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The Cryosphere Discussions

Interactive comment on "Summer sea ice in the recent Arctic: morphological properties in the Pacific sector from the CHINARE 2010 cruise" *by* H. Xie et al.

Anonymous Referee #1

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This paper descibes a cruise in the pacific sector of the Arctic during summer 2010. The focus of the paper is on the state of the sea ice pack, and how this has changed since 2005. However there is alot of extraneous information that distracts from the central theme of the paper, and I may have misinterpreted the main point of the paper. The abstract suggests that the authors are describing the conditions of a 'new normal' state of the pacific sector ice pack, and they claim their observations demonstrate the existence of the 'new normal' sea ice state. I am concerned that this paper does not actually demonstrate that the sea ice state in 2010 was outside of natural variability. The concept of a 'new normal' is lifted from the Arctic Report Card. In my opinion complimentary observations to the Report Card survey of observational data does not

consitute confirmation that there is a 'new normal'. This term is also rather loose, and my feeling is that this is not a strong result around which to focus the paper. To clarify, what additional information does this paper provide over previous results that are mainly based on remote sensing or modeling?

That said, I believe there is value in some of the results presented. In particular the 2010 CHINAIR cruise experienced unusual ice conditions, that should be reported. The data set collected to describe these unusual conditions is comprehensive and potentially informative. More consideration could be taken to quantifying the observed differences in the 2010 ice pack to previous summers. The authors should also focus the paper on the key variables where change is observed. It would be useful to the reader to outline what is currently known about ice thickness and concentration interannual variability in the region of the CHINAIR and HOTRAX cruises. This could perhaps be acheived by focusing the introduction. The discussions about wildlife (for example) is interesting but out of place in the paper.

The authors have presented validation work for visual ship based observations of ice concentration and ice thickness. In my opinion this is the most valuable part of this paper. I would welcome an expansion of the discussion regarding these investigations, and some tightening of the authors methodology. The under-estimation of ice thickness from visual observations is a result that should be reported given the increasing use of ASPECT data to characterise sea ice thickness.

In my opinion there are two key results in this paper, and I would urge the authors to focus on those. These are: 1. Undersampling of ice thickness in visual observations. This is the first time I have seen this discussed, and your observations fit my anacdotal experience making visual ice thickness observations. These results should be dicussed in much greater detail, with consideration of whether the result might be dependent on the ship, and what the relative errors of the em-31 measurements are. The latter could be achieved through literature survey. I also feel that you should provide guidence on how to best make use of ASPECT thickness observations. Do these

capture the modal thickness of particular ice types best? What ice types do ASPECT thickness observations not capture? How should the observations be interpreted in relation to the actual ice thickness distribution (which without an em-31 is not observed fully, and perhaps even the em-31 is missing part of the thick tail of the PDF.) 2. The comparison of ASPECT data with camera for concentration and meltpond cover. The EIScam validation provides some quite reassuring (or at least consistent!) results regarding the utility of ASPECT ice concentration data. This should be highlighted and discussed in more detail.

Comments

Please explain why is melt consistent with high floe speed? Are you referring to the work of McPhee here? If so you should provide references.

In section 3.4 you talk about basal ice melt. Without putting this into the context of average melt rates under the meteorological conditions experienced, it is hard to judge if you really see an enhanced melt rate because the ice was drifting relatively fast.

Your observations of melt rates relative to crack position are rather interesting. I am intrigued if you can show the extend of lateral warming under the ice. Unfortunatly this whole section seams out of place in the paper, and might be better suited to a seperate publication. Perhaps you can expand your analysis to consider the solar energy absorped in the crack and how this relates to latent heat of basal ice melt in the vicinity of the crack.

What do you mean by 'biological richness'?

Comparison between AMSR-E and visual ice concentration could be described more quantitatively. If it is even relevent in your paper.

How did you define the threshold for each EIScam image in ice concentration and melt pond fraction analysis?

Do you account for image overlap in analysis of mean ice concentration and melt pond

cover from EISCam?

Do you think that there is a miss-match between visual and camera ice concentration due to the different spatial sampling that results in preferred ship travel through less consolidated/thinner ice? I am guessing that this is not an issue because you were in first year ice the whole time, but this issue should be addressed in the paper.

Were the differences between camera and observer consitent between observers? Do all observers consistently overestimate ice concentration? If so, this is a useful result, and should be tested on other ships (I suggest you put this in you concluding remarks if you feel additional data would be of value).

Were the drill hole thicknesses used to calibrate the em-31 data? In which case it would not be surprising that agreement was good. Did you only drill level ice, or was drill hole sampling representative of the local thickness distribution? Which method did you use to process the EM-31 raw data?

Can you compare a 12 day average melt rate to the seasonal average melt rate quoted from SHEBA? Perhaps you happened to be on the ice during weather conditions that promoted melt. You could obtain the SHEBA data and do some more detailed comparison to understand if there were any similar time periods in the SHEBA data with enhanced melt rate.

Much of section 3 (Results) reads like a detailed cruise report or blog of the ice observations. It is unclear to me how useful all this information is to the reader, and the information could perhaps be better summerised in a few maps.

I do not think a 12 day drift with speeds of average 0.2m/s can be compared to mean drift calculated over longer time periods (e.g. Spreen et al. 2011) to identify the ice drift is unusually fast. 12 days is within synoptic variability. I have personally observed ice speeds of this magnitude lasting for several days. Hence I believe your statement that this has never been observed before is unjustified.

"The smaller floe sizes, unlike those seen in other areas, e.g. Antarctic sea ice zone, may not result from wave and swell action, but instead from floe fragmentation as melt ponds have larger connections and relative sizes than previously. These more extensive melt ponds result from an increase due to the transition from multiyear to first year ice (Itoh et al., 2011)." This is interesting. Which observations back up this statement?

The comparison between AMSR-E and visual ice concentration lacks depth. The observation that AMSR-E and visual ice concentrations match in the MIZ is qualitative in nature, and based on figure 4 (a1 and a2) I am not entirely sure I agree with your finding. Perhaps what you mean is that the delination between MIZ and pack ice can be identified in the AMSR-E data, at least in 2010.

In Figure 1, do you really need to show the full cruise track from China?

Figure 4 has text that is impossibly small to read when at print size.

Interactive comment on The Cryosphere Discuss., 6, 1963, 2012.