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Interactive comment on "Increased glacier runoff enhances the penetration of warm Atlantic water into a large Greenland fjord" *by* A. J. Sole et al.

Anonymous Referee #2

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This study uses a numerical ocean model to investigate the circulation in Kangerdlugssuaq Fjord (KF), East Greenland. Its goal is to investigate if ocean variability played a role in the retreat and speedup of Kangerdlugssuaq Glacier (KG) in the mid 2000s. The model used is a three dimensional fjord forced by tides, local winds, property changes at the mouth and glacier runoff. It is initialized with hydrographic data collected in 1993.

The main thrust of the paper is to investigate the changes in heat transport associated with various forcings and especially with the (discrete) change in ocean conditions from those observed in 1993 to those observed in 2004. The conclusion, stated in the title, is that increased glacier runoff enhances the penetration of warm Atlantic water in KF and, therefore, of the submarine melt rate of KG.

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Interactive Discussion
Discussion Paper

The model runs presented are interesting and relevant to understanding the circulation in a Greenland fjord, but there are major flaws in the analysis presented and in the conclusions drawn from them (which I describe below). Because of this, I do not recommend publication of this paper. I do believe that these model runs could be used to address a different set of questions and that an appropriate analysis would increase our understanding of the dynamics of glacial fjords. I make some recommendations to this regard below after detailing the problems with this paper.

There are two major problems with this study:

1) It attempts to address a question which is beyond the scope of the model set-up: 'What caused the change in properties in KF from 1993 conditions to 2004 and how did that impact KG?'

The logic of the authors has been to set up a fjord model in which the circulation is driven by glacier runoff, surface winds and tides and initialized it with 1993 conditions. Then they step-change the properties at the mouth to those observed in 2004 and watch the progressive advection of 2004 properties into the fjord. Since glacier runoff is the main driver of the fjord circulation, they then conclude that the change from 1993 to 2004 properties is due to glacier runoff.

This conclusion is unsupported by the evidence presented. To conclude that the circulation driven by glacier runoff is the main driver of changes in KF (i.e. the intrusion of warmer AW) the authors would need to compare all (or at the very least the more likely) drivers of fjord/shelf exchange. Instead, their set-up effectively includes only a limited number of drivers (tides, surface winds and glacier runoff) - only one of which (glacier runoff) is actually capable of driving a significant fjord circulation. It is worth noting that the circulation driven by submarine melting of the glacier itself is also excluded. More importantly, they neglect the most likely driver of change in fjords such as KF - which are fjord/shelf gradients in density and thickness(see for example the recent review of fjord dynamics by Stigebrandt, 2012, and references therein). This is even more troubling given that in the nearby Sermilik Fjord, previous studies have shown these exchanges to dominate the renewal of fjord waters (Straneo et al. 2010).

The question which these model runs address is: 'how long does it take for KF waters to be renewed if the waters at the mouth change AND the exchange is governed only by the circulation driven by glacier runoff? This is an interesting question, but is not the question which the authors claim to be answering and its relevance to the observed change in KF from 1993 to 2004 is not substantiated by any discussion presented here.

2) The second, more concerning, flaw of this paper has to do with the interpretation of the heat transports and equating them with submarine melt rates (which is really the focus of the paper).

Estimating a submarine melt rate without a glacier representation of sorts (i.e. a heat sink that accounts for the amount of heat taken up by a glacier) makes no physical sense. Consider for example the fjord model run for a long enough time (with constant forcings and boundary conditions) so that it reaches steady state. Then the modeled heat transport would necessarily be zero (since heat has to be conserved in the model runs). Yet we definitely would not expect the submarine melt rate to be zero as long as the waters are above freezing temperatures near the glacier. Thus the modeled and the submarine melt rate are not equivalent.

The heat transport estimated in the model runs is, instead, related to changing the heat content of the fjord above the section they are estimated across. It is the heat transport associated with the progressive change in properties in the fjord model from the 1993 initial condition to the 2004 imposed condition at the mouth. This heat transport is going into warming the fjord, it is not going through the northern wall into the glacier. Again, it cannot be used as a measure of submarine melting. Very likey submarine melting will increase as the fjord warms up, but these thermodynamics are absent in this model. The modeled heat transport thus is different from that derived from observations by

Johnson et al. 2011 or Sutherland and Straneo 2012 who are implicitly assuming that the rate of change in heat content of the fjord is small on the time scales considered. To give another example of why the modeled heat transport cannot be equated to a submarine melt rate, consider the following scenario. If the model with the 2004 open boundary conditions is run for long enough, the modeled heat transport will decrease since the fjord has largely equilibrated to the 2004 properties – yet we expect the submarine melt rate to stay high since the water in the fjord remains warm.

Less Major Problems (but still problems):

1) What does the 'spun-up' circulation look like? Is there a circulation? If so why? It seems like in the absence of forcing, and with the fjord equilibrated to the shelf conditions the fjord circulation should be zero. If it is not zero, because there is mixing, then the authors must quantify and describe what this circulation is.

2) The paper makes unsubstantiated claims that glacier runoff is the dominant control on transport of water within the fjord. It is the dominant control amongst those explored in these model runs but it leaves it entirely open that other drivers (e.g. fjord/shelf gradients) may dominate if included in the simulations. Please do not draw conclusions on things you have not studied.

3) It is unclear what velocity is used to estimate the heat transports – the absolute modeled velocity or the modeled velocity with a barotropic component subtracted to impose a zero volume flux (as described in Johnson et al. 2011 which the authors cite). Their results suggest that it is the latter. Removing a barotropic component is incorrect given that there is a net volume flux through a section due to the glacier runoff. If glacier runoff is present and a barotropic flow is removed, this gives rise to an artificial positive heat transport. This seems to be the case with the 1993 runs (CF=0) and varying glacier runoff.

4) The data/model comparisons presented do not, in my opinion, support the conclusions drawn. First of all the velocity section observed by Autosub and the modeled

are different! In general, it makes little sense to compare a velocity snapshot with a modeled velocity where only slow varying processes (except for the tides) are included. Secondly, the CTD data comparisons are also misleading. All they show is that fjord conditions change from the 1993 conditions to the 2004 conditions once the open boundary conditions of 2004 are imposed. And that the speed at which they do depends on the magnitude of the fjord circulation. This is hardly surprising, since they cannot do anything else – and hardly evidence that the model is capturing the correct dynamics. Even a diffusive process would, eventually, have produced the same results.

Interesting Results from the Model Runs

What I think is interesting in this study are the different circulation regimes forced by the tides, the local winds and the glacier runoff. These model runs have revealed an interesting interaction between these different forcings which merits to be analyzed in more detail. The authors could quantify the relative role of these different forcings and examine their role in dominating the variability on different time scales. Effectively the circulation driven by the glacier runoff is a modified estuarine circulation. As such it must depend on mixing, yet there is little or no discussion of the role of mixing on the magnitude and character of the circulation (the only sensitivity discussion mostly focuses on melt rates which are not a good diagnostic of the circulation). I think these runs merit a more in-depth (but sound) analysis and this would really advance our understanding of how these fjords work.

Interactive comment on The Cryosphere Discuss., 6, 4861, 2012.