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Interactive comment on "Discovery and characterization of submarine groundwater discharge in the Siberian Arctic seas: A case study in Buor-Khaya Gulf, Laptev Sea" *by* Alexander N. Charkin et al.

Anonymous Referee #2

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General comments: This paper is the first to provide direct evidence of submarine groundwater discharge in the Arctic, which is an important contribution to our understanding of the Arctic system and how it may respond to climate change. Because of this exciting new finding I recommend that this paper be published after revisions to improve the clarity of the discussion and methods.

Specific comments:

1. Introduction: The background on SGD in the Arctic is lacking, and expanding upon this will help place the importance of the current study in context. There are a few

other references that support the existence of groundwater discharge in regions of continuous permafrost based on thermal gradients (Deming et al., 1992), the mapping of springs (Kane et al., 2013), and modeling of permafrost extent taking into account freshwater inputs from SGD (Frederick and Buffett, 2015). Also, the year for Walvoord and Striegl should be 2007, not 2000.

2. p. 3 lines 24-27: Missing/incorrect references in discussion of previous studies of Ra in the Arctic: Kadko and Muench (2005) were the first to measure 224Ra in the Arctic but are not included in the list, Kadko and Aagaard (2009) did not report any short lived isotopes, and Smith et al. (2003) report 228Ra and 226Ra activities for the Beaufort Sea and central Arctic. Radium-228 activities are also reported in Trimble et al. (2004) and Cochran et al. (1995), although the main focus of these two papers is on Th and not Ra.

3. p. 5 line 21: Why were the samples not counted a third time to correct for 227Ac? If this contribution is assumed to be negligible this should be noted in the text. Clarify why total 223Ra is used instead of excess.

4. p. 7 line 38: There is no mention of how 226Ra or 228Ra are measured, but these long-lived isotopes show up later in the manuscript. The first mention of 226Ra is in the section 3.3, where it is stated that 222Rn has been corrected for ingrowth from 226Ra, but there is no explanation of how this is done. Radium-228 and 226Ra activities are also mentioned later in this section, but there is no explanation of how they are measured. If the 228Ra and 226Ra measurements were made it would be great if this data could be published, even if they long-lived isotopes are not the focus of this study!

5. p. 9 line 9: In the description of the river water endmember it is stated that the activities of 224Ra, 223Ra, and 222Rn are higher than those in seawater, but the average 224Ra in RW is less than that of SW.

6. p. 9 line 17: In the SW description it says that the 228Th/227Th ratio increases by

ingrowth. Should this say increases by decay instead of ingrowth? My understanding is that the ratio increasing because Th becomes adsorbed to the particles and then the 227Th decays faster than the 228Th while the particles are sitting in the bottom nepheloid layer.

7. p. 9: Section 3.4 could be better organized; it's a bit hard to follow the way it's written because the descriptions of the endmembers are mixed in with the interpretations of the data. It would be better if the endmember descriptions were first, and then the data were discussed in the context of the two figures (11a and 11b) separately. As is, there is really no discussion of figure 11b.

8. p. 10 line 28: Figure 12c is referenced, but I think this should be a reference to figure 12d? I recommend introducing this figure (12d) in section 3.4 instead of section 3.5.1 (make it a separate figure), because this helps in the interpretation/understanding of the endmember descriptions.

9. p.11 line 18: Was permafrost thaw considered as a source of Ra? The source of the high Ra is at the place of contact between the ice hummocks and bottom sediments, so if the ice hummocks are thawing this would be a logical place to have some runoff of the melted ice, which could be enriched in Ra.

10. It would be helpful to compare the magnitude of the discharge near Cape Muostakh to that near the Kharaulakh hydrogeological massif; this comparison might aid in the differentiation of the two discharge mechanisms.

11. Why is supplementary table 2 (which is incorrectly labeled as supplementary table 1) considered supplementary and not included in the main text? In my opinion if the wintertime data are included in the main text, the summertime data should be included as well.

Technical comments:

1. Figure 7: numbers need to be larger (can barely read contours, can't read colorbar

scales for salinity/density easily), map needs to be larger (can't read labels).

2. Figure 13: cryogenic squeezing out of brine is labeled as CSB in the caption but CSW in the figure. Recently frozen soil is labeled as RFS in the caption but RFP in the figure.

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