Answer to reviewers

The answers to the reviewers'comments are indicated in italics

Reviewer 1:

General comments

The manuscript by Bourgain et al. reports on several cruises from the Canada Basin from the summer-fall periods of 2008 and 2010. The main data used for this paper are from the 2008 and 2010 CHINARE cruises. Several other cruises combined with ITP data are used to complete the picture for the Canada Basin. This is one of the first times that the 2008 and 2010 data from the CHINARE expeditions are presented. The physical data are used to compare three features – the near surface temperature maximum, the Pacific and Atlantic waters and the freshwater content – and an attempt if made to put these data into context of the Arctic Ocean Oscillation. While it is intriguing to interpret the data in the context of a rapid shift in the phase of the Arctic Ocean Oscillation, 2008 and 2010 are just two points of data. It is inconclusive to interpret two years of data in the context of an interdecadal process. The choice of saying that all changes in the Canada Basin from 2008 to 2010, including changes to the Atlantic water, can be caused by an interdecadal oscillation is not convincing. Thus I do not believe that this paper can be published in its current form. Major revisions that include a different interpretation of the data are recommended. More precisely, I recommend that the authors focus on one storyline at a time and interpret this with other data. For example, the authors could focus on quantifying the sea ice concentration in conjunction with the near-surface temperature maximum or they could examine the Pacific waters with the nutrient data.

The objective of the paper is to analyze the variability of the upper Canadian basin water column between 2008 and 2010, at a time of a strong shift in the phase of the Arctic Oscillation. But we do not pretend that all the observed changes can be explained in terms of AO phase shift.

Specific comments

The paper is poorly cited. There are often statements with no reference (e.g. see the first paragraph of the introduction – how do you know that the largest sea ice changes occurred over the Canada Basin?) or the wrong references for the statement (e.g. Woodgate and Aagaard 2005 did not find that Pacific waters are nutrient rich). Several recent important Canada Basin papers are missing from the paper (e.g. Yamamoto---Kawai et al., JGR, 2009; Timmermans et al., JGR, 2011; Steele et al., JGR, 2011; Jackson et al., JGR, 2011; Mizobata and Shimada, DSRII, 2012; McLaughlin et al., JGR, 2009; Lique and Steele, JGR, 2012) and these papers can help with the interpretation of the results.

We agree with the reviewer that the first version of the paper was strongly lacking previous studies references. The new submission version contains many more references, with a particular emphasis on the introduction where the AO definition, index calculation and impacts were developed.

There are many acronyms that the reader is expected to follow. I would recommend adding a table of acronyms.

We added a table of acronyms at the end of the document and tried to limit as much as possible their use in the text.

The English used is confusing and often incorrect. While I understand the difficulties of not having English as a first language, the writing makes the manuscript difficult to read and interpret. I think that several of the points of the paper are lost in translation.

We tried to improve the English grammar of the text.

The authors use both the terms Canada Basin and Canadian Basin throughout the paper. Steele et al (2011) give a good description of the differences between Canada and Canadian Basin. This should be read and made clear in this manuscript.

In agreement with the advices of the reviewer, we followed Steele et al, 2011 definition of the Canadian and Canada basin to make the difference between the two clear (see section 2.1.) In this study, we focused on the [120W-180W] area of the Canadian basin.

The authors refer to IPY years several times in the paper but don't define what years those are.

We now defined in the abstract what IPY stands for. It is also indicated in the acronym table at the end of the document.

Introduction

The introduction is too short and is poorly cited. Although the emphasis of this paper is the Arctic Oscillation, no description of the Arctic Oscillation is given. A description of known interactions between the Arctic Oscillation and Canada Basin waters should be included. Figure 1 is presented as the Arctic Oscillation Index but no information is given on how this was calculated nor were any references provided.

As advised by the three reviewers, we strongly developed the introduction with a more extensive definition of the Arctic Oscillation, explanation of the index, its known impacts on sea ice and the Arctic Ocean, based on numerous previous studies. In addition, we described the atmospheric state of 2008 and 2010 based on sea level pressure and air temperature at 925hPa data obtained from NCAR/NCEP (see sections 2.2.1 and 3.1).

