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Interactive comment on "Atmospheric forcing of sea ice anomalies in the Ross Sea Polynya region" *by* Ethan R. Dale et al.

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We would like to thank the reviewer for providing useful comments on the text. These have allowed us to strengthen our manuscript and increase the clarity of our discussions. Response to specific comments follow, with reviewers comments quoted in bold.

1. An explicit discussion should be provided as to how the wind and temperature observations at Laurie AWS are expected to differ from those over the Ross Sea polynya to the north. This should be based on topography, roughness length, and stability differences.

Due to lack of weather data available within the RSP, AWS data from sites on the nearby

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Ross Ice Shelf was assumed to be representative of that over the RSP. A multitude of effects will cause the actual winds and temperatures over the RSP differ somewhat from that measured at the Laurie II AWS site, these effects will be inhomogeneous across the RSP. Proximity to topography, particularly Ross Island differs between the RSP and Laurie II. The majority of the RSP is more distant to Ross Island than Laurie II, but some areas of the polynya will be closer as it reaches the northern coastline of Ross Island. Southerly katabatic drainage flows will accelerate around Ross Island, so the net result of Ross Island on these winds at Laurie II will be to cause greater wind speeds. This effect will be present in some areas of the RSP but others will be too distant for this to be significant. Areas of the RSP north of Ross Island will be somewhat sheltered from many of the predominant southerly winds. The surface roughness of the Ross ice shelf, beneath Laurie II AWS will differ to that within the RSP. This will depend on the current state of the RSP. This difference will cause the winds within the boundary layer to differ between these locations. Due to the relatively warm ocean an upward heat flux will occur within the RSP when open water or thin ice is present. This will cause an increase in surface air temperatures over the RSP. This effect will not occur at Laurie II due to the insulation of the thick ice shelf. Due to the lack of measurements within the RSP the magnitude of these effects is unable to be identified.

A shortened summary of this has been added to the discussion:

"Due to lack of weather data available within the RSP, AWS data from sites on the nearby Ross Ice Shelf was assumed to be representative of that over the RSP. A multitude of effects will cause the actual winds and temperatures over the RSP differ somewhat from that measured at the Laurie II AWS site, these effects will be inhomogeneous across the RSP. Proximity to topography, particularly Ross Island differs between the RSP and Laurie II. Southerly katabatic drainage flows will accelerate around Ross Island, causing stronger winds to be observed within the RSP. Meanwhile other areas of the RSP north of Ross Island will be somewhat sheltered from many of

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the predominant southerly winds. Due to the relatively warm ocean an upward heat flux will occur within the RSP when open water or thin ice is present. This will cause an increase in surface air temperatures over the RSP. This effect will not occur at Laurie II due to the insulation of the thick ice shelf. Due to the lack of measurements within the RSP the net result of these effects is unable to be identified."

2. At several locations in the manuscript (e.g., pages 2 and 5) reference is made to the unavailable Jolly et al. (2015) manuscript to claim that the Antarctic Mesoscale Prediction System is unable to resolve the main topographically forced winds of relevance to the behavior of the Ross Sea polynya, therefore implicitly justifying the use of ERA-Interim instead. Given model grid spacings for the Ross Island region as fine as 1.1 km and the generally favorable AMPS validation studies (e.g., Bromwich et al. 2005, Monthly Weather Review), this claim is quite surprising and requires expanded discussion and substantiation.

This paper is now available at *http://journals.ametsoc.org/doi/abs/10.1175/MWR-D-15-0447.1*. We did not use ERA over AMPS because of the issues highlighted in Jolly, but rather because of the temporal consistency of the ERA dataset. AMPS is not a consistent product, since it is operational, making it difficult to use for a longer term climatological study. AMPS also began in 2002 which would cause issues with its usage in this application. A sentence justifying the use of ERA over AMPS has been added:

"Although AMPS provides higher resolution weather data, ERA-Interim was used due to its temporal consistency and data set spanning the AWS period."

3. Given the rather similar analysis by Bromwich et al. (1998), much more consideration and contrasting its results with those here is required.

We feel that our results add to the Bromwich et al. (1998) work significantly, for example the usage of lags to probe physical processes. However, we have added the following material comparing this work with those previous results.

