The Cryosphere Discuss., 7, C773–C783, 2013
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The Cryosphere Discussions
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Interactive comment on "North Atlantic warming and declining volume of arctic sea ice" *by* V. A. Alexeev et al.

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Received and published: 14 June 2013

We thank the reviewer for the suggestions. Below are the answers.

Some general comments:

1) A better explanation of the theory linking the Fram Strait influx and the ice melt would be helpful. A cartoon illustrating the postulated mechanism and identifying the different areas discussed in the study so that they can be referred to in the text would be extremely helpful.

We discussed if the cartoon is needed for the article and came to a conclusion that maybe not. Instead, we are adding graphs of correlation between the Atlantic Water in Fram Strait with bottom melt that will illustrate the effects of the warm Atlantic water on

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sea ice.

2) I am having a little trouble with the stated fact that the AW represents a heat source capable of “melting the ice several times over” and the idea that a change in the amount of heat in the flowing north makes a difference to the ice. If this is to hold, then obviously the processes controlling the heat flux to the ice must be somehow controlled by the amount of incoming heat. Since this is at the heart of the theory, this reviewer at least needs more handholding to follow.

The AW is insulated from the surface by the cold halocline almost everywhere in the Arctic Ocean, except for the area of interest near Svalbard. Changing the stratification (removing the halocline) in the Arctic Ocean could lead to a complete disappearance of the arctic sea ice – that was the main idea behind the statement of AW being capable to “melt the ice several times over”.

Reference to Rothrock D.A., Yu Y., Maykut G.A. Thinning of the Arctic sea ice cover // GRL. 1999. V.26, P. 3469 – 3472 is addressing this question.

3) Some of the figures are rather small and difficult to read. The insets in Fig. 1 are confusing and I don’t see the reason for placing them inside the figure for an online publication. Maybe this was from a prior submission that had a space limitation?

We modified Figure 1 by breaking it up into three panels.

4) Clarify reference periods and areas when anomalies are considered. Be clearer, maybe separate sub-headings, when different geographical areas are discussed, add location markers into the maps so a reader can easily refer to the locations talked about in the text.

Added some reference periods for some anomalies calculated in the paper. It would be nice to hear a little bit more specific suggestions about markers and maps.

Specific criticisms are outlined below in sequence.

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246, 15: : : 150-200 kmˆ3 : : : I can't find a place in the paper where this number is derived. I see 130 kmˆ3.

130 km^3 was our lower end estimate. We therefore decided to up our estimate a little bit for the abstract. Added the 130 km^3 to the abstract referenced as the lower end estimate.

247,27 .. strong seasonal cycle I can't follow what is being said here and how the seasonal cycle and its propagation into the Laptev sea is connected to the thinning considered here.

Rephrased the sentence in the following way:

It also appears that FSBW temperature exhibits a strong annual cycle, reaching maximum values at minimum depth over the study area in late fall and winter and thus providing the strongest heat impact on the ice cover during this season (Ivanov et al., 2009).

248,6 Zhang et al. 2010? This paper talks about AW warming but in that context means "anthropogenic warming": : : There is a reference by Zhang et al. 1996 that looks at the Atlantic water inflow which maybe more relevant.

Thank you. The reference to Zhang et al, 1998 (not 1996) is included.

248, 8 : : : 0.1 m of ice melt/yr: : : I am not sure if this adds up. 0.1 m/yr over 30 years would yield a 3m ice loss. That seems excessive for an average.

We do not see any problems with this. This melt happens with a strong seasonal cycle in the background, therefore one should not expect to see a cumulative effect. We eliminated the reference 0.1 m melt in order to avoid confusion.

Page 247,29: : : as substantial fraction of AW Heat: : :. How do we know this? Is this an assumption, previously established knowledge or conjecture at this point?

We know this from other already available (and published) quantitative estimations.

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The reference on (Polyakov et al., 2010) is given.

250, 3 Figure 1. Warming accelerated: I do see a change with a transition maybe best placed in 1990. The idea of an acceleration calls for substantiation.

We changed the wording to “The warming has peaked in the 2000s”.

250, 12 after entering the Arctic Ocean.... the warm water near the surface: : : this AW signal

propagates

It appears from the argument here that in Whalers Bay, the AW has been able to “melt-out” sea ice even before the increased influx that occurred after 1990. If the AW “signal” propagation occur before 1990 what has changed since then to affect ice cover elsewhere? Figure 1. This figure appears unnecessarily dense and is confusing. Spatial and temporal connections appear misaligned. The vertical profiles from the cross-sections barely show that the AW water is in contact with the sea ice. In 2008 this contact seems minimal (if I’m seeing this correctly). The case would be substantially strengthened if transects/profiles in this area prior to the 1990 transition period showed substantially different vertical structures.

