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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 #2

Received and published: 24 June 2012

The manuscript presents sea-ice observations from the Pacific sector of the Arctic Ocean during the CHINARE expedition in July and August 2010. This data set comprises underway measurements (e.g. sea-ice thickness, video analyses, routine visual observations) and in-situ measurements during ice stations (e.g. sea-ice thickness, snow depth, floe drift). These observations are compared with similar observations during the HOTRAX expedition in summer 2005. Sea-ice concentration is also compared to AMSAR-E satellite observations.

Please find more detailed comments below my answers to the evaluation criteria.

Does the paper address relevant scientific questions within the scope of TC? Yes, phys-

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ical properties, in particular thickness and concentration, of sea ice from observations are still sparse, but strongly needed. Hence the presented data are of high interest for the community.

Does the paper present novel concepts, ideas, tools, or data? No, methods and observation concepts are not novel. However these are state of the art and adequate to discuss the raised questions. The manuscript strongly reminds of a similar publication for a different cruise (HOTRAX) in 2005. The data are to my knowledge unpublished

Are substantial conclusions reached? The manuscript adds new data to support actual observations and conclusions from other studies. To me, it is not clear what significant new conclusions would be. Those that I would identify (1) comparing EM31 with visual observations (2) the “new Arctic” (3) large-scale melt pond fractions of different ice types, might be right but do not necessarily result in first place from the presented data.

Are the scientific methods and assumptions valid and clearly outlined? Methodical description is too detailed and some information even distracts from the main results. To me, this manuscript reads much more like a comprehensive cruise report than a publication for The Cryosphere.

Are the results sufficient to support the interpretations and conclusions? See above and detailed comments below.

Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes, in particular when including given references from former similar studies.

Do the authors give proper credit to related work and clearly indicate their own new/original contribution? In general, yes. The work of Perovich et al. (2009, JGR) is quite essential here and could be cited more, since most comparison numbers are taken from that manuscript.

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Does the title clearly reflect the contents of the paper? Yes

Does the abstract provide a concise and complete summary? In general yes, but similar to most of the manuscript, it is too descriptive to me and lacks clear conclusions and new findings. It was not really attracting me to read the entire manuscript.

Is the overall presentation well structured and clear? I suggest re-writing most parts of the manuscript, because the extensive field-report-like descriptions should be shortened and be more focused on (whatever is then highlighted) the main results as a base for the main conclusions. This would add something like a good “story line” that I was missing. I would also suggest to review the structure of the sections. The results contain quite some methodical details, the discussion introduces and repeats different results, and the conclusions are mainly a summary of the results.

Is the language fluent and precise? Yes. I would edit quite some small formulations here and there, but since I expect major changes anyway, I do not include line-by-line comments here.

Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes

Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes, see comments below.

Are the number and quality of references appropriate? Yes. Depending on other edits, a few references are likely to be added.

Is the amount and quality of supplementary material appropriate? This does not apply, no such material.

Detailed comments

The new Arctic: Major parts of the results are based on comparing the 2005 (HOTRAX) with the 2010 (CHINARE) data. Based on this, the term of “new Arctic” is frequently

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used and argued. I would be much more careful with this conclusion, since it should not be based on observations of a few weeks within two specific years. These conclusions would need a broader data base. Another aspect is that I would not agree that there was a discrete shift in regimes with the minimum in 2007. This is somehow mentioned and put into relation in the manuscript. I would argue that there are changes, but it is likely impossible to pick one year (2007) as transition as well as two periods (2005 and 2010) for validation.

Sea-ice thickness results: I am most skeptical about the quality of the EM31 data. I would assume that the vessel strongly influences the measurements / results. RV Xue-Long was only about 6m away from the instrument, almost the same distance as the ice-water interface at the bottom of the sea ice (3.5m + 1.8m ice thickness). I would see good reasons that the almost constant signal all over the cruise (0.6-0.8m) is strongly biased by this. I do not agree that the visual observations and the EM31 data agree in any way. I did not fully understand what the final conclusions of the authors is, which data set is more reliable or what the main difference between the visual observations and the EM31 causes. This needs to be clearer. However, I would rather believe in the visual data in this case. Even though these are subjective observations, I assume from my own experience that the observational error is much smaller than the given 1 m. How do the ice station data match the EM31 data from the vessel? These should also match with the along-track measurements. Otherwise the results from these stations are difficult to include if they do not represent “typical” conditions. I get the impression that these do not match well, e.g. mean values of 1.0-2.0m from the floes and 0.6-0.8 m from the ship. What are the conclusions from the EM31 measurements during the ice stations? Sea-ice thickness discussions could also well include similar observations from other vessels in the Arctic (e.g. Rabenstein et al., 2010, JGR).

Increased melt rates: I see it most difficult to compare 12-day mean melt rates with means from SHEBA or other long-term observations, although these are the same seasons. It is already mentioned how sensitive this result is on the occurrence on

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short, but strong, melt events etc. In addition, how did the authors distinguish surface and bottom melt?

Snow on sea ice: When I understood that correctly, most of the time approx. 10cm of new snow covered the sea ice. This is highly surprising to me and should be pointed out more, if correct and not confused with any surface scattering layer (as pointed out already).

MYI on 12-day station: I am somewhat surprised by the conclusion that the 12-day floe was thick (1.8m) FYI. Judging from the thickness, photographs, pond coverage, and floe size, I would have estimated that this was at least MYI. If so, this would also change quite some aspects of discussion. Otherwise, the authors need to argue more carefully why they think that this was FYI.

Publishing data: As the authors mention, the data set would be most suitable for further analyses and e.g. use for remote-sensing data validation. For this, I recommend to publish the data directly, and make them available, too. Publishing them, e.g. in PANGAEA or NSIDC, would add a lot of value and make them citable for other users. There are also international efforts in crating and using uniform observation protocols for sea ice from bridges and to gather these data in common (or at least publically accessible) data bases. How do the presented data relate to this?

IceCam results: How did the authors set the thresholds for the IceCam analyses? I assume that results are quite sensitive to this setting.

Figures: Most plots discuss the observations as a function of geographic latitude. Is there a specific reason to assume that the Arctic sea ice shows strong (or any) North-South gradients? Why should that be? I would definitely expect regional differences, but not necessarily meridional gradients.

Fig. 1: I do not see the necessity to plot the track all the way to China, but it would be most useful to add the track of the major comparison cruise HOTRAX.

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Fig. 2: What is the purpose of this photograph collection? If desired, I would suggest to show pictures of all ice stations from a similar point of view, on a similar scale. The caption contains a lot of details that could better be part of a table (e.g. Tab. 1)

Fig. 4: - The plates are much too small. It is difficult to recognize all lines even in 400- The figure caption is quite confusing. The last sentences should not be part of the figure - Some axis are lacking units. - Floe size: What is floe size “0”? (Put legend into figure) - Floe size: It puzzles me that floe sizes are very small (even “0”) although the ice concentration is almost 100- What is “ice-thickness” in different plots, does it include snow? - It is not fully clear to me what is exactly what in plots E-G, especially the difference of E and G. - I would strongly recommend to split and revise this figure including all subplots.

Fig. 6: Why does this only cover 3 days? The manuscript seems like the IceCam was a continuous system.

Fig. 8: This mixture of photographs, data, table should be improved.

Fig. 11: - What is the conclusion from the drift? How does this relate to the other data and observations? - What is the purpose of the 3D view (in addition to a map with colored dots)?

Fig. 12: This figure is merrily discussed. What do all the single date lines mean, why are they included?

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

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