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Discussions

Interactive comment on "Sea ice and the ocean mixed layer over the Antarctic shelf seas" *by* A. A. Petty et al.

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We thank the editor and reviewers for their comments regarding our submission. Please see below for both reviews and our replies (in blue).

1   Reviewer 1 - Laurie Padman

This paper describes development and analyses of a mixed layer model for Antarctica, coupled to a sophisticated sea ice model (CICE). The authors describe the model outcomes for four regions (Ross, Amundsen, Bellingshausen, Weddell) in terms of mixed layer properties and depth, and sea ice growth, decay and advection.

Overall, the goals and approach to this problem are laudable. The results suggest that the model is doing a good job of representing sea ice processes and mixed layer response, despite the "passive" nature of the deep ocean which is held relatively constant by nudging to climatology. It appears the model would provide a valuable framework for further sensitivity studies.

However, I found the technical development section to be too confusing to understand sufficiently, and some sections of analysis to be too convoluted. With this in mind, I've limited comments here to "major" comments only, in no specific order.

1.1   Main/general points

1) Problems begin early (in the Abstract) where the authors talk about "surface power". In the Abstract, this term is not defined. Later, it is defined mathematically, but not in practical terms. It comes across as "potential to deepen or shoal the mixed layer", but it is never clearly explained how wind stress, cooling and brine rejection are put on a level playing field. Presumably wind stress loses out in terms of deep water production because it does not penetrate far, even in a homogeneous ocean because of planetary rotation and low vertical viscosity. But, in layman's terms, how is this constraint relative to buoyancy forcing terms enforced?

Wind stress acts as a direct input of turbulent kinetic energy to the mixed layer (for deepening). The buoyancy terms (brine fluxes, P-E, and surface heat fluxes) result in convective mixing which provides the mixed layer with a further source of energy to deepen (or a sink of energy and potential to shoal the mixed layer through a positive buoyancy flux).

We agree that it is best to avoid using less familiar terms like power input in the abstract so we have replaced the following: "By deconstructing the surface power input to the mixed layer" with: "By deconstructing the surface processes driving the mixed

layer depth evolution". We have also replaced all cases (in the abstract) of: "mixed layer surface power input" with "mixed layer deepening".

We have also added the following to the start of the power input description section: "The mixed layer is provided with mechanical energy to deepen through both convective mixing, driven by surface buoyancy fluxes (where gravitational potential energy is converted into kinetic energy within the mixed layer) and directly through wind mixing."

2) Many sentences are too rambly. Rule of thumb: if a sentence runs longer than 3 lines, it probably needs to be broken down.

Agreed. We have gone through the manuscript and shortened several sentences to improve readability.

3) Explanation of model is insufficient. I recommend the authors include a table of *all* variables in the paper. This may identify constants and variables that have not been adequately explained. Figure 3 is key; however, the caption assumes that everything on this figure needs to be obtained from the main text. A more detailed caption would help, along with including fixed parameters like h_s.

Agreed. We have now produced a table of all model variables. We have placed this in a separate supplementary document (Table S1) as it spans more than one page and isn't deemed to be crucial to the understanding of the model description. The following sentence: "Constants and fixed parameters referred to in this section are listed in Table 1." has been replaced with: "Constants and fixed parameters referred to in this section are listed in Table 1, while a full list of model variables is given in the supplementary information (Table S1)."

We have now added a more detailed caption and have improved Fig. 3 to highlight the surface and mixed layer depths in the revised manuscript (now Fig. 4).

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4) I couldn't find explanations of T_f, h_s (although I finally found it in Table 1), and it looks like 'w' might the same as 'ω' ?? This should be solved by ensuring that every symbol is explained at first appearance. Not much mention of model vertical resolution: I first became aware of this in Table 1.

Agreed. w should be used for entrainment as opposed to ω. We have changed this in the revised manuscript. T_f wasn't explained so we have added the following to the manuscript where the term first appears: "where T_f=273.15-0.054S_{mix} is the freezing temperature for seawater at the surface."

h_s is explained where it first appears, is also listed in the table and is now also shown and described in the improved model schematic (Fig. 4) as mentioned above.

Vertical resolution is not of importance great this study considering it is a bulk mixed layer model (not a z-coordinate ocean model). To make this clearer we have replaced: "We use the mixed layer energy balance formulation.." at the start of the mixed layer model description with: "We use the zero-dimensional bulk mixed layer energy balance formulation.." in the revised manuscript. There is a course deep ocean grid below the mixed layer as described in Sect. 2.2. The inclusion of a model vertical resolution in the table was a misleading error which we have now removed.

5) Text on many figures is too small, at least as figures print out in "printer-friendly version". E.g., it is almost impossible to read text on Fig. 2 (also, to see black direction vectors on a generally dark background in 2f).

Agreed. The text size has been increased in many of the figures to improve readability. We have also changed the colour scale in Fig. 2f (now Fig. 2a) to lighten the background and make the vectors easier to see.