Data

Section 2.2 is missing a lot of information and is confusing. I understand that the emphasis is the CHINARE cruises but the authors still need to report more information on the other methods of data collection. For example, what types of CTDs were used on the LSSL and

Polarstern cruises? What is the temperature and salinity precision for the CTD and ITP data? What are the exact dates of the cruises? Information of the ITP data presented are wrong. ITP1 (line 20, page 2005) did not collect data in 2008 to 2010 – it functioned from 2005-2007. Very vague information is presented on ITP collection dates. The ITPs collect up to 4 profiles per day and several ITPs are present in the Canada Basin each year so I don't understand why only 59 profiles were presented in 2008 and 22 profiles in 2010 for the July to October time period. ITP data are invaluable in this region and could greatly augment the dataset.

We agree with the reviewers that more information was required for the data. For each campaign, we indicated the exact dates, the type of probe, the accuracy… (see section 2.2.3.) In addition, we included all profiles collected by ITPs between the 1st of July and the 31st of September, in the [120W 180W] longitude range of the Canadian basin (see section 2.2.4.). In the previous version of the basin, we only included 1 ITP profile on 10 ITP profiles collected, in order to stay focused on CHINARE cruises, but we agree that it was a bad idea as it obviously decreased the robustness of our observations.

New results from the NSTM

The authors use data from July to October to calculate NSTM properties throughout the Canada Basin. In general, cruises were later in 2010 than in 2008. Studies by both Jackson et al (2010) and Steele et al (2011) found that the NSTM cooled and deepened from the summer to the fall. Thus I don't think that you can say that the NSTM cooled and deepened from 2008 to 2010 without taking seasonal variability into consideration. Perhaps augmenting the CTD data with more ITP data will help solidify your argument.

In this new version of the paper, we now restricted our data set from the 1st of July to the 31st of September. But still, a seasonal variability inside this time period remains. So, we agree with the reviewer that the analysis about the NSTM variability must be handled carefully.

The authors compare their NSTM findings with sea ice studies published by Stroeve et al (2011). Unfortunately, Stroeve et al. did not examine sea ice concentrations in 2008. So lines 11-14 on page 2008 are untrue. Thus the statement 'that the sea ice concentration could be a major driver of the NSTM variability' (lines 26-27, page 2008) is unfounded. Perhaps the authors can include sea ice data in their analysis.

In the new version of the paper, we present average sea ice maps of summer 2008 and 2010 obtained from AMSR-E data (see section 2.2.2.) and we discuss the sea ice conditions of both summers (see section 3.2). We can now analyze the NSTM variability in the light of the sea ice extent variability (see section 4.1.2).

Pacific Waters new results

There is no description as to how the authors calculated the temperature and depths presented in figures 5-8. For example, was a constant salinity used to make the graphs or was the temperature maximum and minimum plotted? This needs to be discussed for all of the 4 water masses.

The index for the Summer Pacific water is now described in more details. We followed Bourgain and Gascard 2012 definition. The SPaW temperature maximum was first identified using a depth range, a salinity range and a temperature threshold. Then, the index was calculated as the difference between this shallow temperature maximum and the corresponding freezing point (see section 4.2.2.1).

For both the Pacific waters, the diffusion of heat that cools the Pacific Summer Water and warms the Pacific Winter Water needs to be considered. For example, Steele et al (2004) suggest that the temperature maximum and minimum disappears after several years of transit. Thus it is possible that warmer Pacific Summer Water is 'newer' (i.e. less heat lost) while warmer Pacific Winter Water is 'older' (i.e. more heat gained from Pacific Summer Water and Atlantic Water). Thus the different properties observed in 2008 and 2010 can't simply be discussed in the context of Arctic Oscillation.

We agree with the reviewers that the age of the Pacific waters must be taken into consideration, as discussed by Steele et al 2004. We included this aspect in our discussion, see section 4.2.2.2.: "Indeed, Steele et al (2004) suggest that the PaW temperature maximum and minimum disappear after several years of transit due to diffusion of heat."

