

**Review of “Large-scale temperature and salinity changes in the upper Canadian Basin of the Arctic Ocean at a time of a drastic Arctic Oscillation inversion.” by Bourgain et al., submitted to Ocean Science.**

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.

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)

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, Proshutinsky and Johnson (1997) 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.

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.

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'll go through the manuscript from the beginning, noting other issues as they appear.

#### Introduction:

- the first sentence requires a reference.
- 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!

#### 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.

#### Section 2.2

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

#### 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.

#### Section 3 (page 2006)

References are needed here, as the different water masses have been defined by different authors.

#### Section 3.1

As mentioned before:

- 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

#### 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)

#### 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.

#### Section 3.4

As mentioned before:

- 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.