The Cryosphere Discuss., 5, C432–C449, 2011
www.the-cryosphere-discuss.net/5/C432/2011/

The Cryosphere Discussions

TCD
5, C432–C449, 2011

Interactive Comment

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

A. Oikkonen and J. Haapala

annu.oikkonen@helsinki.fi

Received and published: 12 May 2011

Response to referees

We would like to thank all three anonymous referees for very useful and constructive comments. All the comments and suggestions helped significantly in improving the manuscript. Most of the suggested changes have been done following the comments.

All three referees mentioned a concern about a weak relation between AO/DA-indices and the changes in the Arctic ice pack. Following the suggestion of referees, we have changed the title; the new title does not include a statement about the importance of AO/DA-states. The new title is "Variability and changes of Arctic sea ice draft distribution – Submarine sonar measurements revisited".

Referees also pointed out the speculative nature of the discussion about the dynamic forcing and about the importance of AO/DA-states. In order to have more explicit and quantitative approach and to have more support for the statements in the discussion, we have added one new figure. This figure is attached to the response (Figure 7.). The new figure shows the average ice drift during our two study periods, 1979-1987 and 1988-2000, basing on IABP buoy data. This figure clearly shows the difference in ice drift patterns. During the former period Beaufort Gyre was much stronger and ice in Transpolar Drift was originated in clearly more eastern parts of the Arctic than during the later period. Due to the westward shift of Transpolar Drift, the large part of the ice entering Fram Strait drifted over North Pole and had origin in the western Arctic during the later period. This new figure of ice drift clearly supports the statements of the impact of dynamics on the evolution on ice draft distribution in different regions presented in the discussion (Chapter 4.4., page 155->).

Another general comment mentioned by two referees was a failure in delineating new findings compared to earlier studies basing on submarine data. With the new title we also want to point out that we are aware of these studies and want to put our work in the context of previous analyses. We strongly believe, that our approach and results differ from them substantially and that the "revisit" is therefore well reasoned.

Many of earlier works basing on submarine sonar data have limited to concern mean sea ice thickness or draft, e.g. Rothrock et al. (1999) and Rothrock and Zhang (2005). However, the state of the ice pack is best characterized by the ice thickness distribution, and changes in thickness distribution may reveal whether the changes are due to predominantly thermodynamic or dynamic processes. Only few studies (Wadhams and Davis, 2001; Yu et al. 2004) have shown the ice thickness distributions. Our analyses are much more extensive than these earlier works and provide more detailed results about changes in ice thickness distribution. We show ice thickness distributions for six regions, two seasons and two 13-year long periods, 1975-1987 and 1988-2000. Our results base on the data from 31 submarine cruises, which is much more than utilized in previous studies; the number of cruises analyzed in the work of Wadhams and Davis (2001) is only 2, and in the work of Yu et al. (2004) the corresponding number is 7. In Wadhams and Davis (2001) analyses are very limited also regionally, covering only Greenland Sea and Eurasian Basin. Also the work of Tucker et al. (2001) bases on submarine sonar measurements, but they do not show changes in ice draft distributions, but only in the fraction of four coarse ice draft classes. Tucker et al. (2001) cover only the narrow sector from Alaska coast to North Pole and a time period of 10 years. The longer time period covered in our work shows very different evolution of ice pack in North Pole: Tucker et al. did not observe any significant change in autumn mean ice draft, but results from longer time period show very strong thinning also in North Pole, both in spring and in autumn. This can indicate that the change in this region has occurred mostly before year 1986, when the study period of Tucker et al. starts.

Important difference in our approach compared to previous works is that we extend the analyses to cover two seasons, spring and autumn. The results reveal differences in the evolution between two seasons, since thinning has been in general more pronounced in spring than in autumn. And as the referee #3 suggested, we have added more thorough discussion about this seasonality of the changes. Unfortunately we cannot give an unambiguous explanation for the causes nor the implications, but possibilities are discussed and hopefully this new observation raises interest and will be considered in future studies. In our analyses we determined seasons so, that spring includes data from April and May, and autumn covers September and October. During the both periods timing of submarine cruises were equally distributed around the center of the date range of both seasons. This makes it possible to avoid seasonal adjustment of measurements, which has been done in many other studies (e.g. Rothrock et al. 1999; Yu et al. 2004). This way we could minimize the risk of artificial effect.