Atlantic waters

Is there any evidence that Atlantic waters in the Canada Basin are influenced by Arctic Oscillation? I don't understand how the pulse---like event is part of the Arctic Oscillation story.

We removed the section about the Atlantic water as we agreed that if the Atlantic water was immediately impacted by the atmospheric forcing, it would be on the Northern Atlantic, where it is at the surface, and not in the area of investigation,in the Canadian basin.

Technical notes

Lines 10-11 on page 2002 – Neither the Pacific nor the Atlantic waters are advected into the deep Canada Basin. Deep Canada Basin water is normally thought of as the water below Atlantic water.

We rewrote the sentence and suppressed the reference to the Atlantic Water as we removed the section dealing with it: "the water masses advected from the Pacific Ocean into the Arctic Ocean" (see abstract)

The paragraph from lines 23 on page 2002 to line3 on page 2003 is unclear

We reformulated as follows: "The Pacific waters were also subjected to strong spatial and temporal variability between 2008 and 2010. In the Canada basin, both Summer and Winter

Pacific waters influence increased between 75°N and 80°N. This was more likely due to a strong recirculation within the Beaufort Gyre. In contrast, south of 75°N, the cooling and warming of the Summer and Winter Pacific waters respectively suggest that either the PaW were less present in 2010 than in 2008 in this region, and/or the PaW were older in 2010 than in 2008. In addition, in the vicinity of the Chukchi Sea, both Summer and Winter Pacific waters were significantly warmer in 2010 than in 2008 as a consequence of a general warming trend of the Pacific waters entering in the deep Arctic Ocean since 2008."

Reporting conductivity accuracy to represent salinity is confusing – why not report it in salinity units?

We now reports the accuracy of the measurement in salinity units (see sections 2.2.3 and 2.2.4) .

The sentence on lines 25-26 on page 2005 is confusing. I don't understand what raw gradients are.

We suppressed this sentence as it appeared useless and confusing.

Please be more specific about how you calculated the minimum radius for kriging (lines 5---8 on page 2006). Do you mean that you always used a radius of 300 km?

In the Kriging interpolation process, the radius was always taken as 300 km. See section 2.3.: "We chose the ten closest points within a 300 km radius to be the Kriging interpolation neighbourhood. The radius was determined by the range of the variogram (Krige, 1951 and Matheron, 1963). "

Some description as to why the estimation error due to kriging is so high in Figures 5-8 is needed. It is disconcerting to see errors that are greater than the observation.

Thanks to the additional ITPs data we included in the analysis, the Kriging interpolation errors strongly decreased, so we do not face this problem anymore.

Coachman and Barnes (1961) should be cited here because they were the first to discuss Pacific and Atlantic waters.

As advised, we included a reference to Coachman and Barnes in section 4: "Then, the Summer Pacific water (SPaW) is located at 50-80m depth, in the upper halocline, and finally, the Winter Pacific water (WPaW) is located in the middle halocline, above the thermocline, around 150 m depth (Coachman and Barnes, 1961)"

Lines 3-5 on page 2007 state that ice-free waters form the NSTM. This is not true – the central Canada Basin was not ice-free during their study. Likewise, lines 22-23 on page 2007 state that Perovich et al (2008) highlighted the important of the NSTM to the ice mass balance. This is not true – Jackson et al, GRL, 2012 found this.

In section 4.1.1., we reformulated the sentence as follows: "Jackson et al. (2010) have shown that the ice free upper ocean layer being in direct contact with the atmosphere and exposed to incoming solar radiation has the capacity to store heat."

In addition, we corrected to put the right reference about the importance of the NSTM for the sea ice mass balance: Jackson et al 2012.