6) The model fit with expectations, primarily in sea-ice characteristics, production, melt and advection, but also mixed layer response, is very good. The maybe naive question I have is: How much does this represent good physics, and how much comes from

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relaxation schemes to keep deep T,S, close to climatology? E.g., I imagine that the SW Weddell Sea is easy to deep-convect in, because its climatological stratification arises from deep convection, while the A and B seas don't allow this because the clim. stratification is too strong. (In those cases, surface forcing can eat into the remnant WW layer, but not more unless deep diapycnal mixing is really high.) That is, if you specified poor deep-ocean climatology, how much would your results change? Does this even feed back into sea ice evolution so that sea ice must match reality at some level?

The influence of the ocean properties in determining mixed layer evolution (and also the sea ice behaviour) was investigated in our recent publication (Petty et al., 2013, JPO). This showed that the atmospheric differences between the Weddell/Ross and Amundsen/Bellingshausen could dominate the mixed layer response over differences in the deep ocean properties. The deep ocean conditions, however, appeared to play a crucial role in simulating realistic sea ice behaviour. We therefore agree with the reviewer that the deep-sea properties are an important factor in the creation of realistic sea ice and mixed layer cycles. The impact of the deep ocean was not investigated in great detail as the earlier idealised study provided a more simplistic and useful method of demonstrating this impact.

To clarify this, the following has been added to the revised manuscript at the end of the modelled mixed layer discussion: "The idealised modelling study of Petty et al. (2012) demonstrated that that regional atmospheric differences are sufficient in controlling the potential formation of deep mixed layers. However, a realistic deep ocean was shown to be important in simulating a realistic sea ice and mixed layer cycle. We therefore believe the use of a realistic 'climatological' deep ocean is an important factor in the realistic simulation of the sea ice and mixed layer depth within each of the regions used in this study. This was not investigated further in this study as the idealised modelling setup of Petty et al. (2012) provided an easier to understand evaluation of this impact."

7) Staying on the same "problem": If the deep climatology is fixed with some fairly short

nudging time-scale (1 y), then interannual variability of upper-ocean and sea- ice might be constrained. E.g., in the Ross Sea, Comiso et al. (2011) and Drucker et al. (2012) found quite strong trends (and interannual variability) in sea ice production and export, and you already comment that perhaps increasing freshwater from the A. Sea is driving trends in Ross Sea shelf water masses. But, by nudging these back to climatology, you set an artificial deep stratification that, I suspect, would damp the modeled interannual ocean and ice variability forced from atmospheric variability.

Yes, we agree that in this model configuration we are limiting, to some degree, the ability of the ocean to provide a strong feedback mechanism to any surface trends. As the main focus of this study is on the mean 'climatological' state of the surface driven formation of shelf waters, we feel this shortcoming does not interfere with the main results of this study. The alternative would be an ocean model that would need to maintain realistic shelf sea properties throughout four different shelf sea regions to provide a realistic feedback mechanism. This is clearly a large challenge and worthy of further investigation.

8) I got a little lost in sea ice forcing. Eq. 21 appears to constrain ice/ocean stress to be 15 degrees off the ocean velocity. That obviously doesn't apply where internal stresses get too high, e.g., against coasts like the western Ross where advection is forced northward regardless. There is insufficient information about CICE *dynamics* to decide what the impact of the assumed rotation angle is, nor did I get a sense of whether it matters or whether ocean currents *not* associated with the wind stress and thermodynamics might be important. It is good to use CICE, but one question some of us have is whether simpler models might be good enough? Maybe codes with a single ice thickness class are adequate?

We feel there may have been some misunderstanding of sea ice dynamics here. The ice is not assumed to be in free drift so the full momentum balance equation for sea ice is used within CICE (See Hibler (1979) for a summary). The ocean turning angle causes the ice-ocean stress (one part of the momentum balance) to act at 15° to the ice

velocity due to the ocean Ekman spiral. Note that the near-surface winds have resolved the Ekman spiral whereas the geostrophic ocean currents haven't. Internal stresses and lateral stresses (e.g. from fixed boundary coastal regions) will also apply to the momentum balance when appropriate. It is also worth pointing out that ice thickness classes have no impact on the momentum balance.

We initially ran the model using ocean currents from reanalysis, however this caused erroneous ice motion when compared with observations. Setting the geostrophic ocean currents to 0 allowed for the impact of an ocean turning angle which improved the fit of ice motion to observations. Ocean currents around Antarctica are still poorly understood limiting any potential improvement to the approach taken here.

9) Figure 7 caption needs to explain the dots on 7a.

Agreed, we have added the following to the caption of Fig. 7 in the revised manuscript:"The black crosses highlight grid cells where the maximum mixed layer depth is greater than 90% of the water column depth."

10) On page 4339 you claim that "the regionally varying surface fluxes can directly explain the bimodal distribution in shelf seabed temperature" with caveats. Not really. There are large-scale reasons for distribution of seabed T that have very little to do with surface fluxes over the continental shelves. Furthermore, as I've suggested above, it feels (to me, at least) like nudging to deep climatology forces the model to set the upper ocean to be in balance (over a time scale of a year or so) with the originally specified climatological T and S fields.

