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Interactive comment on "Sea–ice and water dynamics and moonlight impact the acoustic backscatter diurnal signal over the eastern Beaufort Sea continental slope" *by* Igor A. Dmitrenko et al.

Anonymous Referee #2

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Review of the manuscript "Sea-ice and water dynamics and moonlight impact the acoustic backscatter diurnal signal over the eastern Beaufort Sea continental slope" by Dmitrenko et al.

This paper describes diurnal vertical migration of zooplankton and associated changes in acoustic backscatter intensity measured in the eastern Beaufort Sea in response to daily cycles in light illuminance. Undoubtedly, this study provides a valuable insight into how the polar ecosystem evolves in response to the light cycle and is of interest for polar biologists and oceanographers. However, the manuscript in its present form has

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many flaws and therefore needs major revision. In several places, the text is not well-structured and clearly written. The results and discussion are not well separated and include numerous repetitions of the same content in many places. I would recommend the authors revise their text to improve its clarity. Further, I provided my suggestions as to what improvements are needed before this paper may be accepted.

General comments:

The logical link between evaluation of zooplankton communities and DVM at the mooring in 2003-2005 looks weak. This section and further discussion are entirely based on biological samplings collected in 2016 and separated from the time of mooring observations by more than eleven years. This is a considerably long period. I am surprised that the authors push us to believe that nothing happened with zooplankton communities in the Beaufort Sea over this time. It is unlikely that this is true, especially if we all know about substantial changes taking place in almost all components of the Arctic climate system during this time. I'm not claiming that any new biological species appear at the mooring site even if it might be the case, but it's tough to believe, for instance, that the biomass of each identified class remains unchanged. This raises the question how the presented materials correspond to the conditions at the mooring in 2003? Moreover, zooplankton samples in 2016 were collected for one particular month and, thus, cannot be representative for the entire seasonal cycle, making this part even more speculative. The authors are entirely mute about all these uncertainties. I would suggest the authors either improve that part providing more arguments for why we should trust this analysis or completely remove it.

An additional concern arises about sea-ice data. The authors do not really have at hand an appropriate sea ice thickness record to examine its impact on annual changes of zooplankton DVM. Instead, for simulations, they used a mean seasonal cycle of ice thickness that cannot represent natural year-to-year variability at the CA13 mooring. Thus, the comparison of ice impact on the simulated illuminance in 2004 and 2005 looks questionable. The use of the complementary ICESat data set partially tones

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down this problem, but does not address it in details because these data are also constrained. In that situation, the option I suggest is to rely on available models like PIOMAS, for example, or other ocean reanalyses data, which assimilate ice concentrations and thicknesses measurements. In that case, the discussion of ice impact on DVM may be more straightforward. It is also not clear to me why the authors used two series (Melling et al. 2005 and Krishfield et al. 2014) of the mean seasonal cycle if none of them are from the mooring site and just illustrate typical evolution of sea ice in the region? What's changed if we remove one of them?

I seriously doubt that the presented analysis of the illuminance due to moonlight was carried out convincingly. Figures 6 and 7, where we should see that impact, are very messy and I personally would not say that they show us this relationship in a clear way. For instance, despite the cloudiness during event#6 being about the same low as for events#3 and #4, we do not see any response in MVBS at 28-m depth. Moreover, in Fig. 7 any pattern due to moon phases is not evident at all.

Specific comments:

L61: "228°38.176'E" Here and throughout the text I suggest using western longitude instead.

L64: "CTD (temperature-salinity-depth)". CTD stands for conductivity-temperature-depth.

L71: "...the RDI reports that the vertical velocity is more accurate than the horizontal velocity by at least a factor of two". Could the authors provide a reference for this statement?

L83-87: Please, specify where these data come from and what algorithm was used for ice data processing? I also wonder why the authors used spatially-averaged sea ice concentrations over a ∼200-km rectangle, not just observations at the site of the mooring. The later looks more logical for the purposes of the assessment of light

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transmission under specific conditions at the mooring. Could you comment, please?

L94: "...https://rkwok.jpl.nasa.gov/icesat/index.html ". The link provided doesn't exist.

L98-100: "Finally, we used (iv) satellite synthetic aperture radar (SAR) imagery acquired by Canadian RADARSAT over the mooring location 100 before the sea-ice breakup in 2004 and 2005 (Figure 5)". Please, provide a source for these images as well.

L104-105: "...at times as close as possible to local midnight, with 3 other stations sampled during daytime at times close to midday". Specify the largest time difference between the time of profiling and the local midnight and midday.

L127:134: The description of the radiative model is incomplete. It's unclear what was used to calculate illuminance at the top of snow layer. What exactly does the snow thickness series look like? How were ice concentrations and clouds utilized in these calculations? Does this model simulate light distribution in the water layer beneath the sea ice?

L136: "The diurnal signal variation is presented along the vertical axis of the actogram, while the long-term patterns of diurnal behavior ..." The meaning is not clear. Did the authors mean variations during a day-long period? What does a "diurnal behavior" mean?

L157-159: "Overall, satellite data show that during winter-spring 2005 sea-ice thickness over the mooring location exceeded that for 2004 by >1 m suggesting implications for the under-ice illuminance values". I wonder if this conclusion has any impact on the simulated illuminance.

L161: Likely, the "layer" is missing somewhere.

L194: "...diurnal signal variations". As I noted above, this term is unclear. In actograms presented in Figs. 6-7, changes or variations of diurnal signal are shown along the horizontal axis, not the vertical one. Meanwhile, values along the vertical axis indicate

the temporal changes for any specific day, which might be called diurnal cycle or signal, but not its variations. If that is correct, I would suggest the author rephrase this for clarity.

L202-206: Consider merging this paragraph with L45-50 to avoid unnecessary repetition.

L209-210: “Outside of the polar day, the sun illuminance is opposite to MVBS for the sub-surface layer, while at 108 m depth this relationship becomes positive”. An awkward sentence because it has meaning only for the diurnal changes, not for the illuminance and MVBS themselves. Please, rephrase.

L211-212: “In spring 2004, the modification of the MVBS diurnal pattern from the beginning of May corresponds to an increase of the midnight under-ice illuminance to >1 lux (Figures 6b and 6c).” I think we need a clarification of what “modification” means here. Is it the vanishing of diurnal pattern? Moreover, I doubt that we can trust this number for the under-ice illuminance if we take into account that sea ice thickness was reproduced by the mean annual cycle.

L216: According to the description, ICESat data were available only for one spring month (March) in 2004 and 2005. Assuming dynamical nature behind the ice thickness anomaly in 2005, you cannot easily extend this conclusion for the entire spring or specifically to May 2005.

L216-218: “In spring 2005, the midnight under-ice illuminance >1 lux was lagging that in 2004 by about one week (blue dotted curve in Figure 6b)”. What is the reason of this lag if the under-ice illuminance was simulated using the long-term mean seasonal cycle of sea-ice thickness reported by Melling et al. (2005)?

L218-219: “Note that for winter-spring 2005 the under-ice illuminance, shown in colour in Figure 6b, is overestimated by about a factor of 10 (not shown)”. I’m very confused by this statement. If this is true, why do the authors show us an incorrect pattern instead

of reliable values?

L223: “diurnal signal was enhanced”. What does the enhancement mean in regards to diurnal signal? Is it the increase of diurnal amplitude or what?

L224-225: “In contrast to the preceding and subsequent periods, no seasonal tendency in the duration of high/low MVBS was observed at this time.” The meaning of this is blurry. Could the author formulate this in a more clear way?

L247: “. . .with the MVBS diurnal rhythm in Figures 5d-5h.” Did the authors mean Fig. 6?

L269-270: “During winter, the full moon generates under-ice illuminance up to about 0.1 lux below the sea ice layer with a thickness of around 0.5 m (Figures 3 and 6b).” I cannot understand this. In winter, the sea ice thickness at the mooring site is substantially larger than 1.5 m, not 0.5 m as the authors wrote here. The only months when we see such a thin ice are from July through October, when we see no clear DVM signal.

L273-280: I doubt that this result is well justified. Particularly, I didn’t see any clear pattern in MVBS shown in Fig. 6c in response to the moonlight variability. It is assumed that we should see similar inclined straps as we see in Fig.6b, but this is not the case. For some periods (e.g., Nov 2003, Feb-March 2004) we didn’t see any response in MVBS at all. Thus, I suggest to find another way to present this analysis.

L298-299: “These deviations, however, can also be attributed to inclinations of the ADCP transducer due to high-velocity currents.” Note, that ADCP automatically corrects that inclinations. The reason might be just more dynamical (turbulent) state of the environment associated with larger currents.

L331-332: “moving MVBS upwards.” Please, rephrase because MVBS cannot move anywhere.

L337: “It seems that downwelling #3D is entirely dominated by the moon. . .” Do the authors claim that the Moon impacts downwelling somehow?

L376-377: “It appears that this MVBS anomaly is attributed to the eddy-entrained suspended particles commonly recorded in this area (O’Brien et al., 2011).” If the disruptions of diurnal cycle are attributed to higher concentration of suspended materials in the eddy, why do we see such an unusual pattern in vertical velocity around the core in Fig. 7? I mean very high positive anomalies of the vertical velocity during the day time at 68- and 88-m depths. And a more general question: the authors noted in Discussion that “zooplankton likely avoids enhanced water dynamics” (L539). In that regards, why we do not see vertical migration of both signs in layers above and below the eddy core where we have the strongest currents?

L378: “December/January 2002/2003 “. Check the period.

L403-404: “Our results show that DVM responds to (i) seasonality of the sunlight, (ii) moonlight, and (iii) seasonality of sea-ice cover that attenuates light transmission to the water column.” I intuitively agree with that statement, but I should emphasize that the authors partially fail to demonstrate DVM associated with sea ice changes and moonlight variations. See my other comments.

L445-446: “The inter-annual variability in estimated under-ice illuminance is entirely attributed to the sea-ice thickness.” How was this conclusion made if the mean annual cycle for sea ice thickness was used to simulate the under-ice illuminance? In Fig. 3, for example, I see no difference in ice draft among 2003/04 and 2004/05.

L446-448: “During the ice season, the mean cloud cover ($\sim$40%) showed insignificant interannual variability (Figure 6a); thus, the cloud cover was not taken into account.” This, again, raises the question about what was included in the model to simulate light illuminance.

L550-551: “It appears that upwelling, downwelling, and eddies disrupt DVM by generating a water layer with an enhanced gradient of horizontal velocity.” Could the author explain how “the wind-driven barotropic flow generated by upwelling and downwelling wind forcing” (L543) can enhance velocity gradients? If barotropic means

depth-uniform current, it cannot produce vertical gradients. Or do the authors mean lateral gradients?

L594 and Fig. 7: This paragraph is confusing. Why do the events of upwelling and downwelling (e.g., 9U and 10D, and many others) have the same positive sign for the vertical velocity within the entire water layer? If we assume that the response of zooplankton to those events is to avoid layers with enhanced water dynamics (l.601), we should see the opposite direction in zooplankton migration in the case of bottom-intensified downwelling and surface-intensified upwelling. However, following Fig.7, this is not the case. Keeping in mind my previous comment to L376-377, I would say that the presented materials on vertical velocities do not support the author's conclusion at all.

L604-611. Please, see my general comment regarding the analysis of zooplankton.

L621-622: "The ADCP data are available through the Polar Data Catalogue at https://www.polardata.ca/pdcsearch/, CCIN Reference #11653 (Gratton et al., unpublished)." This dataset is not available using the CCIN Reference provided.

Figure 7: I would suggest the author use a different color scheme for this figure to separate positive and negative vertical velocities in a clearer way. For example, using a white color at zero velocity may help.

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