There are no transects available along 30 deg.E in late fall (October-December) except of the NABOS 2008, shown in the figure. We doubt that such data exist elsewhere. We changed the layout for Figure 1. We still think that the transects clearly illustrate that the Atlantic Water is in close contact with sea ice. We can only work with data that are available and the data from that area are not very abundant.

250, 1 Curious minima: : : On might argue that those minima are only curious because a small area is selected. There are lots of other areas where sea ice has thinned substantially. I think this selection of focus need to be better justified.

The ice in the area of interest is advected from other areas with thicker ice. We have included analysis of back trajectories and correlation of AW temperature with bottom

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melt.

250, 2 : : : thinning is a change in atmospheric circulation and associated ice motion or .. heat budget. This possibility isn't really examined in detail. There are some 'ice and heat' budget arguments made but none sufficient to really reject the alternate hypotheses in my view.

We added pictures with correlation between AW and bottom melt, also a graph with back trajectories of sea ice that illustrate that the ice is of 'local' origin or is advected from areas with thicker ice for all years, except for 2004.

250,10 in all years the thickness minimum is located far away from the ice boundary. The ICESat thickness maps don't go to the ice boundary so we don't know what the thickness variation south of the ICESat boundary is.

The uncertainty increases as we move close to the ice margins because of unknown snow depth. That is why we have not provided retrievals close to the ice edge – not that there are not ICESat acquisitions but that the uncertainties are much more difficult to quantify.

250,2 The use of the NorESM is a good idea in helping to shore up the physical plausibility of the presented argument. Even if not accurate in detail, a replication of the fundamental mechanisms in the model would lend substantial credence to presented hypothesis. Unfortunately the way the model is used here doesn't help a great deal. Can you show that there is a connection between AW inflow into the Arctic and sea ice variability in the model? Showing a strong correlation between this influx and sea ice cover in this sector would be a big help for this paper. Note also Holland and Serreze, 2010 who found that ocean heat transport was only a significant contributor to ice loss in one of the AR4 models. Fig 3 I think the argument of continual thinning of the ice from 2004 to 2004 asks for differences maps of ice thickness.

We have now included a graph with correlation of AW temperature with bottom melt.

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The high bottom melt north of Svalbard is well correlated to the simulated temperature in the AW.

250, 17 qualitatively similar ..These are bottom melt averages: This doesn't seem to support the notion of an increase due to greater influx..

See comment above.

251, 14 clearly driven: I don't see this clearly. The connection is plausible, but the impact on ice melt hasn't been demonstrated (same as above).

We agree – this is has now been addressed. Although other sources of this melt in mid winter is not likely the clarity was not properly demonstrated. Adding the simulated correlations improves this, but to "prove" this we need in situ observations of the upward heat flux, something that is not available.

251, 26 I cannot follow what's being done in the next two paragraphs.

We tried to improve the text in those two paragraphs. We would have liked to see some more specifics about what exactly is unclear.

252, 2.. the shallow location of the: : :.. that it could be well above the freezing point? Why "could be" don't you have the temperatures or is it a question of the uncertainty of the measurements? I don't understand.

Changed the sentence to: "The temperature right underneath the ice in September 2006 and October 2008 is well above the freezing point (NABOS data: www.nabos.iarc.uaf.edu)."

253,6 a ocean heat flux of 100 Wm-2 : : : is therefore a realistic value? I can't follow this?

We feel that the ocean heat flux of the order of 100 W/m^2 is quite realistic during vertical convection and it is not something incredibly high (also see our reference in the text).

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252,12 I can't follow this estimate. An illustration would be helpful. Are you trying to estimate the rates of thinning that would be necessary to affect the observed changes in thickness due to the process you are postulating? This sounds like a good idea but it is a bit hard to follow this argument here.

We could of course add a sketch here, but it would be very simple and primitive. Our argument is very simple: ice melts in the warm water, and the warmer the water the faster the ice melts. The argument is based on numbers with reference to McPhee.

252, 20 calculated anomalies of the heat budget? You are hardly considering the whole heat budget. There is snow and downwelling longwave radiation. Neither the ice dynamics nor the heat budget are treated with sufficient detail to eliminate those as causes of the thinning.

We added a plot with back trajectories to illustrate where the March ice originates.

252,21 warmer air, water vapor from more open water ->Local thinning? DLW anomaly for 2008? What's the base period? Why NCEP for DLW and ERA-Interim from another? Seems like the ERAInterim product would be the superior product. Explain if doesn't matter.

The fields were chosen based on immediate availability of products. References to the baseline averaging period have been added. Qualitatively the pictures of anomalies in DLW are very similar, so it does not make too much difference. We will use the same products if the reviewers will insist.

253,1 What is the conclusion about DLW. I do see positive DLW anomaly in the area. It appears that the argument is that DLW can be excluded as the cause of the thinning in this area. If so, it is a little hard to see why this would follow if indeed the anomaly was positive and would thus have led to thinner ice.