Again, this was investigated in more-detail in the earlier publication (Petty et al., 2013, JPO). While we agree that the reduced stratification of the WR seas likely encourages deeper mixed layers, the earlier research suggested this was only a secondary impact compared to the strong regional variations in surface forcing. As this study provides evidence of a strong bimodal surface forcing, we feel the study adds weight to the hypothesis that differences in the surface forcing (as opposed to

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ocean stratification/temperature) *can* explain the bimodal distribution. Use of the word 'can' implies we are not ruling out other competing mechanisms such as ocean stratification, deep ocean heat transport etc.

To make this clearer we have replaced:"...regionally varying surface fluxes can explain the bimodal distribution..." with "...regionally varying surface fluxes are sufficient to explain the bimodal distribution..,". We have also replaced the caveat at the end of the paragraph: "However, the simplicity of our model means that we are not able to rule out a contribution to this distribution from ocean dynamics." with "However, the simplicity of our model means that we are not able to rule out a competing contribution to this distribution from regionally varying ocean dynamics (e.g. ocean stratification or deep ocean heat transport) as discussed in Petty et al. (2012)."

11) Minor. Sometimes uncertainties are cited at too high accuracy. e.g., on page 4344, you have 578 +/- 39.8 km^3. Perhaps 580 +/- 40 would be sufficient?

Agreed, we have changed this in the revised manuscript.

12) A lot of your comparisons of specific numbers (total ice production and import/export etc) with previous studies would be clearer and more concisely explained in Tables.

The reason this approach was not taken is because the 'observational' studies often analysed different polynyas/regions and also employed different methods. Simply including the values in tables would potential lead to misleading comparison between different studies.

13) The method for defining polynyas so that you can compare polynya production with previous estimates seems somewhat ad hoc so that, when you cite fraction of total production in specific polynyas relative to the total, the numbers don't carry much weight. I would prefer that you just state the importance of polynyas and that your model is too coarse to really capture them.

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We feel that the polynyas are too crucial to the shelf sea ice mass balance not to at least attempt to quantify their significance in this study. We are well aware of the limitations of our approach in this regard and have attempted to make this clearer in the revised manuscript. We have also attempted to make clearer where we have used this approach to compare to observational estimates (which only offer polynya growth estimates) and where we have attempted to add value by comparing the estimated and modelled polynya growth to the total shelf sea ice growth. Quantifying the fractional contribution of polynya ice growth we feel to be an important idea and one that has only been touched on briefly by recent studies. Section 3.4.1 has been reworded based on these ideas.

14) I do not include typo/grammar and minor comment suggestions here. Will do this later or offline via the Editor.

We have checked through the manuscript for additional typos/mistakes. We appreciate any additional errors you may be able to highlight.

2   Reviewer 2 - Anonymous

In "Sea ice and the ocean mixed layer over the Antarctic shelf seas" Petty, Holland, and Feltham combine the sea ice model CICE with a mixed layer ocean model to study the influence of surface forcing (atmosphere and sea ice) on the formation of deep mixed layers in the Antarctic shelf. The lack of a complex, 3-dimensional ocean model is compensated by restoring ocean grid cells below the current mixed layer to observed (reconstructed) temperature and salinity fields. The study has two objectives, a technical and a scientific. First, the new model is validated for its capability of simulating sea ice coverage, formation as well as export, and mixed layer depth against observations and previous model results. Second, the scientific focus is on the annual cycle of mixed layer depth and its forcing thereby comparing processes in two contrasting shelf sea

types, the Amundsen and Bellingshausen seas vs. the Weddell and Ross seas.

The authors nicely demonstrate the dominance of surface salt fluxes (from sea ice growth) over, for instance, wind forcing on the formation of a deep mixed layer using power as the central quantity. Further, it is an interesting point of view to sort the four largest shelf seas into two categories based on their different deep water masses' properties.

Although I like the idea of using a simplified model in order to get a clear understanding of certain processes, the lack of a 3-dim. ocean model and advection in the ocean (the mixed layer (ML) model does not account for horizontal interaction [p.4327/l.6]) are the greatest shortcomings of this study. The uncertainties introduced by neglecting exchange from the ML to the deep ocean and advection must be better discussed.

Using a simplified, computationally less expansive model enables to run the model with high spatial resolution. I am thus disappointed that the authors decided to run with 55km, a resolution commonly used for global 3-dim ocean models these days. It seems that the development of a sea-ice/mixed-layer model should enable the explicit simulation of coastal polynyas on a, say 5km grid. In this case it would not be necessary to use an arbitrary ice growth threshold (p.4342/l.23) to identify likely positions of polynyas.

We agree that resolving polynyas would have improved the results presented in this study. However, the resolution required ($<$5 km) would have increased the computing time considerably. As we had modified CICE substantially by including this new mixed layer scheme, the model was run repeatedly to test the model was working correctly (in different configurations) and to tune several model parameters to simulate a realistic sea ice and mixed layer cycle within all four regions of interest. This would not have been possible if we were running it at such a high resolution (with the computing power available to us).

