDW presence", but I might be confused.

1926/13-14: You need to be consistent in naming the seals. Here you mix "seals I1 and I2" with "seals 44 and 52". You do this somewhere else in the text as well.

1928/5-6: This is a bold statement! The only way it makes sense to me is if I read the blue line on Fig. 8c. But that isn't explained anywhere: it looks like it is meant to be a least-squares fit line but (a) I don't think it really is, and (b) if it is, the error bars on the fit must be large. What I see instead in the actual dashed line on Fig. 8c is large swings in OHCF on synoptic time scales, perhaps with a transition near 20-March.

1929/11: There is not enough information provided to understand Fig. 9b. What time scale is being correlated? Were the T and P signals detrended, or high-pass filtered? (If the latter, what filter characteristics? What should I make of short periods of large positive correlation *before* the data gap? I like the idea of a test for "synoptic" vs "katabatic" when the wind sensor dies; you just need to be clearer about it. I'm a little surprised that synoptic patterns have an in-phase relationship between T and P; I'd have expected maybe 90 degrees out of phase.

1929/24-26: I don't really understand this sentence. The upper-ocean budget represented by eq. 2 only cares about ice formation rate. You wouldn't expect any change in the salinity budget until ice starts to form, which you wouldn't expect until surface T drops to freezing. That is, the relationship between OHCF and flux is sensitive to current ocean state. Three things come from this: (a) You'd be better off just saying it's hard to do the budgets and relationship between sea ice production and heat flux; (b) Seems that advection of sea ice is omitted. It's probably justifiable to say that mCDW advection into Commonwealth Bay isn't important, but not so easy to dismiss sea-ice advection. (c) Seems you could also learn something from the "ocean salinity content"

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calculation independent of OHC.

1930/10-22: Naive question: does this mass balance work out? 20 kmˆ2 area producing 10 m of ice per year is 0.2 kmˆ2/year, not 25-40 kmˆ3/year. I suspect the issue is that you really apply the formation rate to a much larger area. In that case, specify the area and the rate and volume in one compact sentence. However, what you expect in a polynya is that ice forms rapidly but is advected away quickly. If advection is important, then how does that fit with the 1-D approach to sea ice formation represented by eqs. 1 and 2? I'm being deliberately lazy here – I think the answer is in your paper but it isn't clear – so you should spend more time explaining how advection figures into your calculations.

1933/7-10: I think you get yourself into trouble referring to "work done"; better to be precise about what it is that must be done to get back to the winter homogenous (homogeneous?) state. You have to cool it, as you say, but would that be sufficient to homogenize it? Just as important to getting deep convection going is to remove the salt deficit; this is not done by mechanical stirring (what I think of as "work" again stratification) but by the addition of salt during sea ice formation.

1934/12-16: I don't think you want to imply that the seal is a "Lagrangian float", just drifting along with the ASC. After all, most of the time the seals are moving quite rapidly independent of expected currents. I would certainly *not* suggest, as you do, that you have learned anything about the ASC speed from this, even though it is always interesting to note that seals choose this or that environment to hang out in.

General comment: there's a very recent paper by Massom et al. on the fast ice and MGT breakup, in JGR. This should probably be looked through, and cited if only for the details of the breakup.

FIGURES

Check all figures and callouts to figures for consistency in referring to specific panels.

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Figure 1 needs to be much clearer, and to have *all* place names on it referred to in the text, especially all the bays. I suggest replacing the color bathymetry background with a few choice bathymetry contours (500, 1000, 2000, 3000 m ?) so the seal tracks show up better. Map only needs to cover about 120-180 E.

Figure 2: I am assuming Figures end up larger than I see them here: Figure 2 is too small (for older readers!) I can tell the difference between S1 and S2 symbols, for example.

Figure 3: Is the grey vertical line at 148 E important? Two black lines on 3c are presumably gamma_n limits for mCDW? You need to explain them in the caption.

Figure 4: Seal name convention. I think you mean "seals I1 and I2", not "seals 44 and 52".

Figure 5: Since Panel C is DO, not potential temperature, first sentence of caption is wrong. Also, your callouts to these panels are confused by the presence of the DO panel. I suggest moving panels c and d down, so that the DO plot is the last (bottom) one. Then go through the text and figure-5 caption and ensure all references to the panels are correct.

Figure 6: Add seal identifiers I1 and I2 to each of the 4 panels. Note in caption that the location of C-28 is shown by the vertical dashed lines. Figure 7: This, along with Fig. 8, is your most important figure. I would remove panels d-f from Figure 7 and make a new figure out of them. I spent some time trying to map colors in these panels with colors in the Fig. 7a-c. However, one I did, I thought 7d-f needed more space to put on a good show. Regardless of how you do the figure(s), caption for current Fig. 7f should explain the black contours (presumed to be limits on gamma_n).