In the following we respond to the minor comments of each of the referees.

Referee #1:

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

List of recommended references:

AC: Thank you for comprehensive list of further publications. We were already familiar with most of the papers, and they all have been included in our manuscript. New and very interesting paper for us was the work of Rodrigues.

RC: Pg 132, Line 5 and in the title: define AO/DA

AC: AO/DA is removed from the title and the abstract.

RC: Pg 135, Line 16: A 2005 US submarine cruise is now available at NSIDC.

AC: Unfortunately we cannot utilize that data in this study, since it is from July and November and does not fall into either of the seasons used in our analyses.

RC: Pg 135, Line 23: While there is some link between the AO, the DA, and the ice thickness distribution, it must be remembered that the AO and the DA are statistical constructs and not physical modes, so while they are useful descriptors of the atmospheric circulation they can't really be said to cause anything. While you have showed a loose relationship between the DA and the thickness distribution there is also a general trend in mean ice thickness that continues regardless of the phase of the AO and DA. This fact should be discussed. It is also difficult to make sense of connections between the atmospheric forcing from one year and the ice thickness since the thickness represents the integral of the dynamic and thermodynamic forcing over several years.

AC: We want to thank the referee for this very good point. We agree that it is misleading and even wrong to say that AO/DA-indices or states would cause something. This kind of expressions have been changed. Also, overall, we have changed the focus of the manuscript so that the connection between AO/DA-states and the evolution of ice pack has less weight. AO/DA-indices are discussed in introduction and in the sections 4.3 and 4.4, but the statements about differences in atmospheric circulation between the two periods and about their impact to sea ice dynamics bases now on the new figure. This new figure shows the ice drift during the periods 1979-1987 and 1988-2000 from the IABP drifters.

RC: Pg 136, Line 22: Mention Rodrigues 2011 and say what the implications are for your analysis.

AC: We want to thank the referee for pointing out this very new, important and interesting paper. It was actually published after the submission of our manuscript, so we were not aware of its result at the time we were making our analyses and writing the manuscript. Definitely, these new estimations about the accuracy of submarine sonar measurements is worth of mentioning and the presented beam width corrections should be done when possible to minimize the error. Unfortunately we cannot apply the method presented in this paper, because for that e.g. the depth of the sonar would be need, and this information is not available in NSIDC. However, we have added it as reference, explained why we do not utilize these results in our analyses, and included short discussion about the implication of this work of Rodrigues.

RC: Pg 137, Line 6: I am concerned that there could be large regional variations in the ice draft, particularly in regions 2 and 4, that might be sampled differently if the cruises covered different areas within the region in the two time periods. Is there a way to show this hasn't happened? Maybe by showing maps of the sample locations for the two time periods and the two seasons would help.

AC: We have changed the Figure 2. The new figure shows cruise tracks separately for spring and autumn, and for two periods 1975-1987 and 1988-2000. We have also added the comment about region 4 (Chukchi Sea) that in the autumn, the difference in sampling latitudes may impact on observed change.

RC: Pg 137, Line 22: You might change the name of the third ice category to "heavily deformed" or "thick deformed" as deformed ice can be found for any thickness.

AC: Because the determination of different ice types basing on only draft is not at all unambiguous, we prefer calling the ice categories with numbers (instead of naming them after dominanting ice type). Ice category 1 (d<2m in spring and d<1m in autumn) is dominated by FYI, but may consist also thin MYI and thin deformed ice. Similarly, ice in category 2 (2-5m in spring and 1-5 m in autumn) is mostly MYI, but contains also some amount of different ice types. However, in the text, to make it more illustrative, we use also terms FYI, MYI and deformed ice when we describe the evolution of different categories. In this purpose we use now the term "thick deformed", following this suggestion. But always when we refer to certain ice category, we include also the number of ice category (1, 2, 3).