The authors provide detailed background information on water masses (temperature

and salinity) and their formation in the Southern Ocean from the literature, which I appreciated much. However, they do not compare modeled mixed layer temperature nor salinity to observations. Instead, it is inferred that, for instance, when the mixed layer exceeds 90% of the total water column depth that high saline shelf water (HSSW) is/would be formed. The authors need to compare their shelf water from deep mixed layers with observed properties. Being able to simulate a deep mixed layer is great but only if the bottom water produced has realistic properties.

We did not previously state that HSSW formed wherever the water column become destratified (e.g. we highlighted regions such as Berkner Bank in which the salinity remained lower than HSSW). We agree that this could have been explained better hence the change in the bottom salinity figure and introduction of a bottom temperature figure (Fig. 10) in the revised manuscript. We have also extended the comparison of summer/winter bottom salinity (and now temperature) to observations where possible. See later main/general comment for more information of the changes we have made.

Although I think the study is interesting and makes a convincing point in sea ice formation being the dominant driver of deep mixed layer formation, I need answers to the addressed caveats before I can recommend publication. I apologise for submitting my review late.

2.1   Main/general comments

1) The introduction provides a comprehensive overview of the water masses and processes in the Antarctic shelf seas. I'd like to suggest to add a sketch, either in the style of a T-S diagram where water masses are labeled, or two idealized cross-sections (depth vs. latitude) showing the layering of water masses, one for each shelf sea type (i.e. WR and AB) as in Petty et al. (2013, JPO, Fig. 2).

Agreed. We have included a new figure showing idealised cross sections of the two

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regimes (WR and AB), adapted slightly from the JPO paper (Fig. 3 in the revised manuscript). We have also added the following to the manuscript to refer the reader to both schematics: "A simplified schematic of the WR water masses and mixing processes is shown in Fig. 3a." and "A simplified schematic of the AB water masses and mixing processes is shown in Fig. 3b" at the end of each paragraph discussing the relevant regimes (WR and AB).

2) In section 3.1 the authors present a realistic assessment of their models ability to match the observed sea ice distribution. However, the model produces much thicker ice than observed near the coast which likely affects ice growth rates and hence mixed layer formation. The actual difference cannot be estimated from Fig. 6 because modelled ice thickness exceeds the color scale maximum.

Agreed. We have increased the colour scale to make the difference between model and satellite estimates clearer. While this does show up differences, the uncertainties in the 'observed' sea ice thickness prevents a detailed comparison. This is discussed more in the detailed comment below.

3) I doubt that reasonably well simulated mixed layer depth guarantees realistic shelf water properties. Please do present T and S of modeled dense shelf water. Bottom salinity is shown in Fig. 8 with the result that in many grid cells with very deep mixed layers (cells with 'x') the bottom salinity is not much different from the background forcing. Why does a deep mixed layer sometimes yield much higher salinities and sometimes not?

We have now introduced a figure of bottom temperature (Fig. 10) as well as the bottom salinity figure mentioned (now Fig. 9). We have also changed the way this figure is produced such that we now show the salinity (and now also temperature) in March and October as opposed to the summer minimum and winter maximum. In the previous method, the actual impact of destratification was being masked in some cases as the algorithm was just detecting the more saline bottom water which was actually

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present before destratification. By changing this to October (roughly the end of winter) the figure is now more representative of the properties due to destratification (where the crosses are). This shows up bigger and more understandable differences between the bottom temperature/salinity in summer and winter. The results from both new figures are discussed in Sect. 3.3. It is also worth pointing out that the later section (Sect. 3.4.1) looks at the annual ice growth and compares this to the bottom water mass properties to try and understand why certain regions produce HSSW while others do not, highlighting bathymetry as potentially a key factor.

4) It's tricky to neglect ocean circulation on the shelves. You account for the deep large-scale circulation to some extent by restoring to the deep ocean forcing, but your simulation lacks dynamic processes such as Ekman convergence/divergence and horizontal transport within the mixed layer.

We agree these processes are likely important in the behaviour of the shelf seas. We choose to simplify the study by focussing on surface processes alone. While this is clearly a shortcoming if one wishes to understand all the processes taking place over the continental shelf, we feel the manuscript makes clear why it is we adopt this approach.

While the reviewer points out deep ocean restoring is (somewhat crudely) accounting for deep (large-scale) circulation on/off the shelf, we also restore (albeit on a slower timescale of 1 year) the mixed layer/surface salinity towards climatology - see: "The mixed layer salinity is restored back to the annual climatological (30 m) value with a period of one year. This is introduced to balance any loss of salt from deep ocean relaxation and simplistically represents the export of mixed layer waters off the shelf." in the manuscript. This does go someway to addressing the issue of horizontal transport within the mixed layer. This is also touched on in one of the minor corrections (referring to 4341/6)

5) I probably missed it in the text somewhere, but does the deep ocean forcing have

interannual variability or is it a climatology?

