The Cryosphere

Discussions

EGU

Open Access

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

Anonymous Referee #2

Received and published: 2 July 2016

General Comments:

This is a comprehensive analysis of the "winter" sea ice concentration behavior in the Ross Sea polynya in relation to the atmospheric forcing. The near-surface atmospheric conditions affecting the polynya are approximated by observations of 3-m winds and temperatures from the Laurie II AWS, just to the east of Ross Island and adjacent to the northern edge of the Ross Ice Shelf. The results are very consistent with earlier work showing the major impact of strong wind events. In fact, this analysis has a direct predecessor in work of Bromwich et al. (1998) who performed a limited investigation of the Ross Sea polynya variations in relation to winds and temperatures from the Ferrell AWS, yet this paper is barely mentioned. A comparison with the ERA-Interim global reanalysis fields is also included, and shows many similarities with the AWS-based analysis. There are a number of issues that should be addressed before publication.

Specific Issues:

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. 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. 3. Given the rather similar analysis by Bromwich et al. (1998), much more consideration and contrasting its results with those here is required. 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"? 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? 6. Reference is made to the "winter" being April – November. It is more accurate to give the period as April-October. 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. 8. Fig. 1: An inset map is needed showing the Ross Island topography and the locations of Laurie, Ferrell, and Emilia AWS. 9. Fig. 2: Why is the maximum sea ice area exceeded in 2013? 10. Fig. 3: Specify how positive and negative delays are defined, i.e., which variable leads? 11. Fig. 5: I didn't understand the construction and meaning attached to the histograms. 12. Page 8, lines 5-7: This sentence needs to be more carefully formulated for accuracy and understandability. 13. Page 9, lines 14-16: How do incoherent motions demonstrate the critical influence of surface winds on sea ice motion? 14. Page 11, lines 32-33: This sentence is backwards – high winds are

associated with cyclonic motion anomalies.

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.