RC: Pg 140, Line 23: Here and throughout the rest of the paper you have confused ice volume with mean ice draft. Volume has dimensions of m3, never of m. Please change all the references to ice volume to ice draft.

AC: Thank you for pointing this out. In the revised version we have paid attention to the notation, and replaced ice volume with mean ice draft in most of the cases, i.e. always when it refers to integrated distributions. We have kept the term ice volume in the sections/sentences describing the composition of the ice mass, because this terminology (composition of ice volume) seems to be widely used and accepted, even if it is incorrect (in reality dimension is volume per area, i.e. thickness). Somehow, "composition of mean ice draft" does not sound plausible. If there is any better expression, we would be willing to change the term.

RC: Pg 141, Line 9: Please define the cumulative mean ice draft more clearly.

AC: We have determined the ice volume distribution V(d) following Yu et al. (2004): ice draft distribution is multiplied by the mean draft of each bin. For each bin, cumulative ice volume (or cumulative mean ice draft; problem with correct term again) is then the sum of the volumes of bins with smaller mean draft.

RC: Pg 140, Line 10: Percentual → percentage

AC: This has been changed.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

RC: Pg 148, Line 25: What are the intervals for these trends?

AC: We have added the magnitude of the SAT trends. In the revised version: "The surface air temperature (SAT) in the Arctic shows a positive trend, with the greatest rate in the winter and spring, and on the eastern side of the Arctic (up to +2°C decade-1) (Rigor et al., 2000; Polyakov et al. 2003) ."

RC: Pg 151, Line 10: I think you may be overstating the differences in the AO and DA

AC: In the revised version of the manuscript the statement about the differences in ice dynamics during two periods bases mostly on the observed drift from IABP buoy data, not only on assumed differences during different atmospheric circulation patterns determined by AO/DA-indices. Difference in ice drift is presented in a new figure, that we have added to manuscript. This figure shows the ice drift during the two study periods, 1979-1987 and 1988-2000, confirming the statements about the impact of ice dynamics on the evolution of regional ice draft distributions.

RC: Table 1. I think km of track sampled would be more informative.

AC: Following this suggestion we have changed the table 1 so, that it shows the length of submarine track in kilometers instead of number of measurements.

Referee #2:

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

AC: The title has been changed to "Variability and changes of Arctic sea ice draft distribution – Submarine sonar measurements revisited"

RC: 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.

AC: This has been changed. The works of Yu et al. (2004) and Tucker et al. (2001) are now referred and summarized in the introduction. Introduction includes now also much more detailed description what are the differences between our work and previous papers (with similar arguments than presented in the beginning of this response).

RC: L25 ff: g(h) in Eq. 1 is actually dimensionless (a fraction/probability), but when divided by dh has dimension 1/m. AC: The incorrect and confusing sentence has been removed.

RC: 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.

AC: We agree that classification of different ice types is problematic, and it is impossible to separate FYI, MYI and deformed ice into own categories basing only on ice draft or thickness. Therefore we have named our ice categories simply 1, 2 and 3, instead of using terms FYI, MYI and deformed ice. Even if this classification is simple, it reveals important information about changes in the composition of ice mass, which have led to observed thinning. In the revised manuscript we have paid even more attention to all the statements about level ice and deformed ice.

RC: P135, l15: be more explicit about NSIDC as the main source of data. AC: Has been changed: “We utilize data from submarine cruises of the U.S Navy and the Royal Navy from the years 1975-2000. The data are archived and publicly available at US National Snow and Ice Data Center (NSIDC).”

RC: In general, the introduction is quite lengthy and could possibly be written much more concise without loss of information, even if results of Tucker et al and Yu et al were more explicitly included.

AC: This has been done. We have included a short summary of Tucker et al. (2001) and Yu et al. (2004), as well as comparison to our study. On the other hand, we have shortened other parts of the introduction, most clearly the sections about atmospheric circulation and AO- and DA-indices, and the description of characteristics of ice thickness distribution.

RC: P137, l2: Could you better explain the reasons for and implications of this statement.