Figure 8: Caption should cite Eq. 1 for OHC (panel b). Blue line in panel c must be explained in caption, and probably in more detail in text. I suspect it's a least-squares fit, but with a slope that has very large error bars.

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Figure 9: panel b needs much more explanation: a summary in the Fig. 9 caption, and enough detail in the main text so that someone else could recreate this figure for their own data set(s). Fig.9c: Sea ice growth is based on eq. 2, yes? So, this needs to be explained in the caption. Also, units for sea-ice growth are different from the "%" scale implied by the y-axis. Provide a separate y-axis on the right-hand side of 9c, or put the sea-ice growth rate on its own panel. I don't think Fig. 9d gets explained very well in the text. You wouldn't expect sea ice growth to correlate with concentration until the water column is ready to overturn. Also, you have to treat the satellite-derived concentration with a grain of salt until the ice is quite thick. If you are going to keep Fig. 9d, perhaps add specific text to explain what you think is going on when the correlation is significant (3 synoptic events starting $\sim$28 Mar), and the rest of the time when it isn't.

Figure 10: The first two lines are the same as in Fig. 8e, yes? So, state this in caption, and use the same colors. It is not clear what the population is for which the standard deviations are taken. Is it for the eastern Commonwealth Bay area? Is the sample the spatial variability in an ocean/ice model with specific spatial resolution? regardless, is "standard deviation" what you mean (i.e., evaluated relative to a mean value) or do you want to show "root-mean-square" (not evaluated relative to a mean)? The latter gives you some measure of actual growth rate instead of just its variability. BUT ... why not just plot a mean value from Tamura et al 92008) that corresponds to the seal-data-based "mean" value? Why show standard deviation or RMS at all? Somewhere, perhaps in the Figure-10 caption, you should cite the means of the Tamura et al. plots and the seal-based values, perhaps for two periods: last two weeks of March (before sea ice plays a major role) and first 2 weeks of April (when sea cie concentration seems to matter).

TECHNICAL CORRECTIONS

1914/11: apostrophe in "year's"

1914/19: I believe units for ice growth here are cm/day, not cm/s!

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1915/14: write as "is the Antarctic coastal region"; i.e., add "Antarctic"

1915/26: can't say "most unique"

1915/28: "Of particular interest HERE (or "to us") is ...". Other people might find something different in the seal data to be most excited about.

1915/7: spell "sufficiently"

1915/9: "Water input to" (add "input")

1917/5: lower-case "glacial"

1917/13: "sea ice formation REGIONS over the"

1917/21: "known as THE "Home of the Blizzard""

1917/23: comma after "Bathymetrically"

1918/19: what is the symbol "gamma_n x". I've seen gamma_n (neutral density): I assume this is what you mean, but it needs to be explained. But what about the "x" ?

1918/11 and elsewhere: I don't know if the right word is "homogenous" or "homogeneous" (with the extra "e")

1920/25: why say "appear to change" ? Isn't it obvious that they DID change direction then?

1921/9 and elsewhere: citation of dates seems messy; sometimes "the 26 March", sometimes no "the". (I like no "the".)

1921/26: no hyphen needed in "brine rejection"

1922/1: "prefect" => "perfect"

1922/4: Units for OHC as defined by eq. 1 are just "J/mˆ2", not MJ.

1922/7: To be precise, you really should add the integration limits (z1, z2) to the integral

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sign. Then discuss how you will later look at different choices of z1 (and z2?)

1922/19: apostrophe after "seals"

1926/9-10: Again, need a map (Fig. 1) showing all features (including Buchanan Bay) mentioned in the text.

1926/24-25: Would read easier if you said "shoaling of the S=34 isohaline" (i.e., add the "S=")

1927/10: rewrite to "summertime profiles before the seals arrived in ..."

1927/16 and 18: after the first mentions of OHC here, parenthetically refer to (eq. 1). And maybe introduce an equation earlier for OHCF, even if it is just "OHCF=d(OHC)/dt"

1928/5: Commit to referring to "OHCF" rather than OHC flux"

1930/5: "formations" => "formation".

1930/12: Is the sea ice in Commonwealth Bay really "10 m thick"? How do you know this? If so, it must be land-fast, long residence time stuff, or "melange" including bergy bits.

1931/19: "the very core" sounds too colloquial!

1931/23: "the CTD data set detailed ..." (i.e., add "set")

1931/28: Again, is the ice really 10 m thick? If so, why? (lack of advection?)

1932/18: "profiler" => "profilers"

1934/1: I don't think "Of course" is the right sentence intro here.

1945/26: sentence finishing "... previously documented." seems to need more: "previously documented" what?

1936/1: where else would the SML be if not in the surface?

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1936/9: I think this should be "The heat content TIME SERIES showed that ..."

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