Responses to the reviews of the manuscript "Upper ocean stratification and sea ice growth rates during the summer-fall transition, as revealed by Elephant seal foraging in the Adlie Depression, East Antarctica" by G. D. Williams et al.

Note: In the following the reviewer's verbatim comments appear as *emphasized text*, our personal commentary appears in plain text, and the changes that have been made to the paper appear as **bold text**.

RESPONSES TO REVIEWER 1 - L. PADMAN

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 dont provide much information beyond those known already from traditional ship-based tran- sects. 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.

Thank you for your efforts in thoroughly reviewing this manuscript. We agree that the strongest results/findings are from the Commonwealth Bay seal observations in 2010 and fully accept that the ancillary results along the slope front do not provide much new information beyond the ship-based transects. In fact this is the main the reason the 2005 data was not published previously. However we still feel that it is useful to include the 2005 data here. Firstly, the difference in the seals migration between 2005 and 2010 is expected to be of interest to the seal biologist community, in particular given the context of the calving event in 2010. Secondly, whilst the minor extension of the ship-based coverage of the Antarctic Slope Front in itself does not reveal anything exciting for the physical oceanographers interested in this region, it does enhance our temporal and spatial understanding of the region and is worthy of inclusion. Finally, we anticipate that this paper will be read by seal/marine mammal biologists that may not necessarily have a strong background in the physical oceanography of this region, and so the 2005 data acts to introduce the reader to the ASF and the intrusions of modified CDW that are relevant to this area.

Therefore we have taken your alternate advice and sought to improve the figures and make them more user-friendly. We hope this has improved the manuscript to a level acceptable for publication in Ocean Science.

SPECIFIC COMMENTS.

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.

Correct. Our use of the 'shelf break' was incorrect.

Action taken: We have rewritten the text to state **'and onto a region of the conti-**

nental shelf termed the Mertz Bank,

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.)

Agreed.

Action taken: Figure 1 is too large in scale to clearly label the features of the Adélie Depression. We have now split Figure 2 into two figures (due to the on-line 'landscape' layout in Ocean Science) to improve the overall clarity. We have added all missing features introduced in the text.

We dont really need such a comprehensive "road-map" to section 4. You told us earlier what to expect.

Agreed.

Action taken: Text removed.

1924/10 and MANY other places: FIGURE REFERENCING. Settle on ALWAYS referring to the figure or specific figure panel (Fig. 5b). Dont refer to "Panel E" (for example) but to "Fig. 5e". That is, dont 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 dont 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 late in the paper prep.

Understood and apologies for the inconvenience this would have caused during the review.

Action taken: Manuscript reviewed to ensure Figure Referencing is consistent and correct. Upper case panel labeling changed to lower case.

You cant really talk about "numerous mCDW signatures". I think what you mean is "numerous profiles with evidence of mCDW presence", but I might be confused.

Agreed.

Action taken: Text edited to state **showed the most numerous profiles with evidence of warm mCDW, of all seasons.**

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.

Apologies.

Action taken: Naming convention made consistent.

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 isnt explained anywhere: it looks like it is meant to be a least-squares fit line but (a) I dont 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.

Agreed, this was not made clear in the caption. We had presented an estimate of the OHCF based on a least squares fit to the original hourly OHC data from b). We also present an estimate of OHCF based on the 8-day running mean of OHC in b). In hindsight this was overly complex, slightly wrong and at odds with the point we'd hoped to make. We have now simplified the story in our presentation of both OHC and OHCF.

Action taken: Figure 10c now only shows the OHCF based on the 8-day runnning mean data presented in Fig. 10b.

