The Cryosphere Discuss., 5, C111–C115, 2011
www.the-cryosphere-discuss.net/5/C111/2011/

The Cryosphere
Discussions

Interactive comment on "Variability and changes of Arctic sea ice thickness distribution under different AO/DA states" *by* A. Oikkonen and J. Haapala

Anonymous Referee #2

Received and published: 7 March 2011

General comments

This paper describes observations of changes in sea ice draft between 1975 and 2000 derived by submarine upward-looking sonar measurements provided by the National Snow and Ice Data Center. Results indicate marked thinning between the two periods 1975-1987 and 1988-2000, and the authors relate these qualitatively to changes in the Arctic Oscillation (AO) and Arctic Dipole Anomaly (DA) indices. Although analyses of past ice thickness changes are important to better understand the mass balance of Arctic sea ice in relation to climate forcing, and to improve predictions of the future fate of sea ice, this paper adds little new information to the already known changes

described by other authors using the same data sets. Unfortunately, the authors fail to clearly delineate their own findings from the results of past studies, and to demonstrate convincingly what new conclusions can be drawn from their analyses. Apparently, the new aspects of the present paper are that focus is limited to the 1975-2000 time period, that more extensive data are used including a wider seasonal and regional coverage, and that relations to the DA are attempted. All these aspects need to be better outlined and supported. The authors should present a more thorough review particularly of the Tucker et al. (2001) and Yu et al. (2004) papers, and describe the findings of those studies in the introduction (rather than just citing them), which indeed already discussed ice thickness distributions and ice thickness classes, and pointed out the changes occurring in 1987/88 with regard to AO changes, identical to results presented by the authors. The authors should also provide a more thorough discussion of the AO and DA, providing more convincing evidence of some causal relation with ice drift and thickness. For example, this could be done by means of a figure showing differences in atmospheric pressure and wind patterns (maps) between the two periods, or even differences in ice drift. Indeed, even this was already convincingly presented by Tucker et al. It would be nice if these changes could be done as a minimum in a major revision of the manuscript. Finally, the authors address changes in different seasons and find that long-term changes are largest for measurements in spring, an interesting and important result. However, the discussion of these changes is somewhat blurred, and the authors fail to provide convincing conclusions. In particular, it would be nice to include a more thorough discussion of the impacts of variations of the length of the melt season, which has increased according to the authors. Why would this not cause stronger changes of autumn thicknesses? The paper is well written but very lengthy and there are only few typos.

Specific comments

Title

Replace "thickness" with "draft". Spell out AO/DA, or remove completely as suggested by the other reviewer.

Introduction

P133, l19-20: These statements are not fully appropriate, as at least Tucker et al. and Yu et al. discuss thickness distributions and ice classes and compare changes between different periods. L25 ff: g(h) in Eq. 1 is actually dimensionless (a fraction/probability), but when divided by dh has dimension 1/m. P134, l2 ff: I think this classification is problematic and the discussion may be misleading and could be avoided, because multiyear ice can be < 2 m thick in the end of the summer and deformed FYI can contribute to ice thickness classes in the 2-3 m range and above. While a classification is general and while the used intervals may be appropriate, the authors should therefore avoid the additional classification of FYI/MYI. Instead, it may be more useful to attempts classifications into level and deformed ice, and to contrast the areal fractions of those. Indeed this could become an interesting new aspect of the paper, but obviously requires much more work before results can be obtained. Similarly, the paper could analyze changes of ridge spacing and depth distributions. P135, l15: be more explicit about NSIDC as the main source of data. In general, the introduction is quite lengthy and could possibly be written much more concise without los of information, even if results of Tucker et al and Yu et al were more explicitly included.

Data and analyses

P137, l2: Could you better explain the reasons for and implications of this statement. L14: Although probably open water fractions were not discussed by other studies either, it would nevertheless be interesting to include these observations, which could also add another new aspect to this paper. You could define open water as thickness class 0 – 0.1 m. What bin widths did you actually use to compute draft distributions?

Results

This section is very lengthy and could focus more on a summary of the main results.

Maybe some additional information could be included in the tables. P140, l14-20: These are interesting results. What do they imply for the importance of summer melt and the role of melt ponds and ice concentration for ice-albedo feedback? P142, l3-4: This could be picked up later when Fram Strait ice fluxes are discussed. L28: replace increament by increase? P143, top: a discussion of level versus deformed ice may be more useful than of ice thickness classes? L10L remove “deal of” P144, top: again, this discussion could become more interesting if level and deformed ice were distinguished from the profile data, which could also include an analysis of ridge depth and spacing distributions.

Discussion

The discussion is very lengthy and qualitative. It should be shortened and more focused on what really can be learned about the observed changes, rather than include an extensive discussions of all aspects governing the thickness distributions which have no quantitative relation to the results. P144, l7-18: this paragraph should be moved to the introduction. P145, l26: replace has with have P146, l6 ff: What are the implications of a stronger thinning in spring? P148, l5 ff: These are all established arguments but how to they explicitly relate to the observed changes? And what is explicitly know about changes of boundary conditions? P149: very length discussion. P150, l10: Your results show no change in Nansen Basin; how would this relate to Vinje’s results? P151 ff: This is all very qualitative and superficial, and the authors should really focus on causes they address more explicitly, like the presentation of a figure with AO/DA differences between the periods as suggested above.

Conclusions

P154, l4-5: be more careful with your discussion of reduced deformed ice and decreases in mean and MODAL thickness. L14-22: All these papers describe exactly the same behavior. What is new about your results: What does the “different” refer to in line 18?

Tables:

Tables would be easier to read if you would explicitly state the names of the regions rather than use only numbers. Table 2 could include open water fractions. Table 3: include a verbal description of categories 1-2 and regions, either in table caption or in first row.

Figures

Figure 2: Include names of regions. Other figures: replace Region x with region names for better readability. Figure 6: Name categories, either in caption or in legend.

Interactive comment on The Cryosphere Discuss., 5, 131, 2011.