The second paragraph of page 2009 is long, repetitive and confusing. Lines 23-24 on page 2009 – A discussion of the properties of the Summer Pacific waters on the Chukchi Sea shouldn't be confused with properties of the SPaW in the Canada Basin. The waters in the Chukchi Sea are further modified before it enters the Canada Basin. Perhaps the author's own 2012 GRL paper or the Jackson et al (2011) JGR give a better description of sPaW properties inside the Canada Basin.

We made an effort to shorten and clarify the paragraph about the Summer Pacific Waters in the Canada basin (see section 4.2.1.); "The PaW are strongly affected by seasonal processes. In winter, cooling and sea ice production release brines in the water column and contribute to a colder and saltier Pacific water mass than during summertime. As a consequence, this winter Pacific water (WPaW, sometimes referred to as the winter Bering Sea Water like in Coachman and Barnes, 1961) is characterized by a minimum of temperature at greater salinities and at greater depth than the summer Pacific water (SPaW), characterized by a temperature maximum at relatively low salinities (between 31psu and 33 psu). When entering the deep Arctic basin, the SPaW and WPaW influence the upper part and the middle part of the halocline respectively (McLaughlin et al., 2004). As a result, the "double halocline structure" (Shimada et al., 2005; Shi et al., 2005) characteristic of the Canadian basin, is different from the "cold halocline" encountered in the Eurasian basin (Aagaard et al., 1981). "

The freshwater content:

Line 5 says that Pacific waters and river runoff are the primary sources of freshwater in the Arctic. The authors forget about sea ice melt and I would recommend papers by Yamamoto-Kawai et al, JGR, 2009 and Guay et al, JGR, 2009.

We included the role of sea ice melt in the freshwater content variability and referred to Yamamoto-Kawai 2009 and Guay et al 2009. See section 4.4.1.: "The sources of freshwater in the Arctic Ocean are the Pacific waters and the river runoff and, to a lesser degree, the sea-ice meltwater and the net precipitation (Jones et al., 2008; McPhee et al., 2009), […] though Mackenzie river input was identified in the Canada basin (Yamamoto- Kawai et al., 2009), the Siberian river runoff is the largest fluvial component of this basin (Guay et al., 2009). In addition, the increasing role of sea ice meltwater was also identified in the Canada basin (Yamamoto- Kawai et al., 2009)."

Rudels et al, JGR, 1996, first published the freshwater content calculation on page 2013.

We reformulated the sentence as follows: "Following Rabe et al. (2011), we used the following definition to obtain the inventory of liquid freshwater H_{FW} *between the surface and the 34 isohaline depth: …" see section 4.4.2.*

Reviewer 2

The manuscript aims at comparing the state of the upper Canadian Basin in the Arctic Ocean in summer 2008 and 2010. Several specific features in the Canadian Basin are examined, namely the NSTM, the Summer and Winter Pacific Water, the Atlantic Water and the freshwater content. The authors are trying to put their results into the context of the Arctic Oscillation shift that occurred between 2008 and 2010. The study is mostly based on the data from the 2008 and 2010 CHINARE cruises, which have not been published before.

Although the publication of this new valuable dataset would represent an important contribution to the community, there are unfortunately many important issues that prevent the publication of the manuscript as it is now. I honestly do not believe that this could be fixed during the review process, so I would recommend rejection at this stage. I would recommend that the author take time to improve the analysis (maybe focus on one specific feature) and the redaction of the paper, and re-submit the manuscript afterwards.

My main objections are:

1) The authors claim that the oceanic changes observed between 2008 and 2010 are related to the shift of AO. The conclusions on this topic remain almost completely speculative, and in reality only the change of FW due to the change of atmospheric forcing is discussed here. From Proshutinsky and Johnson (1997) and subsequent papers by Proshutinsky, FW accumulation in the Beaufort Gyre has been linked with Ekman pumping in this region, and more generally with the AOO index. However, the AOO index and the AO index are not the same, and the accumulation of FW in the Canadian Basin depends probably more on the intensity and the position of the Beaufort High than the AO index. If the authors want to investigate the link between the FW content and the AO index, they should at least present a description of the atmospheric circulation in 2008 and 2010. Showing the time series of the AO is clearly not enough to describe the change of the atmospheric conditions.