The deep ocean is a climatology."which consist of climatological fields of in situ data". The original text wrongly stated that "the mixed layer salinity is restored back to the monthly climatological (30 m) value". This has now been replaced with "the mixed layer salinity is restored back to the annual climatological (30 m) value". The World Ocean Atlas has monthly, seasonal and annual climatologies. Due to data sparsity, especially within the Antarctic shelf seas, we choose to use annual climatological values for the deep ocean grid. To make this clearer in the text, "These data have been interpolated onto the CICE grid" has been replaced with "The annual climatology has been interpolated onto the CICE grid".

2.2 Detailed comments

4322/7 abbreviation HSSW needs to be introduced

Agreed. We have replaced: "...regions (leading to HSSW formation)" with: "...regions leading to High Salinity Shelf Water (HSSW) formation" in the revised abstract.

4323/22 please add the temperature and salinity properties of HSSW

Agreed. We have replaced "formation of High Salinity Shelf Water (HSSW)" with "formation of High Salinity Shelf Water (HSSW - $\sim$-1.9°C, $\sim$34.75)" in the revised manuscript.

4324/10 split sentence: "...form Antarctic Bottom Water (AABW). AABW drives the bottom . . ."

Replaced in the revised manuscript as suggested.

4324/19ff This is exactly what your model could proof if you were using a higher spatial resolution that would enable the simulation of coastal polynyas (assuming that the wind forcing includes the necessary off-shore winds).

As discussed in the main comments, while this resolution would improve the representation of polynyas, we would have struggled to carry out the necessary model runs to tune and configure the model. An adaptive grid with finer resolution along the coast would be the ideal configuration, however CICE has yet to be configured in such a way. As the polynyas were not the primary focus of this study, such avenues were not investigated.

4324/24 This should refer to Fig. 2f (winds) and add a reference to the temperature map Fig. 2a in the next line. Consider to rearrange the panels in Figure 2 so that wind becomes panel a.

While there was a reference to Fig. 2 at the end of the paragraph, we have now moved this earlier in the discussion of the revised manuscript. We have also changed the panel ordering as suggested. The figure is now referenced as:

"...the difference in the near-surface winds (see Fig. 2a)..."

4325/14 ". . . have been presented (e.g. Klinck et al., 2004)."

Agreed, we have corrected this in the revised manuscript.

4325/20ff Just recently, Assmann et al. (2013) have found that the background flow is setting the availability of heat on the shelf in form of the CDW. Please comment on this. Assmann, K.M., A. Jenkins, D.R. Shoosmith, D.P. Walker, S.S. Jacobs, and K.W. Nicholls, (2013), Variability of Circumpolar Deep Water Transport onto the Amundsen Sea continental shelf through a shelf break trough , J. Geophys. Res., accepted article, doi:10.1002/2013JC008871.

We have changed the following sentence: "In the Amundsen Sea, seasonal wind pulses drive transport of CDW into glacially-carved troughs in the continental shelf (Walker et al., 2007; Thoma et al., 2008; Wahlin et al., 2010; Arneborg et al., 2012)" to: "In the Amundsen Sea, CDW is transported onto the shelf through glacially-carved

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troughs in the continental shelf seabed (Walker et al., 2007; Thoma et al., 2008; Wahlin et al., 2010; Arneborg et al., 2012), with the CDW on-shelf flux controlled by the background ocean flow (Arneborg et al., 2012; Assmann et al., 2013) and episodic events (Thoma et al., 2008)."

4328/14 the heat flux is positive for "from surface layer to mixed layer"

Agreed, we have replaced the following:"an ocean heat flux from the mixed layer to the surface layer" with: "an ocean heat flux from the ocean surface layer to the mixed layer".

4330/5f does 50m or 100m prevent formation of a Weddell polynya? Please rewrite the sentence for clarity.

h_b^{max}=100 m prevents the Weddell polynya formation as it allows for greater dissipation within the mixed layer. To clarify this and the rest of the paragraph, we have replaced the following:

"Note that to allow for the representation of deep (several hundred meters deep) mixed layers we choose $c_B \geq \exp(-h_B^{max}/d_B)$ following Lemke et al. (1990) where we have used a higher value of h_B^{max}=100 m instead of the Lemke et al. (1990) value of 50 m, preventing a large Weddell polynya that forms every winter near the Greenwich Meridian"

with

"Note that to allow for the representation of deep (several hundred meters deep) mixed layers we place a limit on dissipation, such that $c_B \geq \exp(-h_B^{max}/d_B)$ following Lemke et al. (1990). We have used a value of h_B^{max}=100 m instead of the Lemke et al. (1990) value of 50 m to increase the potential dissipation of energy within the mixed layer. This reduces deep convection and prevents a large polynya that forms every

C3072

winter in the Weddell Sea near the Greenwich Meridian"

4330/9 I think, if your model cannot produce the polynya this is an indication for it being related to the deep ocean and deep ocean heat and circulation that your forcing does not resolve. If you find a polynya in your simulation then this is an indication for it being a product of atmospheric forcing (sea ice formation and surface circulation). Either way, it is a difficult decision to artificially suppress its evolution.

