The Cryosphere Discussions

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Comment on tc-2020-352

Alexander Fraser (Referee)

Referee comment on "Brief communication: The anomalous winter 2019 sea-ice conditions in McMurdo Sound, Antarctica" by Greg H. Leonard et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-352-RC1, 2021

A review of The Cryosphere Brief Communication manuscript tc-2020-352 by Greg Leonard et al.

This is a well-written and important study suitable for publication in TC after minor changes. Fast ice is a missing piece of the puzzle in regional (and indeed global) climate models, and understanding its stability is an important part of its model implementation. My only "major" suggestion is really quite minor.

Major suggestion:

The authors present a convincing correlation between storm events and fast ice breakout, indicating that it's probably a direct wind-driven (i.e., dynamical) breakout mechanism (and I agree that this is almost certainly the case) - however no alternative mechanisms are discussed. Other studies have indicated that fast ice may be weakened thermodynamically by basal melt (e.g., Arndt et al., 2020, also TC - however this study implied that summertime mode 3 water incursions were important, which is surely not a factor in the winter). I'm not so familiar with the structure of the water column in the Sound during winter, but is it possible that these wind and polynya events enhance vertical mixing - and if warm water (e.g., mCDW) exists on the shelf here, might its entrainment induce basal fast ice melt? And the lag apparent between some storm events and the breakout might also imply a thermodynamic connection (although I accept your explanation involving the land mask of the sea ice concentration data in probably correct). Looking at Pritchard et al 2012 ("Antarctic ice-sheet loss driven by basal melting of ice shelves"), I can see that there's likely no warm water here so my hypothesis is quite unconvincing - but a brief discussion around alternative mechanisms would be appreciated!

Minor suggestions (line numbers given where appropriate):

6: add “timing of” between “between” and “break-out”

14: Brett et al ref needs year.

18: add “stable” between “the” and “fast”

32: “activity” here is a little ambiguous. You mean sea ice production, right?

35: Probably best to avoid starting a sentence with a number (2019).

41: The Fraser et al 2020 dataset gives 15 day composite maps, not 14 day.

47: The “biased” in here implies that these studies didn’t correctly account for the icescape change. Is this what you really mean - if so, for both studies?

58: Was this IW mode Sentinel-1 imagery? What resolution?. Also Hall and Riggs refs need years.

59: “MSP event” is a little ambiguous. Do you mean a large polynya size event? Also here, I’m curious how an active polynya looks in ice surface temperature - presumably a warm temperature? Or is it masked because largely open water?

65: “Manually identified events” is a little ambiguous. Events of what?

74: “connected to” -> “associated with”

75: “warm temperatures” - what temperature? I presume near-surface air temp?

79: “are correlated to sea ice concentration” - ambiguous description. High SIC? Low SIC? And isn’t “correlated with” better than “correlated to”?

82: By “freeze-up” do you mean pack or fast ice?

85: It first struck me as a little unusual to define a KWI without using wind data. What happens if a low pressure system occurs over the central Ross Sea - doesn’t this also bring warm air and low pressure? Or is this the effect you’re trying to capture - and these pressure systems enhance the katabatics? A little more clarity here would be appreciated.

91: “break-out events” - do you mean fast or pack?

117: Although a brief communication, the “big picture” could do with a little more expansion. E.g., this is one of few case studies on fast ice stability, an area where more research is needed, etc. It occurs to me that this region might be a good one for testing forthcoming fast ice tensile strength parameterisations in prognostic fast ice models (e.g., Lemieux et al., 2016, “Improving the simulation of landfast ice by combining tensile strength and a parameterization for grounded ridges”). Also, are there other regions you know of which have a similar fast ice regime (i.e., deep embayment and lack of grounded icebergs) to which the results of this study might be applicable?

119: The Fraser et al., 2020 dataset is missing from the availability section.

Fig 1: It would be helpful to please annotate the area of active polynya in each SAR image (manually is fine). Similarly for the fast ice edge.

Fig 2: Does the truncation of the upper half of each wind rose remove any/much information? I’d quite like to see the whole thing (if there’s detail in the northerly half) but happy to stick with the half roses if no wind from that half.

Fig 3: A little unusual to not have a colour legend for the upper two plots, although I recognise that they’re only shown to indicate the envelope of previous years (and the reader doesn’t necessarily need to know which year is which). Caption: identify -> identifies. Also is “break-out” referring to fast or pack?