AC: Spring season is defined as April and May, autumn as September and October. The annual maximum ice thickness in the Arctic occurs usually in the end of April or beginning of May (e.g. Rothrock et al. 1999), i.e. in the center of the date range of the determined spring season. Correspondingly, in the autumn, the minimum ice thickness occurs typically in late September. Around the date of annual maximum or minimum value the ice thickness changes rather slowly (especially in spring) and the changes in thickness around the date of extreme value are symmetric. With this we mean, that one month before or after the annual maximum/minimum value, thicknesses are very similar. During the both study periods (1975-1987 and 1988-2000) timing of submarine cruises were equally distributed around the center of the date range of both seasons. This made it possible to avoid temporal adjustment of measurements, which has been done in many other studies (e.g. Rothrock et al. 1999; Yu et al. 2004). This way we could minimize the risk of artificial effect.

RC: 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?

AC: We agree, that changes in the open water fractions would be very interesting aspect. Unfortunately it cannot be done reliably from submarine sonar data. There are several problems in determining the open water (described in Rothrock and Wensnahan, 2007) which have been taken into account when submarine sonar data have been processed before archiving in NSIDC. Even after open water corrections applied, determination of open water contains significant risk of error. AC: We have used bin width of 0.2 m.

RC: 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.

AC: We did not shorten the Results-section much. We believe that the value of this paper is in detailed, quantitative results about changes in the Arctic sea ice draft distributions, which have not been presented in this extent in earlier studies. However, we agree that the manuscript is rather long, and we have been shortening the introduction and the discussion.

RC: 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?

AC: Strong thinning especially in spring, in the beginning of the melt season, could have enhancing impact in the albedo-feedback mechanism. On the other hand, this kind of impact could be assumed to cause even stronger decline in the ice thickness in the end of the melt season, in autumn. So, considering only this observation, the importance of summer melt could be actually smaller in the long term changes than the impact of changes in ice growth in winter. But basing on only this result we cannot make any strong and reliable statement.

RC: P142, l3-4: This could be picked up later when Fram Strait ice fluxes are discussed.

AC: This has been changed accordingly.

RC: L28: replace increament by increase

AC: This has been changed accordingly.

RC: P143, top: a discussion of level versus deformed ice may be more useful than of ice thickness classes?

AC: This has been taken into account.

RC: L10L remove “deal of”

AC: This has been done.

RC: 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.

AC: We agree that different kind of method of determining level and deformed ice could show interesting results. However, eventhough our classification is simple, it reveals important information about changes in the composition of ice mass, which have led to observed thinning. More detailed analyses about ice ridge depth and spacing would be an interesting topic for future work, especially because the loss of deformed ice volume seems to be a major factor in observed thinning.

RC: 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.

AC: We have added one figure showing the ice drift during the two study periods 1979-1987 and 1988-2000 basing on IABP data.

RC: P144, l7-18: this paragraph should be moved to the introduction.

AC: In the revised manuscript, the studies referred in this paragraph, are very short summarized in introduction, as our study is set in the context of previous works.

RC: P145, l26: replace has with have

AC: This change has been done.

RC: P146, l6 ff: What are the implications of a stronger thinning in spring?

AC: Stronger thinning in spring could be assumed to cause more open water areas when summer melt proceeds, and thereby an increase of the heat absorbed to the ocean. Increased oceanic heat flux would enhance lateral and bottom melting. Basing on this view, thinner ice cover in spring could be assumed to result in at least equally thinner ice in autumn, especially since the lengthening of the melt season has been observed. However, this was not observed in autumn ice thickness. The increased oceanic heat flux can effect also during winter, and reduce and slow down the ice growth.

RC: P148, l5 ff: These are all established arguments but how to they explicitly relate to the observed changes?

AC: The strongest and regionally as well as seasonally the most uniform observed change was the loss of thick ice. Thickest ice category is dominated by deformed ice and a straightforward explanation for the loss in this category would be a changes in ice dynamics. In this paragraph, to which reviewer refers, we discuss, if and how the observed loss of thick, mostly deformed ice could be a result of changes in thermodynamics.

RC: P149: very length discussion.