"Bromwich et al. (1998) found annual correlations between SIC in the RSP and wind speed at Ferrell AWS for 1988-1991 ranging from -0.3 to -0.52. We find the multi-year correlation for SIC in the RSP and wind speed at Ferrell AWS from 2001 to 2014 to be -0.67. The disagreement between these values is to be expected as Bromwich et al. (1998) uses a RSP area that extends significantly further from the shore than the one used in our analysis. Winds over their RSP area are not as well represented by the Ferrell AWS as the area used within our analysis justifying the weaker correlation observed. We find a minimum correlation between Ferrell wind speed and SIC to be -0.72 at 10 hours delay, Bromwich et al. (1998) did not calculate correlations at varying delay so comparison with this value is not possible. Bromwich et al. (1998) also find correlations between SIC and inverse temperature ranging between 0.44 and 0.55. We found a SIC, inverse temperature correlation of 0.639 this difference is due to the different RSP areas used."

4. In the conclusions section, a major effort should be made to compare and contrast the present findings with those from earlier work, i.e., to address the issue of "what new aspects have been revealed by the present effort"?

This is covered by point 3.

5. How is the standard deviation of SIC shown in Fig. 1 actually calculated? Is this based on the daily variation from the interannual mean of SIC for the same day? Why start on April 20 rather than April1?

The standard deviations are the daily variations from interanual mean SIC for the entire winter (in this case 20 April-1 November). The 20th of April was used as the start of the winter here as winter sea ice had not formed in the northern areas of the map until this time. Using the first of April results in high standard deviation values in northern areas of the map. These high standard deviations are due to the seasonal cycle not polynya activity so the beginning of April was removed to make the result clearer for the reader. The first of April was used elsewhere to maximise the size of the data set as all other analysis was preformed near the edge of the ice shelf, where sea ice forms early in the season. The definition of the standard deviation has been made clearer and an explanation of the varying time periods used has been added. The beginning of paragraph 2 of data and methods section now reads;

"Fig. 1 shows the standard deviation of SIC. This is defined as the daily variation from the inter-annual winter mean of Bootstrap SIC over the period, 20th April until the 1st of November for years 1979 until 2014. This period was chosen to exclude the annual break out of sea ice which would add variability not associated with day to day polynya activity"

6. Reference is made to the "winter" being April – November. It is more accurate to give the period as April-October.

Agreed, this error has been corrected.

7. It would be helpful to keep reminding the reader why you are using the 2001-2014 period, probably because of Laurie AWS data availability.

Your assumption is correct a reminder has been added at Page 9 Line 5.

8. Fig. 1: An inset map is needed showing the Ross Island topography and the locations of Laurie, Ferrell, and Emilia AWS.

Room for a inset within this figure is scarce, and the figure already contains a lot of information. The co-author group did not feel that a standalone figure was justified.

9. Fig. 2: Why is the maximum sea ice area exceeded in 2013?

This appears to be a graphical error due to the thickness of the line, the centre of the line never crosses 100%. The line thickness's have been modified to minimise this effect

10. Fig. 3: Specify how positive and negative delays are defined, i.e., which variable leads?

Added definition of delay to caption;

"The delay is defined such that positive indicates meteorology measures leading SIC.".

11. Fig. 5: I didn't understand the construction and meaning attached to the histograms.

Added line in text to clarify histograms;

"Histograms are also shown to indicate how the distribution of each wind class changes throughout the period examined. On day 0 all cases are either 100% high

or low winds, but on following days the winds are not classified, this allows the persistence of these wind events to be observed."

12. Page 8, lines 5-7: This sentence needs to be more carefully formulated for accuracy and understandability.

Text was changed;

"All cases show negative correlations between SIC and wind speed. With the exception of a short period in the medium case, spanning -30 hours to 6 hours when weak positive correlations are observed."

13. Page 9, lines 14-16: How do incoherent motions demonstrate the critical influence of surface winds on sea ice motion?

This has been changed to:

"It is also noticeable that no coherent pattern in the sea ice anomalies associated with the medium wind state are observed (not shown). The cyclonic anomalies during strong wind events and anticyclonic anomalies during low wind events highlight the critical influence of atmospheric near-surface winds on sea ice motion in the region."

14. Page 11, lines 32-33: This sentence is backwards – high winds are associated with cyclonic motion anomalies.

This error has been corrected.

Technical Issues: 1. Page 4, line 10: “Margaret” – typo. 2. Page 5, line 2: “water vapor”? Line 30: “System” - typo. 3. Page 6, lines 29-32: Rewrite this sentence for clarity. 4. Fig. 5 caption: “indicate” – typo.

These errors have been corrected.

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