

Interactive comment on “Brief Communication: Evidence of a developing Polynya off Commonwealth Bay, East Antarctica, triggered by grounding of iceberg” by C. J. Fogwill et al.

Anonymous Referee #1

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General comments:

“Evidence of a developing polynya off Commonwealth Bay, East Antarctica, triggered by grounding of iceberg B09B” is a manuscript written with the aim of documenting recent oceanographic observations in a new polynya occurring in the lee of B09B, an iceberg which re-grounded in Commonwealth Bay following the dramatic change in regional icescape in early 2010 associated with the calving of the Mertz Glacier Tongue. These oceanographic observations are supported to some extent by a high-resolution ocean model, of which little description is provided. The text of the manuscript is quite well written, in contrast to the very poor quality of the figures presented.

In summary, it's a manuscript which I would very much enjoy reading if I could a)

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read the figures, and b) be convinced that the heat/salt flux used in the pre-calving configuration was appropriate (see details below).

There are some major issues with this manuscript. These are fleshed out in more detail in my specific comments, but I will list those comments that I view as “major” here:

Specific comment 12: the use of 2009 heat/salt flux data for the “pre-calving” model forcing.

Specific comment 13: the description of the model is woefully inadequate.

Specific comment 16: the model results appear to contradict the observations.

Specific comment 17: the quality of both figures is very poor.

Specific comment 21: The shape of iceberg B09B does not appear to be realistic in the model domain.

Specific comments:

1 The title is somewhat misleading. A polynya is defined as a region of lower sea ice concentration than what would otherwise be expected given the climate of that region. We can easily see that this is a new polynya from satellite imagery/other data. What this paper presents is the oceanographic consequences of this new polynya. As such, I suggest changing the title to reflect this focus.

2 I have an issue with the recurring reference to the “grounding of B09B”. B09B has significantly grounded twice (at least!) since its calving from the Ross Ice Shelf in 1987. As such, reference to the “grounding B09B” is ambiguous. Perhaps change to “recent re-grounding” or similar.

3 Line 1: “triggered by the impact” – this hasn’t been proven. There is an argument that the calving of the MGT was precipitated by the movement of B09B which altered the current configuration, thus calving without an impact. This was outlined in detail by Mayet et al., 2013 (JGR Oceans). This important work on the MGT calving is not cited

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once in this manuscript.

4 P2, L5, other places: The point is made that local changes in the icescape can influence AABW formation, but this ignores the fact that B09B was produced thousands of km away (i.e., not only local, but remote icescape changes can have large impacts).

5 P2, L11-13: There is more to the existence of the CB polynya than just these factors, i.e., the presence of the MGT, B09B, many smaller grounded icebergs around which fast ice forms, all located upstream of the CB polynya.

6 P2, L13-15: The text before and after the semicolon is a non sequitur. Also “the sea ice” is ambiguous.

7 P3, L1-2: As with P2, L5: There is more to the westward flow blocking than just the MGT. See the description provided by Massom et al., 2003 (JGR).

8 P3, L11-13: This sentence could do with a re-write. Also, mention that while the MGT calving cycle may be cyclic, the re-grounding of B09B in CB is likely to be a “spanner in the works”.

9 P3, L16: For completeness, you need to mention the contribution of Cape Darnley polynya to AABW – see paper by Ohshima et al., 2013 (Nature Geo).

10 P4, section 2.1: There is absolutely no mention of which month observations were conducted. This seriously adds to confusion in the interpretation of figures.

11 P4, L24: Tamura et al.’s heat and salt flux data are based on thin ice thickness data, not sea ice concentration data.

12 P5, L1: I see a major issue with forcing the model using 2009’s heat and salt flux data. As shown in Tamura and Williams et al. (2012), 2009 was a strongly anomalous year for sea ice production in the Mertz Glacier polynya. In fact, assuming you didn’t use the MODIS fast ice mask (which there is no mention of in this manuscript), 2009 had the highest sea ice production of all years observed. The use of 2009 may

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have been able to be justified if the pre-calving observations were also conducted in 2009, but they weren't (they were conducted in 2008). While not explicitly stated, it appears that the authors have simply chosen 2009 because it was the year before calving occurred. This appears to be a very poor choice, and has probably influenced the conclusions drawn from the modeling component here. Unless I'm missing something here, I think it would have been much more sensible to force the pre-calving model run using either a more normal year for sea ice production, or a sea ice production climatology based on all of Tamura's years of observation. The choice of 2012 for post-calving seems fine, however. Perhaps the choice of 2009 could be justified by performing a sensitivity analysis? I'm not sure how a sensitivity analysis could be done without rerunning the whole model though.

