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The Cryosphere Discussions

Interactive comment on "North Atlantic warming and declining volume of arctic sea ice" *by* V. A. Alexeev et al.

Anonymous Referee #2

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The paper attempts to establish a link between increased northward transport of heat in the ocean through Fram Strait and observed changes in ice thickness in a sector of the Arctic that may be influenced by such changes. This is an important and timely topic as it may help with the attribution of sea ice losses to physical processes that link sea ice loss to anthropogenic and natural variability (hence my excellent rating for significance conditional closing the gaps). The paper arrives at the conclusion that as much as 20% of the arctic-wide sea ice volume loss that occurred between 2004 and 2008 maybe attributable to the increased influx of heat in the Atlantic Water (AW) through Fram Strait. Presented as such, I think this is a fairly significant claim that calls for substantial evidence. Although the authors provide some evidence that suggest a plausible connection, I don't think the paper in its current form provides sufficiently solid evidence that would justify the presented claims. Part of of this impression maybe

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due to the somewhat disorganized presentation and detail of arguments, other arguments seem to have obvious gaps that can and should be filled before a connection between the ice loss and the northward ocean heat transport can be established as "clear". The argument begins by juxtaposing a measured warming in Fram Strait AW core temperatures from 1960 through the present, with ice thickness changes from the ICESat satellite that occurred between 2004 and 2008. Sample ocean transects north of Svalbard for 2004, 2006, and 2008 show relatively warm water near the surface showing a potential connection of AW water to the sea ice. Given that the data record isn't complete enough to establish a clear causative connection between the influx and the ice melt, the authors then attempt to shore up their argument by examining and rejecting alternative hypotheses. This seems like a reasonable approach, but the presentation and execution falls short of making this approach convincing. I believe substantial work to better explain the theory of the mechanisms and to shore up their argument by additional analysis is needed before this paper should be published. Alternatively a scaling back of the presented claims to what is actually supported by the presented evidence might be another approach. 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. 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. 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? 4)Clarify reference periods and areas when anomalies are considered. Be clearer, maybe separate sub-headings,

TCD
7, C53–C58, 2013

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. Specific criticisms are outlined below in sequence. 246, 15. . . 150-200 kmˆ3 . . . I can't find a place in the paper where this number is derived. I see 130 kmˆ3 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. 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. 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. 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? 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. 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. 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. 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 re-

TCD

7, C53–C58, 2013

ject the alternate hypotheses in my view. 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. 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.

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

251, 14 clearly driven: I don't see this clearly. The connection is plausible, but the impact on ice melt hasn't been demonstrated 251, 26 I cannot follow what's being done in the next two paragraphs.. 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. 253,6 a ocean heat flux of 100 Wm-2 ... is therefore a realistic value? I can't follow this? 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. 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. 252,21 warmer air, water vapor

TCD
7, C53–C58, 2013

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 ERA-Interim product would be the superior product. Explain if doesn't matter. 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. 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. 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.. 253, 14.. is clearly related to more open water there. . . plausible, certainly, but "clearly related". . . evidence for this needs to be provided. 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 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, ice-melt connection. 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. 253,26. . . and there

TCD

7, C53–C58, 2013

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. 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. 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 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. 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? 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. 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?

Interactive comment on The Cryosphere Discuss., 7, 245, 2013.