

## ***Interactive comment on “Bathymetry and oceanic flow structure at two deep passages crossing the Lomonosov Ridge” by Göran Björk et al.***

### **Anonymous Referee #1**

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#### GENERAL COMMENTS

This paper presents and discusses recent bathymetric data and hydrographic features in the Arctic region of the Lomonosov Ridge, which is characterized by complex dynamics. If on the one side, the topic should deserve publication thanks to the detailed bathymetric survey and the collection of CTD casts in a very (scientifically) interesting region, on the other side I cannot recommend the publication of the manuscript (ms) in its present form. Unfortunately, the paper still suffers from a number of weaknesses and major revisions are required. While “Abstract” and “introduction” are pretty well organized and clear, “methods”, “results”, and “discussion” need strong improvements. I have given some general and (not all) specific comments below that I hope will help the Authors to prepare a more robust version of the paper in the next future. In particular, the description of the thermohaline properties needs to be re-organized. Finally, I

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strongly recommend the Authors to review the English through a native English speaker before next submission.

SPECIFIC COMMENTS Hereafter, I will report some comments on each section of the ms.

#### Methods

This section misses many information: detailed period of the cruise (days, months), explanation about collection and use of silicate and dissolved oxygen data (maybe coming from discrete water samples and laboratory analyses?). Moreover, if you compare your 2014 data with previous ones, especially if the latter are taken 10 years before, you should justify your choice, being aware that the comparison of thermohaline properties taken in period much different can raise some criticisms. A minor comment: I suggest the Authors to write somewhere in the methods that they analyze thermohaline properties by using potential temperature, salinity and dissolved oxygen, and define those parameters and their symbols once and for all.

#### Results

In section 3.1 the Authors start describing the bathymetry of the region. I had to spend a time to compare figures 1 and 2: I would suggest the Authors to use the same criteria of orientation in panels of figure 2 (a, b) with respect to that in figure 1 (b). It would render easier for the reader to visualize regions and possible pathways of water masses in the study area discussed later within the ms.

Page 5: After line 20, Authors describe the hydrological properties. I suggest adding a phrase that could connect the first part of the section 3.1 with its second part.

Line 21: substitute “the section across the passage” with something like “the north-south hydrological section across the passage. . .”

Lines 27-29: Are you speaking about the surface warm core? If so, please move up this phrase within the text, where you are speaking about the surface layer, otherwise

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it is difficult to follow the description of the thermohaline properties.

Lines 31-32: explain why such properties of the bottom layer are “anomalous”. From figure 3, which this part of the text is referred to, the mentioned temperature increase at the very thin bottom layer is not visible. Moreover, cold and salt waters, hence very dense (why you do not add also potential density data?), are normally trapped in the bottom layer of a basin, and lower dissolved oxygen values confirm that they are also pretty old (i.e. not ventilated since a relatively long time).

Line 34: remove double ‘to be’.

Lines 35-37: please better explain this part and/or support with adequate references (e.g. Chelton et al 1997, JPO). How did you calculate the Rossby radius? Why did you not mention this calculation in the “methods”? Again, did you consider checking satellite images, sea level, or horizontal distribution of potential density to see if any eddies would be visible? It could support the discussion.

Page 6:

Lines 4-5: remove “matching the salinity and temperature data”. Perhaps, you could discuss the origin of this bottom water masses, and how they accumulated in this part of the ridge, remaining likely isolated from the rest of the water column.

Lines 6-7: please change this part with “Vertical profiles of  $\theta$  and S collected in the southern passage of the LR are compared with those collected in the Makarov Basin (Stn. 145 in fig. 2b) and Amundsen Basin (Stn. 148 in fig. ???) and shown in figure 4.”

Lines 7-19: this part need to be re-written to render it clearer. I was totally lost reading this part. Please write in more orderly manner about layers, water masses, possible pathways, explain why you chose two reference stations, and so on.

Lines 20-36: the same comment as above: re-write this part to be more clear. Authors start introducing silicate data without any previous explanation of them in the “methods”. Additionally, they suddenly refer to mooring data gathered in 1995-1996 but this

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part is not well inserted in the context.

Page 7:

Line 2: “upwelling” is a specific oceanographic process, in this case is it driven by what? Maybe Authors mean “upward displacement”

Line 5: “turbulent diffusion”? ok, but please justify or refer to appropriate bibliography.

Lines 8- end of the page: to be re-organized. Again, comparing 2014 thermohaline conditions with those of 1995-1996, after you have written that the variability is large in this region, does not make any sense, unless you justify this approach. Moreover, remove conclusions from this section.

Page 8:

Lines 1-6: move this part to the “methods”.

Lines 20...: the same comment used for the previous page. The description of the thermohaline properties distribution is confusing, and need heavy improvements, in terms of language used and organization of the text.

Discussion:

Based on the comments I have provided for the results, I could say that the discussion has to be revised accordingly to the future changes required for the “results”. However, I will provide here some comments: Page 10, Lines 34-35: how do you define the flow “largely barotropic”? Is it reported in literature?

Page 11, line 7: I do not think that “water streaming” is appropriate, please check it.

Page 11, Line 11: indicate “Gakkal Ridge” in figure 1.

In general, it seems to me that parts of the discussion could be moved to the introduction, while here the Authors should discuss their own data with more detail. Doing so, they could provide some nice conclusions (now they are not clear) on water masses

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distribution (as they did) and some speculations or hypotheses on the evolution of the thermohaline properties according with previous already published data.

FIGURES:

Figure 3: To use different color scales in each layer can be useful to see the variability within each of them, but can confuse the reader because it seems that (e.g.) intermediate and deep layers have different values while they are almost similar with the exception of the very bottom layer. Try to use the same scale in each layer.

Figure 4: respect always the same order,  $\theta$  first, S second, Dissolved oxygen third (if you want to show), and then  $\theta/S$  diagram. In general, figure is not clear, all profiles seem bold, and colors between st. 145 and 148 are not clearly distinguishable. Finally, for the exact location of the stations, it is better to refer to figure 2, not figure 1.

Figure 5: from this figure, it seems that silicate and dissolved oxygen data comes from discrete water samples. Why the Authors did not described this aspect in the "methods"?

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