13 Section 2.2: The description of the model setup/domain is completely inadequate. What is the resolution? Latitudinal and longitudinal extents? Grid setup? Bathymetry used? What hope does someone have of reproducing your results without this fundamental information? Or are you tasking the text "similar to Cougnon et al., 2013" with providing this information? How similar, exactly? In this case, you need to be more explicit here. And how was the fast ice treated in the model? What was its horizontal extent? Tamura and Williams et al. (2012) highlighted the importance of using accurate fast ice in polynya studies, but the fast ice implementation in the model is not even mentioned here. Was the "dagger" fast ice forming around grounded icebergs to the north of the pre-calving MGT included? This acts to extend the MG polynya (both pre- and post-calving). So many unanswered questions related to the model domain.

14 P5, L18: "piled up" is a very vague statement, it's possible to be much more exacting. As showed by Massom et al. (2010, JGR Oceans), the very thick fast ice immediately east of the MGT was thermodynamically thickened, and not "piled up" at all. Or are you referring to the largely dynamically-thickened fast ice (which probably was "piled up") east of the pre-2010 grounded position of B09B (see Fraser et al., 2012, Journal of Climate)? I'm not sure which you're referring to, because you state "piled up

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to the east of the glacier tongue”. Do you mean immediately east? Or farther afield?

15 P5, L20-21: Figure 1D shows very similar salinities in 2008 vs 2011 though.

16 P6, L7 vs L21. The comment is made in L7 that the post-grounding water column is saltier than pre-calving, based on observations. However, in L21 you say that the post-grounding water column is fresher, based on model results. This is also manifested in Fig 1A vs Fig 2C. No mention of this discrepancy is made in the text of this paper. It seems like a major failure of the model to reproduce the observations. Could you make a comment about this?

17 P7, L22: The blocking of the coastal current is a major result of the model, yet its importance is not emphasized anywhere in the discussion. Here might be a good place to include it.

18 P8, L2: How does the sea ice production compare between your pre and post-calving years?

19 P8, L10: Is HSSW formed in this region able to go on to form AABW? A comment on the bathymetry in the region of the new polynya would be appropriate here.

20 Figure 1 is very poorly presented. There are numerous typos (Decmebr and Tounge). The font size varies wildly across the figure, much of the text is illegible. Both inset maps for Fig 1A are almost useless. The lower left one really suffers from not having a coastline drawn. Fig 1A needs much more annotation. What is continent? What is fast ice? What is pack ice? How does the date of acquisition of this image relate to the time of field observations? Fig 1A should be zoomed out a little to provide more context – we can't even see the “original” B09B grounding location or the full extent of the tongue. There is absolutely no representation of the icescape pre-calving! The caption is confusing in the way that it references the sub-figures (and doesn't even mention sub-figures E, F or G). The figure refers to both B9B and B09B. The color “blue” is given a capital letter in the caption for some reason (and “red” doesn't even

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rate a mention).

21 Figure 2 is very poorly presented. Summer and winter figures seem randomly placed. Wouldn't it be a good idea to arrange all "winter" figures on the left, and all "summer" figures on the right? And why does "Nov-Dec" appear before "Aug-Sep"? It's chronologically backward. Fig 2B has no label on the legend. This figure is completely illegible in print, and only slightly better online. There's a fundamental problem with the presentation of Figures 2A and 2B: since the pre-calving vectors are directly over-plotted on the post-calving vectors, and there's no translucency, then it's impossible to assess if the underlying vector if the overlying vector completely obscures it. It's a terribly unreadable way to present two vector fields. At the very least, one series of vectors should be offset slightly. Possibly most importantly, the outline of B09B appears to bear little resemblance to the shape of that in Fig 1. Why is the eastern end of B09B not tapered in the model domain? B09B is referred to as both "B09B" and "B09b" in the caption. Finally, the caption could use some revisions, English-wise – some strange sentences as well as some parenthesis nastiness.

22 Figure S1 adds very little to this manuscript. It would be sufficient to say that the xctd matches the microcat values very closely (possibly give an RMS difference, or similar measure of agreement).

Technical corrections:

1 Be consistent with capitalization of T in Mertz Glacier Tongue.

2 Strange bracket situation on P2, L21. Strange space situation there too.

3 P3, L18: Replace "are" with "is".

4 P4, L22: Double reference to Cougnon et al., 2013.

5 Be consistent with "fast-ice" vs "fast ice".

6 P3, L5: check capitalization for the C, C and O parts of ECCO.

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7 P6, L1: Colder than...?

8 P6, L8: Replace “occurred” with “now occurs”.

9 P6, L18: Location of Adelie Depression needs to be shown in a figure.

10 P6, L5: I don't like the use of “present position” for B09B. What if it moves? Better to tie the description to a year or epoch.

11 P8, L15: Counter parts should be counterparts.

Again, I'd like to reinforce that I think this kind of experiment is very interesting, and would like to see a revised version which can convince me that the heat/salt flux forcing (pre-calving) is appropriate (a reference to specific comment number 12). I hope the authors can take the time to convince me, or to run the model again with more appropriate heat/salt flux forcing.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-19, 2016.

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