On a related note:

-the AO index should be defined and described correctly if this is the focus of the paper. References are needed here.

-A review of the impact of the AO variations on the features (other than FW) examined here should be included. I don't really know why we would expect an impact of the AO on the AW temperature or on the NSTM. If it has been suggested in previous studies, they need to be cited here.

As advised by the three reviewers, we developed the introduction very much to describe better the Arctic Oscillation, how the Arctic Oscillation index is calculated and the known impacts of the Arctic Oscillation on the Arctic Ocean (see section 1). In addition, we introduced sections describing the atmospheric and sea ice state of the years 2008 and 2010 based on data obtained from NCEP/NCAR and AMSR-E data respectively (see sections 2.2.1, 2.2.2 and section 3). As a consequence, many references that were missing in the first version of this paper are now present in this new version.

2) The authors examine 4 features in details. However, none of them is fully investigated and most of the conclusions remain speculative (each of the paragraph closes by “it is more likely…”). Although it is a good idea to structure each paragraph with a “background” section and a “new results” section, I actually don’t see clearly the new results of the paper. I would recommend that the authors focus on one (or at least less than 4) feature, so they can deepen their analysis and propose robust physical mechanisms that rely on proof (and not on previous studies)

In this new version of the paper, we included more ITP data to strengthen our observations and improve the Kriging interpolation results. We also introduced many more references to previous studies to strengthen our analysis. Moreover, we suppressed the section about the Atlantic water to focus on the very upper part of the water column: the freshwater, the NSTM and the Pacific Waters.

3) The state of the art of the current literature is not properly reviewed and the results are not properly compared to previous studies in many places. For instance, NSTM has been investigated in several studies (Jackson et al. 2011, 2012, Steele et al. 2010, 2011), which should be cited and discussed here. Most of the conclusions of Section 3.1.2 are based on the study by Stroeve et al (2011). However, in this paper, the sea ice conditions in 2009-2010 are compared to a long-term climatology, without mention to the conditions in 2008. So the conclusions of this study should be used with more caution when the authors compare the conditions in 2008 and 2010. This is also true for the Section 3.4: Many important papers are missing here. For instance, were the first to examine the link between the atmospheric conditions and the FW content in the Beaufort Gyre. These are just examples of a more general problem. The authors need to make a real effort to correctly refer to the state of the art.

The new version of the paper contains many more references than the previous version that was severely lacking of it. This helps us to strengthen our analysis.

As far as the NSTM is concerned, we introduced references to Jackson et al., 2012 and Steele et al., 2011. We agreed with the reviewer that using Stroeve et al., 2011 sea ice maps was not appropriate to compare 2008 and 2010 sea ice conditions. This is why we inserted maps of average summer sea ice extent in 2008 and 2010 obtained from AMSR-E data to discuss consistently the NSTM variability.

In the new version of the paper, as far as the freshwater is concerned, we now refer to Yamamoto- Kawai et al., 2009; Guay et al., 2009; Timmermans et al. (2011); Proshutinsky and Johnson (1997, in the introduction), in addition to the references already cited in the first version of the paper.

3) The text is really not clearly written, understanding it is difficult in many places. The English is often incorrect. It would probably be helpful to have the manuscript proofread by a native English speaker.

We tried to improve the English grammar of the paper.

4) The figures are also hardly readable and need to be improved. In particular, the colorbar in Fig. 5, 6, 8 and 9 makes the quantities on the figures almost impossible to read. In the following, I will go through the manuscript from the beginning, noting other issues as they appear.

Introduction:

The first sentence requires a reference.

We included a reference to Shimada et al., 2006: "We propose to analyse the recent evolution of the upper ocean water masses in the Canadian basin, where the exchanges between the ocean and the atmosphere are particularly strong due to reduced Arctic summer sea ice cover (Shimada et al., 2006)."(see section 1)

As mentioned before, the AO needs to be defined. What do we expect an impact of the AO shift on these 4 specific features? A review of the literature is required here!

