Landfast Ice Growth and Displacement in the Mackenzie Delta Observed by 3D Time-Series SAR Speckle Offset Tracking

By B.-H. Choe and others

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Review

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Summary

This manuscript is nearly identical to a manuscript by the same authors that I recently reviewed for Geophysical Research Letters. Unfortunately I see that my fundamental concerns regarding the feasibility of measuring ice growth with this technique have not been addressed and the authors have not considered the alternative and simpler mechanism that I proposed to explain the apparent vertical motion of the scattering surface of the ice. As a result, my review remains substantially similar to the review I provided when this manuscript was submitted to GRL.

The manuscript presents an interesting analysis of SAR speckle offset tracking applied to the measurements of horizontal ice motion and ice growth in landfast ice near the Mackenzie Delta. By combining results from imagery acquired from same-day ascending and descending orbits, the authors are able to estimate 3D motion of the surface from which radar energy is backscattered. In the case of the relatively low-salinity ice found in parts of the Mackenzie Delta, the scattering interface is assumed to be the ice-water interface and thus the observed vertical motion of the surface is interpreted as ice growth. This finding is supported by results from a 1D ice growth model which is in agreement with observed downward motion of the scattering surface. Elsewhere, positive vertical motion is assumed to indicate pressure ridging. Significant horizontal motion is also observed, which is attributed to wind, currents and river discharge.

The authors have chosen a complex region of the Arctic for their study, where atmospheric, cryospheric, marine and terrestrial processes interact. Unfortunately, the text suggests that the authors do not have a deep familiarity with the geophysics of these systems and as a result I fear they are misinterpreting their results. In particular, I am skeptical that the backscatter in the regions where the ice exhibits downward motion is coming from the bottom of the ice as the authors assume. Details are given in my major comments below, but in short, my reasons are:

i. The backscatter from the ice bottom is unlikely to remain coherent over periods of 12 days or more during growth
ii. the authors present no direct observations of the ice salinity to support the assertion that the C-band radar is penetrating to the bottom of the ice
iii. the authors overlook a much more likely mechanism for downward motion of the ice surface near a large delta during winter.

3D measurements of small-scale ice motion could be of considerable value for understanding dynamical processes in the Arctic coastal zone and I do not wish to discourage the authors from

continuing this line of research. However, if they must address my concerns below if they are going to continue to assert that their observations are related to thickening of the ice cover.

Major Comments

1. No explanation of why backscatter from ice bottom would remain coherent during growth
The speckle offset tracking technique requires that the scattering surface remains coherent between image acquisitions. However, in the case of scattering from the bottom of a growing floating ice cover, the radar is seeing an entirely new surface at each acquisition and I therefore see no reason why the speckle would remain consistent over timespans of 12 days. The authors need to provide more explanation of how the scattering characteristics of the underside of the sea ice (if indeed that is where the signal is coming from) would remain constant as new ice forms below each previously imaged surface.

2. Inadequately supported assumption that ice bottom is source of SAR backscatter
The authors assume that the SAR signal from the stabilized floating landfast ice is coming from the ice-water interface. The basis of this assumption is the low-backscatter signature of presumed bottomfast ice nearby and the presence of low-salinity ice in this region reported by MacDonald et al (1995). However, none of ice sampled by MacDonald et al was completely fresh and most contained a significant seawater fraction. Moreover, close inspection of the SAR intensity imagery (Fig 3d,e) shows linear spatial patterns typically associated with surface roughness features. It therefore seems likely that some fraction of the microwave energy returned from the ice is coming from its upper surface and volume. This has a significant bearing on the interpretation of the SPO results, but is not discussed in the manuscript.

3. Elevation increase due to ridging is unlikely to be detectable with this method
The authors attribute positive vertical ice surface motion to be the result of pressure ridging. This explanation sounds highly unlikely since any such ridging would dramatically change the surface scattering characteristics of the ice such that coherence would be lost between image acquisitions. This is similar to the problem of maintaining coherence from the ice bottom during thermodynamic growth, but a more extreme example of surface change.

4. No discussion of vertical motion due to changes in local water level
I was surprised that the authors failed to consider other sources of vertical motion, besides ice growth and ridging. A far more likely source of vertical motion is variation in local sea level due to tides, winds, currents and river discharge. The authors clearly state that the discharge from the Mackenzie River continues to decrease between January and April, but fail to consider that this might lead to a decrease in water level near the Delta. This is a far more likely explanation for the negative vertical motion at locations LFI 1-3 that does not require the radar to penetrate the ice or for speckle to remain consistent from a growing ice bottom.

5. Interpretation of river current-induced horizontal motion is unsupported
The text repeatedly makes a connection between offshore motion of the landfast ice near the Mackenzie Delta and the direction of river discharge toward the Beaufort Sea (e.g., lines 137-139; lines 155-157; and line 193). However, I find this an unlikely explanation and the only

supporting evidence the authors provide is that the discharge in winter is non-zero. If the authors wish to strengthen their claim that any offshore motion of the landfast ice caused by river outflow, they should estimate the likely basal stress on the ice and describe a likely mechanism by which this could fracture and displace the ice.