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Interactive comment on "Acoustic mapping of mixed layer depth" *by* Christian Stranne et al.

Anonymous Referee #1

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As a companion to the lead author's paper published (with some different co-authors) in Nature late last year (DOI:10.1038/s41598-017-15486-3) , Stranne and colleagues present what I would term a technical-demonstration paper showing how the depth of the ocean surface mixed layer may be sensed acoustically. The demonstration is set in the Arctic using data from two icebreaker cruises that sampled in both ice-covered and open-ocean regions at various vessel speeds. While the spin is largely positive, several limitations to the technique are discussed that will constrain where and when the approach will yield scientifically useful information.

Apologies for the cumbersome terminology, but I believe it is important to distinguish between the ocean surface "mixed layer" and the "mixing layer." I consider the latter to be the span that is actively being stirred vertically at the time of observation; it can be (and is frequently) thinner than the mixed layer whose base might mark the maximum depth of turbulent stirring in the past. My sense is that the acoustic technique

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presented by Stranne and colleagues preferentially identifies the base of this deeper, possibly remnant surface mixing layer (in part due to the typically larger vertical gradient at the mixed layer base and its greater depth - at least in the data sets presented that were acquired from a large-draft vessel). Either way, I believe it is important to recognize this distinction and discuss if/how each class of "mixed" layer might be observed acoustically. One reason for worrying about (weak) stratification within the surface mixed layer is its possible manifestation of restratification processes including submesoscale instabilities (see Timmermans et al., 2011, doi: 10.1175/JPO-D-11-0125.1). Indeed, restratification processes are just as important as the surface stress and buoyancy forcing cited in the paper's introduction (page 1, line 41) in controlling mixed layer depth.

The authors employ the de Boyer Montegut et al. (2004) protocol (at times modified to use a smaller temperature difference criterion) to estimate the depth of the mixed layer depth in CTD data used as ground truth for their acoustic scheme. Adoption of a technique based on temperature is a bit odd for Arctic data since at cold temperatures, density is so strongly controlled by salinity. While I doubt it would change the main conclusions of the paper, it might be worth trying the Holte and Talley (JAOTech, DOI: 10.1175/2009JTECHO543.1) algorithm, particularly for those cases where there was disagreement between the methods. I would also quibble with the exclusion of very shallow mixed layers in the analysis, though I certainly understand the constraints deriving from vessel draft and acoustic blanking period. In the summer Arctic sea ice zone, the upper ocean can be stratified all the way up to the ice-ocean interface. (Drainage of melt ponds is an important summertime stratification mechanism.) The authors show one such example in figure S1 but I worry such stratification is common throughout much of the Arctic in summer, and that observations from a deep-draft vessel will give biased results. Speaking of vessel draft, there is of course the strong possibility that ships disturb the near-surface stratification, introducing yet another source of error.

My final general comment concerns the interpretation of the acoustic observations, exemplified by the sentence on page 2 line 20: "The increased SNR of wideband echosounders have made it possible to map density stratification in the ocean." The authors don't actually invert their acoustic data to estimate the ocean density profile. Rather, it is my understanding that they equate regions of enhanced acoustic backscatter with regions of enhanced vertical density gradients, the one discussed here being the mixed layer base.

I continue with more specific comments/suggestions:

Page 1 line 32-34: I note that light is also a significant factor impacting phytoplankton growth, which can be impacted by MLD and residence time for phytoplankton near the air-sea interface.

Page 1 line 47: the term "temporal sampling frequency" could be confusing - I initially thought of the sampling rate of the CTD instruments, not the time between vertical profiles. Page 2 top: I note that the remote sensing observations from the GRACE satellite mission are indicative of more than surface ocean properties.

Page 2 paragraph starting with line 3: I found it curious that this brief history doesn't begin with echosounding to determine water depth. See http://oceanexplorer.noaa.gov/history/electronic/electronic.html

Figure 1 (and others): I found the quality of the figures in this pdf to be not as crisp as I like. I'm hoping this is just a consequence of the review copy that was made available to me and that the published document will be better (i.e., quality more like the similar figures in the lead author's recent Nature paper).

Page 3 line 21: The authors might wish to temper this phrase: "Together, the SWERUS-C3 and AO2016 expeditions spanned the breadth and depth of the Arctic Ocean..." No observations were obtained in the Canada Basin for example.

Page 4 line 38: "A CTD [profile] was collected..."

Page 5 line 35: this sentence has no real content. Much better to make a technical statement and cite a figure in support.

Figure 2 and those similar: Please give the location and date that these data were collected. In this caption and those similar, panel B should, in my opinion, say CTD profile, not profiles, or CTD-derived temperature and salinity profiles.

Page 6 line 11: "EK80 data [are] available "

Page 8 line 3: might be good to note the different criteria for MLD used by these previous authors.

In summary, I believe that after revision, this work will be suitable for publication in Ocean Science.

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