We included a definition of the Arctic Oscillation, an explanation about the Arctic Oscillation index and a state of the art of the impacts of the Arctic Oscillation in the Arctic Ocean. (see section 1)

Section 2.1: I don't clearly see the point of separating the regions like that. The authors want to investigate the basin scale conditions. I understand that the change may be different in the deep Arctic and on the shelves. I would suggest considering only one region (or only the deep Arctic and the shelves) in the text and in the Tables, as they do not provide much valuable information. The authors should also avoid the use of too many acronyms to improve the readability of the text.

We find necessary to separate the Canadian basin in five regions as the behavior of the features we are investigating changes a lot within the Canadian basin. By calculating the parameters inside each region, we reduce as much as possible the standard deviation.

We tried to reduce the use of acronyms as much as possible and introduced a table of the acronyms at the end of the document to help the reader.

Section 2.2: The data set (other than the CHINARE data) has to be presented here with more details.

We provided more details about the campaigns: type of CTD, accuracy, exact dates of the cruise… In addition, we now used all the ITPs data collected in the Canadian basin in summer 2008 and 2010. This increase of data strongly reduced the Kriging Interpolation errors. (see section 2.2)

Section 2.3: The last sentence acknowledges that the difference between 2008 and 2010 could be due in part to the seasonal change between summer and autumn. This needs to be discussed in details in the following section. In particular, this could have large implication for the NSTM that has been shown to vary seasonally. The effect of the seasonality is even harder to dismiss, as the dates when the data were collected are not even mentioned.

We commented about the potential impacts of the seasonal change on the NSTM variability, as the analysis is based on data collected between the 1st of July and the 31st of September. In addition, we added more references concerning the NSTM and displayed sea ice maps of summer 2008 and summer 2010 using data from AMSR-E.

Section 3 (page 2006) : References are needed here, as the different water masses have been defined by different authors.

In the new version of the paper, we included a reference to Coachman and Barnes (1961) following the advice of one of the reviewer (see section 4).

Section 3.1 : Many studies about the NSTM should be cited and discussed here. The study of Stroeve et al does not examine the sea ice conditions in 2008. If the authors want to examine the link between the NSTM and the sea ice concentration, they could include an analysis of the sea ice concentration here. The possible bias do to the seasonality of the NSTM needs to be discussed.

In the new version of the paper, we present average sea ice maps of summer 2008 and 2010 obtained from AMSR-E data (see section 2.2.1.) and we discuss the sea ice conditions of both summers (see section 3.2). We can now analyze the NSTM variability in the light of the sea ice extent variability (see section 4.1.2).

In this new version of the paper, we now restricted our data set from the 1st of July to the 31st of September. But still, a seasonal variability inside this time period remains. So, we agree with the reviewer that the analysis about the NSTM variability must be handled carefully and we included a comment about it in section 4.1.2.

Section 3.2: Again, the link between the observed changes of the Pacific Water and the AO are not discussed here (while it has been done in the literature)

We included references about the impact between Pacific Water and the AO, mainly in the introduction of the new version of the paper (see section 1).

Section 3.3: L. 17‑19 page 2012: where do these numbers come from? Part 3.3.1 is confusing, as the authors do not make the difference between the advection of the warm/cold pulses through the boundary current and what is happening in the interior of the Basin. What is the dynamical explanation of the observed cooling (warming/cooling by diffusion?) We do not expect the same mechanisms to occur on the shelves/slopes and in the interior, and the warming/cooling order of magnitude should be different as well. This needs to be taken into account in the discussion presented here. The influence of the AO shift on the AW (if any) is not discussed here, so this part does not really fit into the general story.

We removed the section about the Atlantic water as we agreed that if the Atlantic water was impacted immediately by the atmospheric forcing, it would be on the Northern Atlantic, where it is at the surface, and not in the area of investigation, in the Canadian basin.

Section 3.4: The current literature is poorly reviewed on this topic. The conclusions on the influence of the AO remain purely speculative. The accumulation/release of FW from Ekman pumping could be directly computed from the atmospheric fields.

We introduced more references about the freshwater content variability to strengthen our analysis (see section 4.4.)

Reviewer 3:

General comments:

The authors compare water-mass properties and freshwater content between 2008 and 2010 in the Canadian Basin. They argue shifts in the freshwater distribution that they claim are a result of a shift in the AO index. They also claim a cooler Near Surface Temperature Maximum in 2010, shifts in Pacific Water distribution and little change in the Atlantic Water layer. The authors introduce a lot of material that is only superficially described. As a consequence, it is difficult to discern new robust results that are different from what has already been published.

In this new version of the paper, we included more ITP data to strengthen our observations and improve the Kriging interpolation results. We also introduced many more references to previous studies to strengthen our analysis. Moreover, we suppressed the section about the Atlantic water to focus on the very upper part of the water column: the freshwater, the NSTM and the Pacific Waters.

The Arctic Oscillation needs to be defined and set in context with the Arctic Ocean Oscillation index (see Proshutinsky and Johnson, 1997). The introduction appears incomplete and needs many more citations and explanations of the physics of how the large-scale atmospheric forcing is well known to influence water-mass properties and Arctic wide freshwater distribution. See Proshutinsky et al., 2009, McPhee et al., 2009. How do the recent changes relate to the shift in large-scale forcing discussed by Timmermans et al., 2011? How do the maps of freshwater distribution differ from those already published?

As advised by the three reviewers, we developed the introduction very much to describe better the Arctic Oscillation, how the Arctic Oscillation index is calculated and the known impacts of the Arctic Oscillation on the Arctic Ocean (see section 1). In addition, we introduced sections describing the atmospheric and sea ice state of the years 2008 and 2010 based on data obtained from NCEP/NCAR and AMSR-E data respectively (see sections 2.2.1, 2.2.2 and section 3).As a consequence, many references that were missing in the first version of this paper are now present in this new version.

Specific comments:

Abstract: Ice-Tethered Profilers, not Platforms

We corrected it.

Data section 2.2 needs a lot of improvement. Why give detail on the Chinese cruises and not for the others? What does this mean: "(ITP-1 profile on 10-July to mid October 2008 and 2010)"? ITP 1 did not profile in 2008 - 2010.

We agree with the reviewers that more information was required for the data. For each campaign, we indicated the exact dates, the type of probe, the accuracy… (see section 2.2.3.) In addition, we included all profiles collected by ITPs between the 1st of July and the 31st of September, in the [120W 180W] longitude range of the Canadian basin (see section 2.2.4.). In the previous version of the basin, we only included 1 ITP profile on 10 ITP profiles collected, *in order to stay focused on CHINARE cruises, but we agree that it was a bad idea as it obviously decreased the robustness of our observations.*

Paragraph 10: "seasonal variability might intervene in the differences observed from one year to another": The authors never say how they rule this out as being the cause of perceived interannual variability in the NSTM.

In this new version of the paper, we restricted our summer time range to the 1st of July and the 1st of September. In section 4.1.2., we included the following comment about it: "One must consider the possibility for the observed NSTM variability to be affected by seasonal variability, in addition to differences between 2008 and 2010, as the observations were collected between the 1st of July and the 31st of September, which is a time period long enough to capture the cooling and deepening of the NSTM from summer to fall (Jackson et al., 2010; Steele et al., 2011)."

Paragraph 25: State how you define the NSTM.

We now included this information: "Following Jackson et al.'s (2010), we defined the NSTM as the temperature maximum that was nearest to the surface and above 40dbar, with a salinity less than 31psu and a temperature at least 0.2°C above the freezing temperature." (see section 4.1.2).

The authors state they use a definition of PWW to have temperatures below -1.4C, but Figure 7 shows temperatures above this value.

Thanks to the highest numbers of observations we used to perform the Kriging interpolation, we do not have this problem to face anymore, in the new version of the paper (see section 4.2.2.2. and figure 9).

There are many grammatical errors in this manuscript.

We tried to improve the English grammar of the paper.