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Ocean Science Discussions

Interactive comment on "Surface waters properties in the Laptev and the East-Siberian Seas in summer 2018 from in situ and satellite data" *by* Anastasiia Tarasenko et al.

Anonymous Referee #1

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Overview.

This manuscript uses a variety of resources, including in situ measurements, satellite measurements and reanalysis products, to describe the evolution of the Laptev and East Siberian Seas in late summer of 2018. The manuscript begins with a correct statement: that the region of interest is understudied. I think that the manuscript should be publishable, but only after significant attention has been directed (1) at the introduction (for context) and conclusions (for the meaning of the results in context), and (2) at Ekman fluxes and Ekman pumpting. For the Ekman point, I believe the calculations to be invalid in shallow and coastal waters; see specific point 10 below. More on point (1)

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follows, because at present, I think that the manuscript reads more like a report than a paper.

The manuscript introduction has some weaknesses. It tells the reader some of what happens in the region, but it does not really say why the region is important, why it matters. If I were making a case as to why a reader should care about the region, I would be looking at freshwater fluxes, specifically the role of the Siberian shelves in modulating the inflow to the Arctic Ocean of the great Siberian rivers, either those that discharge directly into the Laptev and East Siberian Seas, or those west of Severnaya Zemlya, some of whose freshwater runoff enters the region through the Vilkitskiy Strait. The most recent (and very good) review of such issues is the Eddy Carmack paper (JGR 2016).

Also, if the region is understudied (and I agree that it is) then the introduction needs to describe whatever of importance has been published on the region. Item 4 below gives three references led by Tom Armitage. They are pan-Arctic remote sensing papers that have quite a bit to say about the Siberian Shelves, and they are not mentioned. Plus I could add Johnson & Polyakov (GRL 2001), Semiletov et al. (GRL 2005), Lenn et al. (GRL 2009 and JPO 2011), and the Lenn papers remind me that the manuscript talks about currents but does not mention tides, which are important in the shelf seas. There may be more papers, these are just a few that come to mind. I am not confident that the authors have thoroughly reviewed the literature.

Then concerning the final section called Discussion and conclusions; this section is really not much more than a brief restatement of the work reported in the preceding sections. If the introduction does not tell the reader why the region is important, then the conclusions cannot then tell the reader why the new results matter.

Specific comments are in order of occurrence.

1. The first paragraph on pp 1-2 describes the region. Reference should be added to the map of Figure 1; all locations in the text should be labelled, so please add text

"Severnaya Zemlya" to the map. Later you refer to Arkticheskiy Cape, add this as well.

2. In the same first paragraph, it is correctly stated that the region is little-studied, but I think there should be a sentence stating why the authors think the region is important. At the end of para. 1 on the top of p 2, add a sentence "The region is important because ...".

3. When talking about salinity, it is not appropriate to use "PSS". If using the new Absolute Salinity, then you can say parts per thousand, ppt, or use the "per mille" symbol. If not, then just "salinity of nn.n" or "nn.n in salinity" is correct.

4. Recent papers led by Armitage using Envisat, Cryosat and GRACE between 2003-2014 seem not to have been looked at: JGR 2016 is about sea surface height variability, with discussion of Siberian shelf seas; Cryosphere 2017 is about surface geostrophic circulation; GRL 2018 is about sea level & surface circulation response to the Arctic Oscillation. Some the material in these papers is directly relevant, and this omission should be corrected.

5. All figures with multiple panels, please label them a, b, c, etc. and refer to the panels as such in the manuscript text. All captions must state all plotted quantities and their units.

6. Section 3.1.2 on salinity, and cf SMOS text in 2.2.2. I doubt that the spatial resolution is as high as it appears in Figure 3. I understood SMOS to resolve at about 100 km, in 2.2.2 the authors mention sampling at 15 km resolution, but adjacent points are surely not independent. How does this affect their statistics?

7. Section 4 first & second lines, your temperature accuracy cannot be 1 m°C, so why are you quoting temperature values to three decimal places?

8. Figures 5 and 7, Hovmoller plots: you are inconsistent. Figure 5, longitude (x-axis), days (y-axis); figure 7, vice-versa. Pick one orientation and stick to it.

9. Figure 8, temperature sections. Improve your presentation, please. Viewing the

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PDF, about half of the figure is just black and any temperature structure is obscured. The simplest solution would be to plot contours in white – not black!

10. Section 4.1.2. Please improve your terminology. "Transport" is typically a volume transport, units mˆ3/s. In your eq. 1, stress (N/m2) divided by [density (kg/m3) * Coriolis (sˆ-1)] has units m2/s. These units appear in tiny (almost unreadable) notation in figure 9. This is neither a velocity nor a transport. Importantly, though: are your Ekman calculations valid in shallow water? Ekman's assumptions included (1) no boundaries (remote from coasts), and (2) deep water (typically >200 m). What is the Ekman layer depth? Is it not more likely that upwelling / downwelling are dominated by sea surface height changes in shallow water? For instance, a wind from the west will cause surface water movement to the right (the south) in the northern hemisphere. Water "piles up" against the coast, and that induces downwelling (at the coast): see papers by Steven Lentz, for example. Is this in accord with your calculated vertical velocities in figure 9? I would say not.

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