The minimum in sea ice thickness is reached right along the zero line of the downwelling radiation anomaly in 2008. Besides, the shapes of the anomalies are com-

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pletely different.

253, 6 The snow-on-ice anomalies should be : : : correlated with DLW anomalies because both depend on precipitable water. This state requires substantiation. The DLW primarily depends on air temperature, clouds and to some degree on precipitable water. The connection between snow precipitation and total precipitable water also requires substantiation if this argument is to be invoked.

Completely removed the sentence to avoid possible confusion.

256,10. The argument about snowfall needs some explanation.. Are the authors talking about an effect on the ICESat retrievals or the effect of snow on limiting thermodynamic growth? The ICESat thickness retrievals account for snow depth using data similar to the ones used here, so the argument would have to involve an a "miscorrection". This would need explanation. The thermodynamic growth hypothesis would have to involve snowfall over the trajectory of the ice over the growth season or some way to explain the "local" snow effect.

We will use the same argument as for the DLW anomaly above. As we are pointing out, snow can affect both ice growth and accuracy of retrievals. However, the shapes of the snow anomalies are completely different from anomalies in sea ice thickness. As follows from out back trajectories analysis, ice does not travel too far in one season, so more of the ice is 'local' to our area of interest. Therefore the 'observed' DLW and snow anomalies cannot produce observed anomalies in thickness.

253, 14.. is clearly related to more open water there: : : plausible, certainly, but "clearly related": : : evidence for this needs to be provided.

Changed to 'likely linked'.

253, 24 It seems that the "ice-dynamics" hypothesis for explaining the thinning in this area needs a little closer examination before it is rejected. It seems like this would be one of the "primary alternative" explanations for the observed thinning . Advection of

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an anomaly is only one potential mechanism, divergence of ice another. It seems like the authors have the tools at hand to do this more thoroughly given that the rejection of alternate hypotheses is a significant element in making the case for the Fram-Strait inflow, icemelt connection. (will be resolved after inclusion of backtrajectories and correlation of AW with sea ice).

Backtrajectories added, also see our response above.

253, 25 Physical processes are quite different in the Barents Sea. Is this a new section? I was reading prior discussion as applying to the whole study area including the "Barents". Please organize so it is clear that what are differences and commonalities between different sectors of the study area.

We do not discuss Barents Sea in this paper. Moved this general discussion closer to the end to avoid the disruption of the argument.

253,26: : : and there is a general lag of about a year or two between anomalies.. Is this statement based on data presented here or does this refer to a different study (Arthun?). Please clarify.

Yes – based on Årthun et al.

254, 1 two pairs of years have very similar wind patterns I assume you are referring to Fig 3 and the ice-motion patterns here? What are the similarities and differences? I don't know what I should be looking for. I see similarities in speed and direction in different areas of the plots.

A plot with back trajectories has been added.

255, 6 Sea ice anomalies in the central Arctic Ocean could probably be explained: : :. I don't understand why this statement is here? Are you trying to contrast the changes in ice thickness here with changes in ice thickness elsewhere? I think this is an important issue with this study. If you select a particular area and try to explain the changes in ice thickness there, then I do think it is important to make clear why an explanation

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of thickness changes in this area can stand on its own. This is particularly so when the presented evidence linking the thinning and the inflow has substantial gaps. An uniformed look at the map of thickness changes between 2004 and 2008 raises the obvious question: “so if it is inflow here, what’s causing changes of similar or greater magnitude elsewhere”. I don’t think a “beyond the scope of the study” sits well with the significant claim of the overall result and some arguments are needed why it is meaningful to look at this area in isolation.

Again, our main argument is – the ice in our area of interest originates from areas with thicker ice (back trajectories), therefore thinner ice can only be explained by the local processes.

254, 10 between 2004 and 2008 anomalies in sea ice thickness occurred in the same area and increased in strength. Which area, what’s the reference period and where do I see an increase in

strength of the anomaly?

“Which area” – see Fig.3. “what is the reference period?” – 2004-2008; “Where do I see an increase in strength of the anomaly?” – see Fig.3

254,14 for our estimates we assume FY ice: : : I cannot follow the below argument and what is shown in Figure 6. Why is a FY thickness assumed if this is measured from ICESat? What is the reference period and area for calculating the anomalies and how is the 130 kmˆ3 volume change computed. What does the 130 kmˆ3 value mean, it is left dangling.

Rephrased the paragraph.

255, 1 The first sentence in the Discussion reads as if the “pre-thinning” of the ice by Pacific Water has contributed to the thinning by AW, that seems to contradict the assertion that there is no upstream ice thickness anomaly and that the thinning is local only?

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We do not say anything about local ice “pre-thinning” in this sentence. We do not see any contradiction between mentioning “overall ice thinning” and the rest of the Discussion.

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