Paragraph re-written as **The hourly OHC averaged over the 200m depth range across the Commonwealth Bay region is $\sim$400 MJ m^{-2} at the beginning of March and decreases to $\sim$50 MJ m^{-2} at the end of April (green line in Fig. 10b). An 8-day running mean was applied to smooth the high frequency perturbations associated with the movement of the seal and internal dynamics within the survey area (blue line in Fig. 10b). Based on the OHF trend, assuming no further net advection of heat into the region from mCDW or otherwise, the OHC in the upper 200m is expected to have reached zero in the first week of May 2010. The OHCF based on this 8-day running mean (blue line in Fig.10c) fluctuates on roughly synoptic timescales, reaching $\sim$-200 W m^{-2} in a peak period between 9–16th March. After 20th March there is a transition to weaker OHFC, peaking at -100 W m^{-2} thereafter. Overall, whilst the OHCF is very sensitive to small changes in the mean layer properties, we do find a pattern of net heat loss from the region, consistent with our earlier finding from summertime ship-borne CTD data (see Fig.4a), that Commonwealth Bay is removed from the major pathways of mCDW influence in the Adélie Depression.**

*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.*

Agreed, more information is required. This was a running correlation with an 8-day window, i.e., 4 days of data either side of each data point. This is why 4 days of data were lost from the beginning and end of the record. The T and P data were de-trended beforehand. The short peaks of large correlation before the data gap are peculiar and we have no explanation to suggest to the reader. The first one signals the warm event, associated with an increase in air pressure from the 12-16th March. The second peak is associated with a cooling event, associated with decreasing air pressure. We can speculate here that these could be the result of small-scale katabatic events turning off and on, respectively. If there is a seasonal shift to the katabatic regime, it maybe initiated by such small events. However this is pure speculation. It would be great to be able to examine longer time-series of T and P in conjunction with actual wind measurements to explore this further. Alas, as noted, it remains very difficult to retrieve wind measurements from this location.

Action taken: Text edited to include **In the absence of wind measurements, we seek evidence of katabatic processes by examining a running correlation with an 8-day window between de-trended air temperature and pressure (Fig. 9b). The negative values in the first period, albeit punctuated by short positive events, suggest the influence of the synoptic regime (temperature and pressure are out-of-phase). In the second period (5–18th April) there is a shift to a more persistent positive correlation (temperature and pressure in-phase), implying the increasing influence of katabatic processes**

from April. That is, the pressure dropped significantly, but air temperatures remained cold, or decreased further, instead of increasing as expected in a synoptic regime. It is hoped that future AWS deployments can retrieve longer time-series in conjunction with wind data to test these ideas further. ...

Caption edited to include **Running correlation (8-day window) between linearly de-trended air pressure and temperature.**

Im a little surprised that synoptic patterns have an in-phase relationship between T and P; Id have expected maybe 90 degrees out of phase.

Again we were not clear. The axes for the pressure data were reversed to accentuate the synoptic vs katabatic patterns, which subsequently appear in-phase (synoptic) vs relatively out-of-phase (katabatic), and the opposite of what is expected, i.e., out-of-phase (synoptic) since low pressure systems are warmer and high pressure systems are colder. We will keep the figure as is, but improve the description in the text and caption to make this point clear.

Action taken: Text edited to include **(Fig.11a - note the reversal of the pressure axis).** and **The negative values in the first period, albeit punctuated by short positive events, suggest the influence of the synoptic regime (temperature and pressure are out-of-phase). In the second period (5–18th April) there is a shift to a more persistent positive correlation (temperature and pressure in-phase), implying the increasing influence of katabatic processes from April.**

Caption edited to include **Time series of: a) AWS data from Cape Denison. Air pressure (HPa, blue, y-axis reversed) and air temperature (°C, black); ...**

I dont really understand this sentence.

We believe there has been a misunderstanding brought about by a lack of clarity in our writing. We mean that polynyas can 'back-fill' after/during a period of high sea-ice growth, effectively choking themselves as the in-situ ice, indicated by increased sea ice concentration, retards the heat loss from the ocean to the atmosphere necessary to maintain the high sea-ice growth. We did not mean to imply the OHCF at this juncture. In this section we are solely considering the relationship between sea ice growth and concentration. As mentioned earlier, OHC should decrease to zero as the ocean temperatures reaches the freezing point.

Action taken: Text edited to **One explanation for this could be a short-term negative feedback during the latter stages of a sea-ice growth period. If the polynya forms sufficient ice, without advecting it away, then the growth rate would fall in relation to the reduced ocean-atmosphere heat flux.**

The upper-ocean budget represented by eq. 2 only cares about ice formation rate. You wouldnt expect any change in the salinity budget until ice starts to form, which you wouldnt expect until surface T drops to freezing. That is, the relationship between OHC and flux is sensitive to current ocean state.

This is correct. We partially address this when initially introducing the the mean layer temperature (Fig. 8a) and mentioning that mean salinity was already increasing across all available layers (Fig. 8d) before the surface freezing point was reached.

Action taken: None

Three things come from this: (a) Youd be better off just saying its hard to do the budgets and relationship between sea ice production and heat flux; (b) Seems that advection of sea ice is omitted. Its probably justifiable to say that mCDW advection into Commonwealth Bay isnt important, but not so easy to dismiss sea-ice advection. (c) Seems you could also learn something from the "ocean salinity content" calculation independent of OHC.

a) This is correct. b) This is correct. However given the configuration of Commonwealth Bay, in particular within the interior, and the prevailing wind direction, in particular within the interior region we are concerned with here, we do not expect there to be a significant amount of sea-ice advection. Nonetheless we should state this assumption explicitly. c) We considered this but at this stage felt it better to wait until we can make an assessment of the total full-depth salinity content when the mooring data becomes available.

Action taken: Sentence added **The main caveat to our interpretation here is the potential impact of sea ice advection from outside of Commonwealth Bay. This is likely to be negligent when the polynya is active, due to the morphology of the bay and the expected direction of the offshore wind forcing, but could be important during the inactive phase between peak events.**

Naive question: does this mass balance work out? 20 km2 area producing 10 m of ice per year is 0.2 km2/year, not 25-40 km3/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? Im being deliberately lazy here I think the answer is in your paper but it isnt clear so you should spend more time explaining how advection figures into your calculations.

Once again our writing was unclear. 'This' referred to the Williams and Bindoff (2003) result, and not the results in this paper.

The mass balance works out, though the different numbers quoted from the literature make different assumptions about the area over which the sea ice is growing/rejecting brine. In this paragraph we are comparing growth rates and annual ice production rates. However what is perhaps not clear is that these estimates use different methods. Williams and Bindoff (2003) completed full-depth salinity and heat budgets around a closed box against the MGT, excluding Commonwealth Bay. They then chose an active ice formation area within that box to attribute the T/S budgets to an ice production rate. The area of active sea-ice production was estimated to be 20 km^3, or 20% of a previous polynya area estimate of 100 km^3 by Massom et al., (1999). In hindsight this information is irrelevant to the discussion and confusing for the reader and so will be removed. But returning to the calculations briefly, 20 km x 20 km x 0.01 km (10 m) = 4 km^3 a^{-1}. In the case of the estimates from the Tamura satellite data, presented in Williams et al., (2010a) and updated here, sea-ice growth/production was summed over the Commonwealth Bay area of 40-50 km^3. Once again, just roughly, 45 km x 45 km x 0.015 = 30 km^3 a^{-1}.

While sea ice advection is certainly relevant, even critical to the efficiency with which a polynya produces 'enhanced' sea ice growth, it is not important in our calculations of total ice volume derived from brine-rejection. The grey area comes about when assuming a polynya area to derive the growth rate or vice versa. This is the weakness of the Williams and Bindoff (2003) result. However our calculation is a simple 1-D estimate, which is effectively averaged over the sampling area from which the 1-D profiles were taken. And this is comparable to the estimates in Williams et al., 2010a and our update here using the ERA-40 and ERA-Interim data, respectively, from Tamura because the sampling region of the seals closely matches the area over which the satellite estimates were taken.

Action taken: Text edited to improve the discussion of previous estimates. Sentence in question removed and the previous sentence edited to include **Sea ice production estimates were made by ?, for the polynya region adjacent to the MGT, using heat and freshwater budgets around a near-closed loop of ship-based CTD measurements and an assumption that active sea ice formation occurred over a 20 km^2 area.**

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.

Agreed. We mean to suggest that the summer/fall transition is a preconditioning of the water column to the full depth convective state of the winter mixed layer. We want to describe this as a two-stage process: 1) the cooling of the surface layer back to the freezing point and 2) the convective overturning of the remnant summer mixed layer below.

Action taken: Paragraph rewritten as

This study has demonstrated the processes of the summer-fall transition that a) re-condition of upper water column in the polynya region back to the near-surface freezing point necessary to initiate sea ice growth and then b) remove the salt deficit to initiate full convective overturning and new shelf water formation. The timing of these processes is relative to the air-sea interactions driving it and the initial stratification of the upper water column at the end of summer, influenced by the SML properties and presence/absence of mCDW. Regions with shallow/weak SMLs and/or minimal penetration from mCDW can therefore be expected to start the sea ice growth season, and subsequent shelf water formation earlier. It follows that any regional changes to these processes in future climate scenarios will lead to the changes in the start of the sea ice growth/shelf water season. More work is required to completely understand the temporal and spatial variability of mCDW around Antarctica relative to the major polynya regions.

*I dont 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.*

We agree, this paragraph and indeed this entire section of the manuscript is speculative in nature. We will eventually look in more detail at the specific dive behavior of the seal as it migrated westward in the vicinity of the ASF. Of course seals are not Lagrangian in their motion all the time, but we understand that at certain times, e.g., after feeding, they can go into a 'drift' mode. We also agree that we have not learnt anything about the ASC speed from this. However it is the first indirect inference that the ASC continues along this region and we simply wish to state that the mean drift speed estimated from the seal migration is within the range of expected ASC speeds from other observations around East Antarctica.

Action taken: Text edited to remove any suggestion that we've made direct observations of the ASC from our description and interpretation of the seal's movement. Specifically the text was changed to

The second oceanographic process that appears to be utilised by the seals, in particular in the SEaOS deployment, is the westward Antarctic Slope Current (ASC) over the upper continental slope. This region is documented around other regions of East Antarctica as having a narrow, fast-flowing westward jet (20–30 cm^{-1}) that is vertically homogenous and pinned to the 1000m isobath (???). While elephant seals can and do move independently of ocean currents and are not expected to act like Lagrangian floats, they are known to periodically operate in 'drift mode', during which time they could be expected to 'go with the flow'. Figure 2 showed that seal S2, after initially investigating the polynya region in the vicinity of the Mertz Bank and Mertz Depression, travelled westwards from 147–141 °E. We estimate a mean speed of 17.5 cm s^{-1} along this path, which is conservative given the extra time taken to complete a minimum of twelve dives plus additional random movements. This does not provide a robust observation of the ASC, however as it is in reasonable agreement with the reported speeds of the ASC jet in other regions, it does suggest the ASC exists acrosss the AGV slope and that the seal was utilising in its movement.

In both SEaOS and IMOS deployments, the seals that travelled to the AGV region all approached the continental shelf break between 146–148°E. As discussed in an earlier section, this is a region of mCDW penetration across the shelf break. Based on these two surveys we speculate that this is the third oceanographic process the seals are using, i.e. the upwelling of warm, saline mCDW onto and across the continental shelf break, as a preferential pathway into the continental shelf region. Potential benefits include the energy saved by staying in warmer water and 'going' with the flow. Again it is difficult to speculate on the robustness of this assertion with only two years of data. Future work examining the dive behavior of the seals will explore in greater detail their interaction with the processes of the Antarctic Slope Front.

General comment: theres 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.

Thank you. We've now read this paper but it appears to still pre-date the breakup of the MGT, focusing mostly on how the mechanics of the fast ice region to the east of the MGT influenced its stability. The only mention of the break-up is in a small addendum, post-submission, at the end of the paper.

Action taken: None

FIGURES.

*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.*

Agreed - in this and other figures the major problem has been our failure to optimise the layout for the online 'landscape' format of OSD. We've reduced the coverage of this map, though it still extends to cover all seal tracks, our goal being to advertise the entire area covered by the IMOS seals for interested parties in the SASSI community. We've changed from color to contour bathymetry and added relevant place names. We feel that Figure 2 is better suited to placing all location names in the Adlie and George V Land region.

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.

Agreed. This has now been split into 2 figures. All additional place names have been added. Fontsizes have been increased for greater clarity.

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

Figure 3 is now Figure 4. The grey vertical line is the boundary between the two bathymetry datasets used. It is an artifact. Correct, these are the 28.00 and 28.27 neutral density contours. Caption improved to explain this.

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

Apologies.

Action taken: Text edited. **Weekly locations of IMOS seals I1 and I2**

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.

Understood.

Action taken. Figure 5 now Figure 6. We still prefer to keep the section of potential temperature and dissolved oxygen from NBP00-08 together to maintain the seasonal progression of the panels. However we have switched panels C and D to avoid the sudden change from spring potential temperature to summer dissolved oxygen. We've also now added labels to the panels to make this clearer.

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.

Agreed.

Action taken: Figure 6 is now Figure 7. Seal identifiers added to the title of each panel. Caption edited to include **Vertical black dashed lines indicate the zonal location of iceberg C-28 between ∼145–147°E.**

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 γ^n*).*

Thank you, this is a good suggestion. Figure 7 is now Figure 8 and Figure 9. Contours and lines in Figure 9 are explained by reference to Figure 3.

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 its a least-squares fit, but with a slope that has very large error bars.

Figure 8 now Figure 10.

Action taken: Caption and text edited - see previous comment.

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 dont think Fig. 9d gets explained very well in the text. You wouldnt 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 ?28 Mar), and the rest of the time when it isnt.

See earlier response to similar comment. The sea ice growth rate is normalised for overlay with the sea ice concentration. This is now explained better in the caption. Regarding sea ice concentration and growth, here we are focusing on the core of the polynya. As the polynya fires up, the sea-ice concentration should initially increase. Thereafter, the polynya could be expected to become a victim of its own success and 'backfill/choke' on the ice it has produced, subsequently causing the growth rates to decrease

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).

The caption for this figure has been improved and the colors now match the lines in the earlier figure. The area over which the satellite data is averaged is the total Commonwealth Bay region. The sample has an area in the order of 50 km^2. Recent ocean/icec models used in this region have a spatial resolution in the order of 2-5 km^2. Yes, we mean the standard deviation. We choose not to plot a mean value because we want to show the range of estimates based on the two satellite datasets used. The standard deviation is necessary because we are trying to show the estimates over the 1992–2007 period because we expect our result from 2010 to be within this range of variability. We agree, it would be good to cite the means.

Action taken: Figure caption improved. Range of standard deviations unchanged. Mean seal and satellite estimates for sea ice growth periods in March and April cited in the caption.

Comparison of seal-derived (this paper) and satellite-derived (following ?, using ERA-Interim and NCEP-2 data) sea ice growth rates. Time series of seal-derived sea ice growth rates (m day^{-1}) are from Fig. 10e (6–200m in green, 6–300m in red). The shaded areas represent one standard deviation either side of the mean daily satellite-derived sea ice growth rates over the entire Commonwealth Bay polynya area from 1992–2007. Results are shown for NCEP-2 data (dark gray shaded area) and ERA-Interim data (light gray shaded area), respectively. The maximum daily satellite-derived values are shown as thick lines (again, NCEP-2 is dark gray, ERA-Interim light gray). We identify two sustained periods of sea ice growth from the seal data, i.e., 4–29th March and 3–18th April, and estimate mean sea ice growth over these periods to be 5.1 and 7.2 cm day^{-1}, respectively. The mean sea ice growth estimated from both satellite datasets, over the same dates between 1992–2007, is 3.3 and 4.3 cm day^{-1}, respectively.

TECHNICAL CORRECTIONS

Thank you for your efforts in detailing these. We apologise for the number of basic errors.

Action taken: All suggested technical corrections have been made.

RESPONSES TO REVIEWER 2 - ANONYMOUS

GENERAL COMMENTS.

Review of "Upper ocean stratification and sea ice growth rates during the summer-fall transition, as revealed by Elephant seal foraging in the Adelie Depression, East Antarctica," by G.D. Williams, M. Hindell, M.-N. Houssais, T. Tamura, and I.C. Field. In this paper the authors use CTD observations from instrumented Elephant Seals of the Antarctic coastal ocean off of Adelie and George V Land during the summer-fall transition during two separate years (2005 and 2010). The 2005 observations are primarily used to examine modified Circumpolar Deep Water along the continental shelf edge. The 2010 observations are primarily used to examine the time variability of upper water properties during the summer-fall transition in Commonwealth Bay and use this to estimate the sea-ice growth in the Commonwealth Bay polynya during this time period. There is also some speculation as to what these observations show about the foraging habits of the seals. In general, I thought this paper was well written and the subject matter and results are interesting (to me anyway and I would think to a wide group of high latitude re- searchers) and certainly appropriate for publication. I have several concerns, but these are all minor and should be easily dealt with by the authors. My summary recommendation is to publish once the editor is satisfied that the concerns listed below have been addressed.

Thank you for your efforts in completing this thorough review.

SPECIFIC COMMENTS

Did the MGT really extend all the way out to the continental shelf break? The pre-calving northern end looks to be at about 66.7S on Figure 2b while the shelf break is north of 66.0S.

Correct, the MGT does not extend all the way to the shelf break - the text used was misleading.

Action taken: We have rewritten the text to state **'and onto a region of the continental shelf termed the Mertz Bank,**

Can the authors label Buchanan Bay, Watt Bay and Commonwealth Bay (at least Commonwealth Bay) on either Figure 1 or 2?

Yes.

Action taken: Figure 2 has been significantly improved with respect to the naming of regional features.

Do the authors have access to any of the results from the ALBION project and, if so, do they show any salinities > 34.77

The hydrography of the coastal bays in the Adelie Depression area is indeed the focus of the Albion project, based on moorings and repeated CTD surveys. The time-space coverage of the Albion data set- is larger than the one provided by the elephant seal tracks used in the present manuscript. Several studies analysing the interannual variability and the spatial contrast in the coastal bays are currently under way. We indeed observe the highest bottom salinity (reaching 34.8 in some years) in Commonwealth Bay, as already suggested by Williams and Bindoff (2003). Considering the different focus and context of the present analysis, we now think it is not necessary to mention the Albion project in the manuscript and prefer to take out the sentence related to it.

What value was used for the heat capacity?

We calculated the heat capacity for each data point using the salinity, temperature and pressure at that data point, following the CSIRO seawater routine SW_CP. This produced a range of values from 3.9621 x 10^3 to 3.9987 x 10^3 J kg^{-1} C^{-1}.

Do the authors have any error estimate for the location data?

This is work in progress. There has been some recent paper published on this and we now provide a citation for it. Basically data is now given an error class for the location. The maximum error class is for > 1500m. Our data is in this error class, and in fact often appears to much greater than 1500m (see Figure 2 inset).

Action taken: Citation provided to Vincent et al., 2002 and Patterson et al., 2010.

I believe Charrassin et al. (2008) estimate that the biggest error in their method for estimating sea-ice production was due to ignoring surface precipitation. I am not familiar with the precipitation in this area, but I imagine it is much much less than the ? 10 cm/d of sea-ice growth the authors estimate. However, as the authors point out, this is an area with exceedingly strong winds and lots of surface advection of ice (hence the polynya). I would think there might be some horizontal salinity advection here too. Do the authors have any thoughts on error estimates of this method in this particular location, especially since this is a one-d method and ignores advective processes?

This is a very valid point. You are correct, the precipitation is weaker in this region of the Antarctic coast, but that salinity advection is potentially very important, in particular because there is another polynya region upstream from Commonwealth Bay. Our 1-D model could just be showing the influence of sea ice production outside of Commonwealth Bay. We anticipate being able to address this when further estimates of sea ice growth rates in Commonwealth Bay are made using the mooring time series from the ALBION project.