AC: This has been shortened.

RC: P150, l10: Your results show no change in Nansen Basin; how would this relate to Vinje's results?

AC: Vinje (2001) found no trend in sea ice export in Fram Strait, but reported high

TCD

5, C432–C449, 2011

Interactive Comment

variability. Vinje estimated, that anomaly in ice volume outflow through Fram Strait could cause up to 0.8 m change in ice thickness, if distributed over the whole Arctic Ocean, over period of 7-12 years. According to Vinje, the period 1990-1997 showed increase of about 40% in efflux. Among our six study regions, Nansen Basin is the region closest to Fram Strait, but strong variations in the outflowing ice volume were not reflected in the ice thickness distribution in this region. Nansen Basin is a region, where ice advection plays an important role. As the new figure of ice drift shows, the origin of the ice advected into Nansen Basin shifted from eastern parts of the Arctic to much more western Beaufort Gyre. During the later period, which includes also the years of very high ice export reported by Vinje, more thick MYI from Beaufort Gyre was captured to Transpolar Drift, and entered first Nansen Basin balancing the thinning impact of thermodynamics, and further drifted through Fram Strait causing anomalously large fluxes.

RC: 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.

AC: In order to have more explicit and quantitative approach and to have more support for the statements in this discussion, we have added a figure showing the ice drift during two study periods, 1979-1987 and 1988-2000. This figure clearly shows the differences in ice drift and supports the statements of the impact of ice dynamics in the observed strong changes in Easter Arctic and nearly unchanged ice thickness distributions in Nansen Basin.

RC: P154, l4-5: be more careful with your discussion of reduced deformed ice and decreases in mean and MODAL thickness.

AC: In the sentence that this comment refers, "is mainly responsible" is very strong statement. It has been changed to "has had a large impact".

RC: 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?

AC: In the beginning of this response there is more detailed description of differences between our study and earlier works. In general, this study shows the changes in Arctic sea ice draft distributions more extensively and more in detail than earlier papers. Regarding the new results, we can point out the seasonality in thinning (stronger decline in spring than in autumn), which is discussed more thoroughly and more in detail in revised manuscript. Compared to other studies about ice thickness distribution, larger data set reveals clearly different evolutions in some regions. E.g. 10-year long period analyzed by Tucker et al. (2001) did not show any significant change in North Pole, while a longer time period covered in our work, shows very clear thinning and strong loss of thick, mostly deformed ice, both in spring and autumn.

RC: Tables would be easier to read if you would explicitly state the names of the regions rather than use only numbers.

AC: We have added also the names of the regions to the tables.

RC: Table 2 could include open water fractions.

AC: Unfortunately, determination of open water cannot be reliably done from submarine sonar data.

RC: Table 3: include a verbal description of categories 1-2 and regions, either in table caption or in first row.

AC: Description of ice categories has been added in table caption.

RC: Figure 2: Include names of regions.

AC: We have changed the Figure 2. The new figure shows cruise tracks separately for spring and autumn, and for two periods 1975-1987 and 1988-2000. We have also included the names of the regions in the figure caption. New version of Figure 2 is attached.

RC: Other figures: replace Region x with region names for better readability.

AC: We have added the names of the regions.

RC: Figure 6: Name categories, either in caption or in legend.

AC: Definition of ice categories has been added in figure caption.

Referee #3:

RC: Line 64: this is the first mentioning of open water. It should be discussed earlier, when categories are defined.

AC: We have added a mentioning of open water in the beginning of previous paragraph (where FYI, MYI and deformed ice are first mentioned).

RC: Page 4, top. Definition of AO years and DA years, including Figure 2, is very confusing. From this figure it is impossible to deduce whether a certain year is dominated by AO or by DA. This is also true for the two time periods used by the authors in the text.

AC: We have made Figure 1 (AO- and DA-indices) more clear. We have marked several years, not only the beginning and the ending years of the two periods. Now it is easier to follow how different years are located in AO/DA-space.

RC: Page 9: References to Rothrock et al. 2003 and Kwok and Rothrock 2009 would be appropriate in Section 4.1.