Strong mixing in the eastern Weddell Sea resulted in the entrainment of heat into the mixed layer and the formation of a sensible heat polynya. While this is a process that is possible and has indeed been observed in reality (i.e. the Weddell Polynya in the 1970s), its continuous formation in this model configuration was erroneous and somewhat problematic. While we agree suppressing its formation by tuning this parameter is somewhat artificial, we feel that through the improved fit to observations, it was a necessary action.

4330/21 Which one is the mixed layer depth you diagnose and plot (that of T or that of S, which is a 10m difference if I understand correctly)?

Yes there is strictly a 10 m difference between the two definitions. The mixed layer we use in all figures and discussions is actually based on the 'temperature' mixed layer - i.e. the distance from the base of the surface layer to the base of the mixed layer as now highlighted in the new model schematic (Fig. 4). While this is technically a width and not a depth, we feel the 10 m discrepancy is unimportant to the results and discussion.

4332/Equ.14 and 15 typo: "w" is supposed to be Greek letter omega referring to Equ. 12, I guess

Yes we wrongly use both ω and w to refer to entrainment. We choose to use w throughout and this has been replaced in the revised manuscript.

4334/1 "(ocean currents are neglected here)" Does this mean U_w=0 in Equ. 12? If so,

C3073

please state this clearly: "U_w=0". In fact, this is an item of major concern (see general comments above).

Yes this does indeed mean U_w=0. We have replaced "$\mathbf{U_w}$ is the near-surface ocean velocity (ocean currents are neglected here)" with "$\mathbf{U_w}$ is the geostrophic ocean velocity (ocean currents are neglected here meaning U_w=0)". While this is a big assumption, geostrophic ocean currents around Antarctica are so poorly understood we felt this was the best approach to take.

4336/25f nice conclusion!

We thank the reviewer for their kind words!

4337/4 Please properly introduce abbreviations/labels used in map plots, e.g. "Berkner Bank (BB)".

These were introduced in Fig. 1, however we have now also included the definitions where they first appear in the manuscript and in any subsequent figures.

4337/21 What is the salinity of LSSW in your simulation? By the way, this is a good example of where you present observed salinity but actually only compare "(de)stratification" with the model. (see general comment above)

LSSW is not often used in the literature so we have instead decided to refer to Eastern Shelf Water (ESW) which is thought to be due to a combination of mixed layer deepening (destratification) from sea ice growth and fresher glacial meltwater (Nicholls et al., 2009). There is no agreed upon definition of the salinity of ESW, however it is often taken to be waters around the freezing temperature with a salinity less than 34.5. We have therefore replaced "which is often classified as Low Salinity Shelf Water (LSSW)" with "which is often classified as Eastern Shelf Water (ESW)" in the revised manuscript to refer to the low salinity destratified waters along the Luitpold Coast.

There is no agreed upon name for the water mass that forms over the Berkner Bank which is often of a higher salinity than ESW but not high enough to be classed as

C3074

HSSW. While Winter Water is sometimes used, this would be confusing to introduce in our discussion as we mainly take Winter Water to refer to any remnant waters from mixed layer deepening.

4341/6 reading about ice export I am again worried about the neglect of advection in the mixed layer, i.e. export of fresh melt water in summer or salty water in winter. Please elaborate more why you can neglect this (either in Introduction or Model [4327/6] section)

As stated in the CICE configuration section, "The mixed layer salinity is restored back to the annual climatological (30 m) value with a period of one year, to balance any loss of salt from deep ocean relaxation." As this climatological value is higher than the very fresh summertime mixed layer salinities, this acts to increase the salinity in the mixed layer (or remove the fresher waters) throughout much of the year. During autumn/winter, the mixed layer salinity restoring will then likely act to lower the salinity (on a slow timescale) representing some small export of salty mixed layer waters off the shelf. Stronger export of the salty waters is then achieved through the restoring of the remnant mixed layers within the deep ocean grid. The above sentence has been changed in the revised manuscript to make this clearer: "The mixed layer salinity is restored back to the annual climatological (30 m) value with a period of one year. This is introduced to balance any loss of salt from deep ocean relaxation and simplistically represents the export of mixed layer waters off the shelf."

4342/23 You estimate the presence of polynyas based on an ice growth threshold, which is a nice, valid approach, I think. However, I would be interested how this compares to an ice concentration or ice thickness or thinnest ice category area fraction threshold. Your model must show some loose or thin ice in these grid cells to grow more ice than average.

We originally investigated using either thin ice or low concentration detection along the coast, however, this did not detect the coastal polynya locations as expected. We

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believe new ice grows at such a rate that the grid cells always maintain a high concentration (and thickness) of ice within the time step. These regions still grow considerably more ice than other regions due to the strong ice divergence (no ice is being imported into these grid cells from the south), meaning they still act as strong 'sea ice factories' similar to the behaviour of coastal polynyas.

4343/10ff This sentence tipped me off: You run a simple model. Why do you not run it with 5 km resolution to actually simulate polynyas?

See earlier discussion

4343/19 - 4344/12 This paragraph is messy. I can't find the respective numbers of Weddell polynya, Weddell shelf, Ross polynya, and Ross shelf ice production form your model. It seems to me that a Weddell polynya production of 258 km^3 is already 25% of the total (Weddell + Ross?) shelf production of 1020 km^3. Also, this part adds to my confusion about when you validate your model against observations and when you actually aim to provide new scientific insight.

We have reworded this section which we feel has significantly improved the clarity of the message as suggested.

4344/22 Please provide boundary lines defining your regions of all Weddell (Ross) sea incl. deep ocean, shelf only, and polynya area. Do you use the white line in Fig. 7 and 11 to limit the shelf area? What does the line show?

The all Weddell and Ross seas are defined by the southern coastline and the relative lines of latitude - Weddell (0-60W) and Ross (135W-195W). This wasn't added to the figures as the total sea area is never graphically displayed due to the focus on the shelf seas. In the revised manuscript we have replaced the following sentence: "Using this same ice growth calculation procedure, we can estimate the total ice growth of the Weddell (0-60W) and Ross (135W-195W) seas, including over the deep ocean" with "Using this same ice growth calculation procedure, we can estimate the total ice

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growth of the Weddell (0-60W) and Ross (135W-195W) seas (regions are bounded to the south by the Antarctic coastline and extend northwards wherever sea ice is grown)"

For further clarification, we have replaced the following sentence in the revised manuscript:

"This value corresponds to the lower end of the Tamura et al. (2008) and Drucker et al. (2011) scales and provides 'polynya' grid cells in reasonable locations along the Ross and Ronne ice fronts (Fig. 11)." with:"This value corresponds to the lower end of the Tamura et al. (2008) and Drucker et al. (2011) scales and provides 'polynya' grid cells in reasonable locations along the Ross and Ronne ice fronts (highlighted by the black crosses in Fig. 12)".

4345/19f remove sentence "To our knowledge . . ." I assume that if there were any, you would compare your results to them.

This sentence has been removed in the revised manuscript.

4345/20f Fig. 10c shows net export form the Bellingshausen Sea in most years. So, there's great import and melt but still net export? Is ice just passing through Bellingshausen Sea?

Yes, ice is imported in the east and exported in the west, meaning ice passes through the Bellingshausen shelf in a clockwise circulation.

4346/19f Again, increased spatial resolution would help fix this shortcoming.

See earlier discussion

4348/1 please rewrite this to stress the difference between Table 3 and Figure 12 even more: spatial correlation using time mean vs. temporal correlation using spatial mean. See 4353/20.

Agreed. We have replaced with: "To understand the link between spatial and temporal"

with "To understand the link between spatial (temporal mean - intra-regional variability) and temporal (shelf sea mean - interannual variability)" and in the conclusions we have replaced: "A linear regression analysis is performed to determine the spatial and temporal correlations" with: "A linear regression analysis is performed to determine the spatial (temporal mean - intra-regional variability) and temporal (shelf sea mean - interannual variability) correlations"

4348/22 – 4349/10 and 4350/5-24 pick highlights, don't discuss every correlation.

Agreed. We now only mention the moderate/strong correlations in the revised manuscript. This is stated at the start of the section. Weaker correlations are still shown in the Table/Figure.

4351/12f remove sentence "Figure 13 shows . . ."

We have removed this from the revised manuscript.

4351/18 – 4352/5 This paragraph uses a lot of language that expresses speculation, such as "are thought to", "will result", "potentially increases", "probably plays". You are running a relatively simple model, i.e. you should be able to completely understand the processes or consider removing this speculative paragraph.

We have removed overly speculative statements and have attempted to make this paragraph clearer in the revised manuscript. The impact of winds on ice growth/export is a complex process due to dynamic/thermodynamic feedbacks in the sea ice behaviour (note that while the ocean model is simple, the sea ice model is not). We have kept in some statements that imply uncertainty in our understanding, as they highlight interesting and potentially important climatic processes which we feel require considerably more research.

4352/13 Does your model produce HSSW (T and S properties) where the water column is destratified, i.e. where there is a deep mixed layer?

As stated in this paragraph of the conclusion, destratification does not necessarily lead

to HSSW formation. We state it only leads to HSSW formation "along the southern coastal ice fronts in both seas". Regions of destratification over the Berkner Bank "do not lead to HSSW formation". We feel this is a clear enough distinction already contained in the manuscript. We have also introduced a map of bottom temperature as discussed in the main comments.

4352/19ff This is the major contribution of your study to new scientific understanding. I strongly recommend to stress this more and shorten everything else.

Agreed. We have replaced: "By deconstructing the surface power input to the mixed layer, we find that the net salt flux from sea ice growth/melt dominates the evolution of the mixed layer in all regions, with a smaller contribution from the surface heat flux." with: "By deconstructing the surface processes driving the mixed layer depth evolution (surface-mixed layer mechanical power input), we find that the net salt flux from sea ice growth/melt dominates the evolution of the mixed layer in all regions, with a smaller contribution from the surface heat flux." and placed this in its own paragraph within the conclusions.

Table 1 Please use a separate column for the values of the constants.

We have changed this in the revised manuscript

Table 3 "P" could easily be misunderstood as power. Maybe using "$F_{rain} + F_{snow}$" would be more clear (see Fig. 3)

Agreed. We have replaced this in the manuscript and added the following to the caption for clarification: "Note that $F_{rain} + F_{snow} = P$ as in Fig. 2f."

Figure 1 I really like this figure as a starter. Nevertheless, I'd like to suggest splitting it in two, i.e. make panels b and c a separate figure, and move the inlays in a outside the plot.

We looked into doing this but the bathymetry plot looked sparse when we moved the inlays outside of the plot. We still wanted to focus on the bathymetry between 0 and

1000 m so much of the figure was just yellow (>1000 m). Based on this, we believe it is best to keep the figure how it is.

Figure 2 Mention what "(JAS)" means, i.e. "(July-September, JAS)"

Agreed, we have replaced (JAS) with (July-September, JAS) in the revised manuscript.

Figure 3 Please add much more information to the caption: A=ice concentration, F=fluxes, S=salinity, T=temperature, . . .; what are black and red arrows, red=heat flux? What is F^o; add h_s=10m and h_{mix} on the right side of the panel with a bracket spanning the respective part of the water column.

Agreed, we have added more information to the caption as suggested in the revised manuscript.

Figure 6 The color scale ends at 2 m; it is not clear by how much your model actually overestimates ice thickness since maximum color is reached in many places along the coast.

We have extended the colour scale in the figure to 3 m in the revised manuscript. This does help highlight that much of the thick ice was around 2 m. There are still small localised regions that see a thickness up to 3 m. While this thick ice is not apparent in the satellite estimates, the large uncertainties involved with the approach of estimating thickness using the assumption the snow-ice interface is at sea-level makes it unclear how reliable the satellite estimates are, especially where we might expect thick ice build up along the coast (due to northerly winds).

Figure 7 Please add explanation for black crosses and white outline to caption.

Agreed. The following has been added to the revised manuscript: "The black crosses highlight grid cells where the maximum mixed layer depth is greater than 90% of the water column depth. The white line is the 1000 m isobath contour taken from the RTOPO dataset (Timmermann et al. 2010)."

Figure 8 List all abbreviations used in the maps in the caption. There are many grid cells where mixed layer depth exceeds 90% of the water column (black cross) but salinity didn't change from deep ocean salinity boundary condition. This needs to be better discussed in main text.

Agreed. We have added the following to the caption of Fig.8: "BB: Berkner Bank, FD: Filchner Depression, FIS: Filchner Ice Shelf, BIS: Brunt Ice Shelf, LC: Luitpold Coast, RI: Ross Island, M: McMurdo Sound, TNB: Terra Nova Bay.". See main comment earlier for more discussion surrounding this figure.

Figure 9 Panels a-d should be climatology just like panel e. You focus on seasonal variations not inter-annual ones – which is fine but I think a showing a climatology would be a better match. You can mention in main text that you don't find any trends. Consider making panel f a separate figure , it's hard too read. You could also add a panel in which P_{net} is scaled to 1 so one can see the partitioning for the Bellingshausen and Amundsen seas better.

Agreed. We have changed Fig. 9 to a climatology of the annual power input (1985-2011) in the revised manuscript (now Fig. 11). As panel (e) highlights the regional differences in the surface power input, we have changed the scale on the AB plots ((b) and (c)) to make the partitioning of surface processes clearer. We don't therefore feel the need to create another panel in which P_{net} is scaled. We have made panel (f) a separate figure as suggested (now Fig. 12). We have also added the following to the end of the power input description to explain this energy input term: " In later sections, the annual time integrals of these power input terms are used to analyse the net annual (mechanical) energy input to the mixed layer, referred to as W_{salt}, W_{pe}, W_{heat}, W_{wind}, W_{net} (not density weighted, so in units of J m^{-2})."

Fig. 10 Again, the time series is not discussed, focus on climatology. You could indicate interannual variability by adding a shading around climatological curves, e.g. 1 standard deviation.

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We believe this figure (now Fig. 13) benefits from showing the interannual variability more so than the previous power input plot as this shows annual values as opposed to a seasonal cycle. The alternative would be to show bars which would still require a whole figure and we don't believe would make the information clearer. We have introduced shading to highlight interannual variability in the previous figure of power input (now Fig. 11) following this suggesting however.

Figure 11 What does the white outline indicate?

The following has been added to the caption of Fig. 11 in the revised manuscript: "The white line is the 1000 m isobath contour taken from the RTOPO dataset (Timmermann et al. (2010)."

Figure 13 I didn't understand the discussion associated with this figure very well. It seemed pretty speculative to me. I suggest removing this part for conciseness and focus on your main story (Fig. 9).

We agree some of the discussion surrounding this figure is hard to follow. However, we believe the issues raised by the lack of correlation between the Ross shelf mixed layer deepening and meridional winds is important and worthy of further discussion/research. We have therefore decided to keep this figure and its discussion in the revised manuscript.

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