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Interactive comment on "Warm Winter, Thin Ice?" *by* Julienne Stroeve et al.

Anonymous Referee #1

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Warm Winter, Thin Ice by Stroeve and others

Summary: Stroeve and others investigate the impact of 2016/2017 anomalously warm winter on sea ice thickness using the CICE model and CS2 thickness observations. A secondary objective of the study is to compare three difference approaches of ice thickness retrievals from CS2 to CICE. The authors demonstrate that recent warm fall temperatures (i.e. since 2012) impact winter sea ice thickness by reducing wintertime growth which was particularly strong in 2016/2017. Overall, I think this manuscript can find a place in the literature when the author's address my major concern that thinning in 2016/2017 especially, north of Greenland and the Canadian Archipelago was not entirely driven by thermodynamics (i.e. positive snow depth anomalies) but rather reduced ice convergence.

Major comment: The authors have not made a convincing argument that snow depth is

the primary mechanism for reduced ice thickness north of Greenland and the Canadian Archipelago in April 2017. While I agree snow depth is the major source of uncertainty in CS2 retrievals, ice dynamics during the winter of 2017 in this region was likely more influential and should be discussed. The authors suggest the positive ice thickness anomaly in November 2016 north of Greenland and the Canadian Archipelago did not persist because of snow loading and in turn reduced thermodynamic growth but ice dynamics (i.e. lack of ice convergence) is more likely the culprit here. Indeed, the fall of 2016 was the warmest on record and these temperature anomalies persisted into 2017, thinning ice in some regions (Barents Sea) but this thinning also manifested enhanced surface heating changing atmospheric circulation over the Arctic and especially over the Beaufort Sea. Consequently, the Beaufort High collapsed in the winter of 2017 and this reduced ice convergence against the northern Canadian Archipelago and Greenland which is clearly apparent from the sea ice motion vectors in Figure 8 of the author's paper. The latter process seems to be more likely the cause of why the November ice thickness anomaly in this region was not preserved as atmospheric circulation prevented dynamic ice growth (convergence) which typically dominates during the winter in this region. I think the authors should acknowledge that ice thinning in the Arctic is not entirely thermodynamically driven and ice dynamics also play a role which is underscored by Kwok, 2015, GRL.

A second related point is that multi-year ice is the dominant ice type north of Greenland and the Canadian Archipelago which has consistently been preserved despite the shift from multi-year ice to first-year ice elsewhere in the Arctic. This suggests that the snow depth here should be somewhat similar to the Warren Climatology. This was actually reported to be the case based on recent measurements from Haas et al., 2017, GRL and hence CS2 estimates in this thick MYI region should be reliable. The latter point also lends further support to reduced ice convergence being more influential on thinning than thermodynamics.

Specific Comments Line 286-288 Ok, but there appears to be a mix of positive and negative anomalies. The most prominent feature worth mentioning is the CS2 strongest thinning anomalies are along the northern coast of the Canadian Archipelago.

Line 297-299 I'm not convinced that the snow loading in CS2 has caused this difference in April 2017 north of the Canadian Archipelago and Greenland. If I recall, the Beaufort High collapsed in the winter of 2017 and this reduced convergence against the northern Canadian Archipelago and Greenland which appears to the case in Figure 8. The latter seems more likely the cause of why the thickness anomaly in this region was not preserved as atmospheric circulation prevent dynamic ice growth. This seems to be captured across all CS2 products but not CICE-ini. This needs revision. See major comment.

Line 413-415 The snow is important but ice thickness is strongly influence by dynamics (i.e. convergence against the Canadian Archipelago and Greenland) and this needs to be mentioned in the discussion as well. See Kwok, 2015, GRL. Furthermore, MYI is the dominant ice type north of Greenland and the Canadian Archipelago which has consistently been preserved despite the shift from MYI to FYI elsewhere. This suggests the snow depth here should be similar to the W99 which was found reported by Haas et al., 2017, GRL hence CS2 estimates here should be reliable and lends further support to reduced ice convergence was more influential on thinning. See major comment.

Table 1 What is the source of the data in this table? The passive microwave algorithm from Markus et al., 2009, JGR?

References: Haas, C., Beckers, J., King, J., Silis, A., Stroeve, J., Wilkinson, J., Notenboom, B., Schweiger, A., & Hendricks, S. (2017). Ice and snow thickness variability and change in the high Arctic Ocean observed by in situ measurements. Geophysical Research Letters, 44, 10,462–10,469. https://doi.org/10.1002/2017GL075434

Kwok, R. (2015), Sea ice convergence along the Arctic coasts of Greenland and the Canadian Arctic Archipelago: Variability and extremes (1992–2014), Geophys. Res. Lett., 42, 7598–7605, doi:10.1002/2015GL065462.

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