AC: This has been changed accordingly.

RC: Sections 4.3 and 4.4 look like a speculation. The authors presented no solid proof that changes in PDFs are really related to the factors described in these sections.

AC: In the section 4.4 (dynamic forcing) we have added new figure showing the ice drift during two 13-year study periods basing on IABP drifters. In revised manuscript the discussion about the changes in dynamics and about their impact on the evolution of regional ice draft distributions bases mostly on this figure, i.e. on observations of ice drift.

RC: Figure 1. It would be nice to see time series of PDFs (if possible) and AO/DA indices.

AC: It would be possible to show yearly PDFs. This would just need numerous figures to be shown clearly, since we have data from about 20 years, and PDFs from two seasons and six regions.

RC: Figure 2. Please describe what inhomogeneous colors within each region mean.

AC: We have changed the Figure 2. The new figure shows cruise tracks separately for spring and autumn, and for two periods 1975-1987 and 1988-2000. New version of Figure 2 is attached.

RC: Figure 6. Please describe categories in figure caption.

AC: Description of ice categories has been added to figure caption.

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

TCD
5, C432–C449, 2011

Interactive Comment

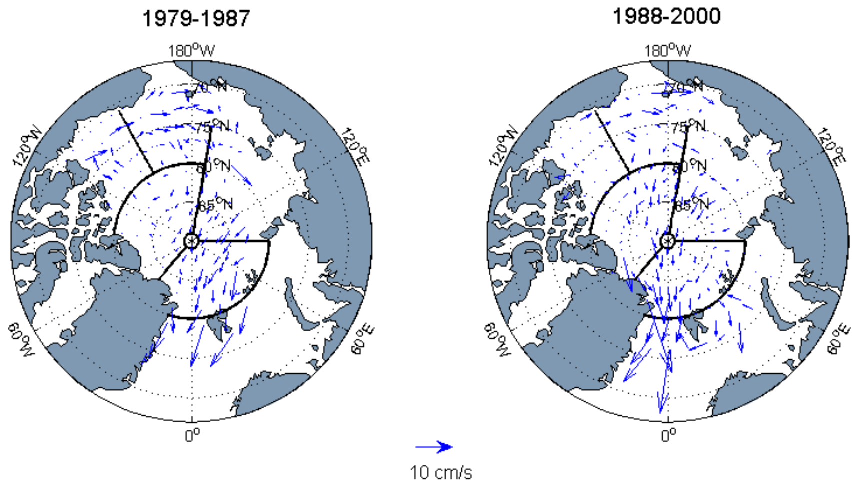

Fig. 1. Figure 7. Sea ice drift during the periods 1979-1987 and 1988-2000.

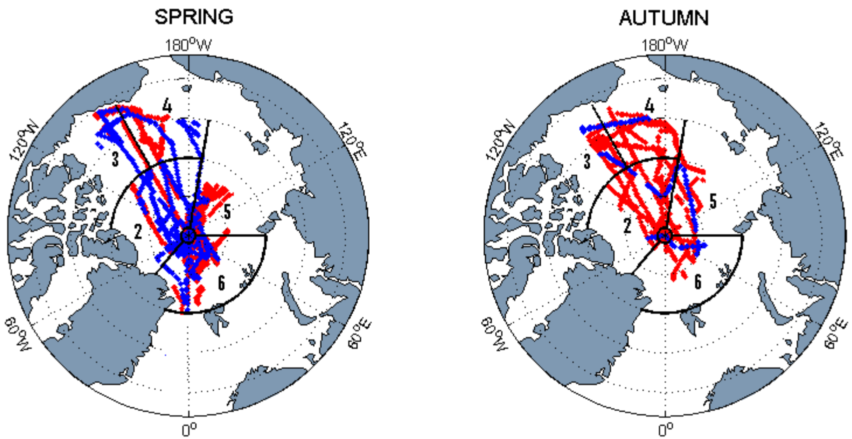

Fig. 2. Figure 2. Submarine cruise tracks in Spring (on the left) and Autumn (on the right), and during the periods 1975-1987 (blue) and 1988-2000 (red).

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper