Anonymous Referee #2

This paper presents an interesting and concise account of an innovative acoustic method to detect with high spatial resolution the depth of the ocean mixed layer, or mixed layer depth (MLD), a quantity that is of interest for a number of practical applications in oceanography. It is shown, using acoustic mapping, in combination with CTD profiles, that reliable estimates of the MLD may be obtained using the former method. The main obstacles to reliable MLD estimates are very shallow MLDs (lower than 10 m), or the existence of excessive biological scatterers, which confuse the vertical distribution of the reflection coefficient, by introducing noise. The paper appears to be scientifically sound, and is clearly written, reporting novel results that are worthy of publication in Ocean Science. There are a few non-critical points (listed below) that I would like to see addressed before I can recommend acceptance. Therefore, at this point I recommend that the paper undergoes minor revisions.

Minor comments

1. Page 1, line 20: "These prerequisites [MLD well-defined and absence of biological scatterers] are often met in the open ocean". Given that the study focuses on the Arctic Ocean, can the authors be sure that this remark is of general applicability, and not limited to that ocean? If not, then the necessary cautions should be noted.

We do not claim that these prerequisites are always met, but that they are often met. It is widely recognized that productivity is generally higher in coastal waters than in the open ocean, which is also consistent with what we see in our data (there are of course exceptions, for instance along the equator due to upwelling). This notion is supported by the difference between the estimated average primary productivity in the world oceans (~50 g C m^{-2} $year^{-1}$) and the estimated average primary productivity in estuarine waters (~250 g C m^{-2} $year^{-1}$), a factor of five.

From "Phytoplankton primary production in the world's estuarine-coastal ecosystems J. E. Cloern, S. Q. Foster and A. E. Kleckner".

2. Page 1, lines 27-28: "generated by wind stress and buoyancy fluxes at the air-sea interface", and lines 41-42: "The MLD is controlled primarily by surface stress (exerted by wind or sea-ice), buoyancy fluxes (heating/cooling, ice melt/formation, or precipitation/ evaporation), and dissipation". In this picture, the effect of waves is missing. It has been established that surface waves, through their interaction with the wind stress and generation of Langmuir circulations, exert a decisive control on MLD growth (e.g. Thorpe, 2004, Ann. Rev. Fluid Mech.). This should be recognized.

The primary cause for wave generation is wind stress, so waves and Langmuir circulation can be thought of as integral in the statement "surface stress (exerted by wind or sea-ice)".

3. Page 2, line 26: "ensonified". This word is probably unfamiliar to the readership of Ocean Science. Consider providing its significance on its first mention.

We agree with the reviewer and have now added a clarification.

4. Page 3, Figure 1: This figure looks somewhat fuzzy (I am not sure if this only occurs in the version available for review, as that happens in some journals). The green dots (particularly on the yellow track), and especially the blue dots, mentioned in the caption, have very limited visibility. Consider using different colours with a better contrast withthe blue background.

Yes, this is a PDF compression problem. We guarantee that the figures will look nicer in the final version. We have also improved the contrast between the different colors.

5. Page 4, line 13: "attitude", and line 23: "match-filtered": again, this terminology may not be familiar to the readers (it is perhaps over-technical), so provide a clarification of its meaning the first time it appears in the text.

These terms are only to be found in the methods section. While we agree with the reviewer that they are technical, it is likely that only readers that are specifically interested in these technical aspects of our method would go through these details.

6. Page 4, line 35: "Demer et al.", and page 5, line 2: "Lurton & Leviandier". These parts of the citations should not appear between brackets, as the corresponding references are incorporated in sentences. Please correct.

Fixed

7. Page 6, caption of figure 2: "Vertical magenta lines". These lines are rather difficult to discern in the blue background. Consider improving this aspect.

In the high resolution version of the figure, the magenta lines are easily discernable.

8. Page 6, paragraph between lines 20 and 27: The authors note that the criterion for detecting the MLD using CTD of using a temperature variation threshold of 0.2 degrees failed in the Central Arctic Ocean. Can they advance a physical interpretation for this behaviour, i.e., why in the Central Arctic Ocean and not elsewhere?

The simple explanation is that the temperature gradient between the mixed layer and the water mass beneath it is generally smaller. This is now stated in the text. However, the reason for this difference is a more complicated matter that is well beyond the scope of the present manuscript.

9. Page 7, figure 3: The horizontal scale of panel b in this figure appears no to be similar to that of panel a, but is not indicated. Please add that information.

The scale on the x-axis is "CTD observations" with equal distance between each observation. This has now been added.

10. Page 7, table 2: "rmsd". Not much is said in the text about how this quantity is defined and how it differs from the standard deviations in the two columns to the left. Please add that information.

The root-mean-square deviation is referring to the deviation between the two methods. The standard deviation represents the variability of the MLD observed within each method. We have now clarified this in the text.

11. Page 8, lines 19-20: "The acoustic method enables the study of internal waves propagating on the layer interface at the base of the mixed layer". What might generate these waves? Is there a possibility that the MLD measurements could be contaminated by waves generated by the remote interaction between the ship and the density interface at the bottom of the mixed layer (often called pycnocline)? It would be a good idea to discuss this aspect, as it might affect the proposed method in general (although not necessarily in the examples presented here).

We agree with the reviewer that it is possible that part of what we see, in terms of internal waves, might be generated by the vessel. We now discuss this possibility in the text. This would be a general problem for many ship-based observations, including observations made with CTD and free falling microstructure probes. In this study we settle with the fact that we can observe internal waves.

12. Page 9, line 11: "splitting/merging of layers". Can the authors be a bit more specific about what physical processes might cause this splitting/merging?

This is actually an open question, see Stranne et al. 2017.

13. Page 10, figure 6: This figure is presented as an example of measurements contaminanted by biological scatterers, which makes it difficult (or even impossible) to reliably determine the MLD using the proposed acoustic method. However, in the reflection coefficient graph shown in figure 6c it is still possible to distinguish the MLD as the depth below which the reflection coefficient starts to have a large variability. I wonder whether it would be still possible to usefully determine the MLD by appropriately exploiting that property?

This might indeed be a possibility. A similar approach has already been established: the gradient criterion method, see for example the De Boyer et al. 2004 paper where they review different methods.

14. Page 10, line 12: "rosette". This word is not used elsewhere in the manuscript, so consider replacing it by another, more standard word.

This is, as far as we know, the established term for the steel or aluminum structure on which CTD sensors and bottles are mounted.

15. Page 10, line 28: "lower success rate in coastal areas". Could this also be related to the greater abundance of biological scatterers in those regions? If yes, please adda comment explaining this.

Fixed

16. There are a number of figures (S1-S5) referenced in the text (page 6, lines 24-25; page 7, lines 3 and 11-12; page 8, line 44; page 10, lines 20 and 34), but not included in the manuscript. Is this just a referencing problem, or are those figures really omitted, in which

case allusions to them would need to be removed, with some detriment to a few justifications in the text?

These figures are included in the Supplementary Information (hence the S in front of the figure number).