Action taken: Paragraph added on this subject after the sea ice growth rate estimates are made

? stated the biggest error in this 1-D method of estimating sea ice production was neglecting the impact of surface freshening by precipitation. However the precipitation rates in Commonwealth Bay are likely to be small compared to the 5–10 cm day^{-1} of sea ice growth estimated here. The largest potential source of error in our case is the lateral advection of salinity, in particular from the influence of the polynyas to the east in Watt and Buchanan Bay. In the most extreme scenario, the increase in salinity in Commonwealth Bay (see Fig. 10d) could be purely the result of sea ice production in the polynya regions upstream. However the case for strong sea ice production/brine rejection in Commonwealth Bay is compelling, including the mooring data from 1998–2000 in ? showed that there is a distinct input of salinity from sea ice production in the Commonwealth Bay region when comparing the Adélie Sill region to the region west of the Mertz Glacier. We anticipate being able to quantify the influence of salinity advection when the ALBION mooring data from Commonwealth Bay is published, in particular the instruments at depth in the Commonwealth Bay Hole.

Do the authors know of a reference that shows that the weaker spring mCDW signal relative to August is due to the increase in shelf water from August- October through the *sea-ice growth season? Any possibility that this could just be variability in the on-shelf intrusions of the mCDW?*

We don't know of such a reference. We expect that there could be variability in the strength, timing and location of the mCDW intrusions. However it has not been directly quantified through the current set of observations, i.e., the seasonal snapshots across the shelf break come from different years and the mooring observations are outside the main mCDW intrusion pathway. We do expect the salinity and density of the shelf region to increase from August to a maximum in October and that this should 'deter' mCDW intrusions, or at very least significantly dilute their properties.

Action taken: Text edited to include **In spring (panel B, late October 2004), the water column is predominantly cold, dense shelf water. The shelf-break transect showed weaker mCDW properties relative to August. While this could be variability in the strength, timing and location of mCDW intrusions for this region, we can also expect that this to be related to the increase in shelf water from August–October through the last part of the sea ice growth season (?), which presents a density barrier that blocks/dilutes the mCDW intrusions/properties.**

How is the blue line in Figure 8c computed? It looks too straight to be a flux computed from the 8-day running mean in Figure 8b. Is it just a linear fit to the dashed line? Also, if it is, the high temporal resolution ocean heat content flux (dashed line) for the last part of the time series (past 18 Apr.) certainly does not look like the flux has decreased "to 0 at the end of April."

Agreed, this was not made clear in the caption. We had presented an estimate of the OHCF based on a least squares fit to the original hourly OHC data from b). We also present an estimate of OHCF based on the 8-day running mean of OHC in b). In hindsight this was overly complex, slightly wrong and at odds with the point we'd hoped to make. We have now simplified the story in our presentation of both OHC and OHCF.

Action taken: Figure 10c now only shows the OHCF based on the 8-day runnning mean data presented in Fig. 10b.

Paragraph re-written as **The hourly OHC averaged over the 200m depth range across the Commonwealth Bay region is ∼400 MJ m^{-2} at the beginning of March and decreases to ∼50 MJ m^{-2} at the end of April (green line in Fig.10b). An 8-day running mean was applied to smooth the high frequency perturbations associated with the movement of the seal and internal dynamics within the survey area (blue line in Fig. 10b). Based on the OHF trend, assuming no further net advection of heat into the region from mCDW or otherwise, the OHC in the upper 200m is expected to have reached zero in the first week of May 2010. The OHCF based on this 8-day running mean (blue line in Fig. 10c) fluctuates on roughly synoptic timescales, reaching ∼-200 W m^{-2} in a peak period between 9–16th March. After 20th March there is a transition to weaker OHFC, peaking at -100 W m^{-2} thereafter. Overall, whilst the OHCF is very sensitive to small changes in the mean layer properties, we do find a pattern of net heat loss from the region, consistent with our earlier finding from summertime ship-borne CTD data (see Fig. 4a), that Commonwealth Bay is removed from the major pathways of mCDW influence in the Adélie Depression.**

Wouldnt this imply little or no "net" advection of heat into the region? There could *still be heat advecting in from below in April that equals the heat mixed out from above. Note that any heat from mCDW would likely be coming in to the region from below 200m (see Fig. 5).*

Correct. See previous response for action taken.

What exactly does "de-trended" mean here? That is, what kind of trend is removed: linear, sinusoidal annual cycle, something else?

By detrended we mean the linear trend is removed.

Action taken: This is now stated in the text and captions.

I guess that the correlation at each point of the time series shown in Figure 9b is the correlation between the temperature and pressure over some window centered on the time plotted. If so, can the authors give the window range? If not, can they provide more details?

Correct. We should have provided this information.

Action taken: Figure 9 now Figure 11. Caption edited to include **Running correlation (8-day window) between linearly de-trended air pressure and temperature.**

Same questions as above, but for Figure 9d. Also, which of the three ice concentrations presented in Figure 9c was used for the correlation calculation?

Agreed.

Action taken: Caption edited to include **D) Correlation coefficients between linearly de-trended sea ice growth (based on 6–200m layer in Figure 10e) and sea ice concentration (4-day running mean from Fig 11c). Dashed lines indicate 95% confidence level. Open circles indicate significant correlations (p < 0.05).**

I think the result is promising too, but it seems really difficult to tell without the 2010 Tamura estimates. If 2010 is a normal year, then it seems that the satellite method underestimates ice growth by a significant fraction. I like the idea of plotting the 1-sd range for the satellite estimates, but I think it would also be helpful for the reader if the authors could provide a mean value of the ice formation over this time period for all four (6-200m salt balance, 6-300m salt balance, Tamura w/ NCEP2 forcing, Tamura w/ ERA-Interim forcing) estimates.

Agreed. Due to some latency in the satellite data required for Tamura to provide his estimates for 2010, we cannot address this yet. The inclusion of a mean value of ice formation over this period is a good idea. We experimented with including this on the graph, however it became too busy.

Action taken: Following a similar suggestion from another reviewer, we now include mean results for the two major periods of sea ice growth, from both the seal and satellite data, in the caption. **Following Fig.10a, the mean sea ice growth estimated from both satellite datasets, over the two sustained periods of sea ice growth from the seal data, i.e., 4–29th March and 3–18th April, between 1992–2007, are 3.3 and 4.3 cm day^{-1}, respectively.**

FIGURES.

Figure 1: It is difficult to distinguish the grey seal tracks from the bathymetry in some

areas.

Agreed

Action taken: Figure 1 reworked replacing colour bathymetry with contours.

Figure 2: I had to really really blow up the figures to see the details on my screen (no way anyone could see some of these in a print version). I suggest breaking this figure up into two larger ones. I never did find the labels for Adelie Bank (AB) and Adelie Depression (AD) on either figure. Last sentence of figure caption is confusing: Either it is only meant for figure 2b (and the previous sentence is only meant for 2a) or there is a typo ("Pre-calving" should be "Post-calving") in which case it is only appropriate for 2a (where the only outline is post-calving).

Agreed.

Action taken: Figure broken up into two larger ones. All features now labelled.

Figure 3: Please label or describe the different black lines (i.e. 28.00 and 28.27 neutral density, surface freezing point, etc.) in 3e.

Action taken: Additional lines labelled on the Figure and described in the caption.

Figure 5: I think decreasing the contoured temperature range would help visualize the different water masses. Right now, it is hard to tell much difference below -1.5 and I think the maximum value could be reduced from 0.0 (-0.5 maybe?) and still have the two warm locations in 5e stand out.

Good idea. We tried this, but it turns out that the larger range help make the mid-range values we are trying to highlight, stand out more.

Action taken: None.

Figure 6: Do the vertical dashed lines near 145 and 146.5 represent the extent along C-28?

Correct

Action taken: Caption now includes **Vertical black dashed lines indicate the zonal location of iceberg C-28 between ∼145–147°E.**

Figure 10: What is the dotted black line near the bottom of the shaded ERA-Int estimate?

This is simply the overlap from the NCEP-2 datarange.

TECHNICAL CORRECTIONS

Thank you for your efforts in detailing these. We apologise for the number of basic errors.

Action taken: All suggested technical corrections have been made.