Interactive comment on "Zooplankton diel vertical migration in the Corsica Channel (north-western Mediterranean Sea) detected by a moored ADCP" by Davide Guerra et al.
Anonymous Referee #1
Answers (*A*) to reviewer's comments (*R*) are written in *italics*.

General comments
The paper deals with the analysis of backscattered acoustic ADCP data in the Corsica Channel during a period of two and half years to provide understanding on zooplankton behavior and evidence of its vertical migration. The paper contains interesting analysis and findings, but it's lack in some parts and in some discussions. It seems to me that it has been written in a hurry, neglecting some aspects and argumentation on several items with the consequence of not being always clear and correct. Moreover, biological measurements are not really linked to other data. There are several main corrections to do or parts to explain. As the items treated in the paper are interesting, I recommend the publication after all the main following issues have been addresses.

Main points:
R: There is at least one other recent publication on this item in the Mediterranean Sea (page 3, lines 25---27), that is the 2018 paper by Ursella et al. published in Progress in Oceanography on the Southern Adriatic Sea.
A: Thank you, the reference has been added and briefly described. It was available online just a few days before our submission, thus we missed it

R: Potiris et al. 2018, and Pinot et Jansà 2001 also studied the link between DVM and lunar cycle (page 4, line3).
A: Added

R: At lines 12---18, page3, it is not totally clear what is referred to the whole Mediterranean Sea and what to the Ligurian (also the reference list at line 13 is mixed, but then you speak of the Mediterranean, with a parenthesis on the Ligurian). Please rewrite the sentence.
A: we rewrote the sentence

R: At page 4, Line 10 you write: "to determine how much zooplankton"; this sentence means that you are able through backscattering energy data to measure quantitatively how much zooplankton is present, that is not true, as you also mentioned few lines above. Please change.
A: We rephrased the sentence, which is now: "allow to know relative abundances of zooplankton present at a certain depth"

R: At line 12, page4, you write: "to identify the drivers": this is a final sentence. As the driving mechanisms of DVM are not totally understood, I would suggest a softer sentence: "to identify the possible drivers". The same at lines 18---21, page 9: the sentence it is very definitive/strong and should be softened and contextualized.
A: Done, we accepted the suggestion to use a softer statement. At p. 9 we deleted ''understand'' and put "improve knowledge about what might possibly drive''.

R: you speak of two general and widely accepted assumptions in zooplankton studies (page 5, lines 29---31), but this is not really true. In reality, the sentence found in Heywood 1996 has a slightly different meaning from the one in your text. He says:" For vertical velocities, the water upwelling or downwelling is usually small under general oceanic conditions, except during events such as internal waves... ". I think you should better explain why in your case you can consider the upwelling/downwelling negligible or change the sentence. Moreover, the second assumption is not generally true: in the case of strong phyto blooming, the layers interested by it, "produce" quite strong signal. The same happens in zones rich in particulate as it could be the layer near the bottom. Anyway, there is no reference for this second assumption. Please explain.

A: We have rephrased and added a reference study from which it results that the Corsica Channel does not belong to the group of upwelling/downwelling areas of the Mediterranean. The second is especially an assumption that we made for our work, but that is often done in previous studies, among which we decided to mention one. We also added a small remark on the fact that sound backscatter has also other causes, which we are not able to discriminate. This is the sense of an "assumption"

R: the sentence at page 6, Lines 1---2, is not a consequence of the previous one. Moreover, data of zooplankton biomass are not "obtained by the ADCP". Please rewrite the sentence.
A: The sentence is still valid, we have cancelled the term "therefore" and made some other changes. The sentence is now: "In general, information on zooplankton biomass and vertical motion inferred from ADCP data are more qualitative than quantitative".

R: The paragraph at page 6 from line 25 to line 31 is quite confusing and it should be rewritten. There are not---explained variables in the definition of the two slant range limits, and also the reference is inappropriate.
 Moreover, the sentence at lines 27---28 is quite twisted. In addition, which "values detected" (line 29) do you mean? As it is, sentence at lines 30---31 is not appropriate as you use eq. 2 to calculate R. Finally, as state by Deines 1999 and Bozzano et al. 2014, the lower limit for the slant range is defined as pi*Ro/4, in order to be used it in the formula of backscatter coefficient (Sv) and not as a general criteria of quality control. Therefore, I would move all this discussion in the following paragraph after definition of R, also for clarity.
A: We have rewritten this part and added the missing definition of two variables. We think that the explanation is now much clearer. We do not understand what is inappropriate about the reference, it is a technical manual for the instrument written by the manufacturer. The order in which equations are presented has also been changed.
Concerning the comment about the lower limit of the slant range, the reviewer is right, but in our case no data were rejected because of this, but only because some were exceeding the maximum threshold, which is indeed a general criteria of quality control. Therefore, for the sake of clarity, we split the text, leaving discussion about Rmax here and moving Rmin to the following paragraph for the calculation of Sv.

R: Do you really have percent good greater than 90% also for data during the day in the parts of the water column where zooplankton has migrated away?
A: All data where PG<90% were discarded, before analyzing them, so the answer is yes.

R: The paragraph 3.2 is nested and difficult to follow. Why don't you give formulas 3 and 4 when you mention them the first time at lines 14---15? Eq.2 and 3 are not correct, maybe typing error. Please re---write the paragraph. Moreover, why do you use the formula by Deines 1999 to calculate Sv instead of the upgraded/corrected one suggested by Gostiaux and van Haren 2010 (also in Bozzano et al. 2014)? Please explain.
A: Following a previous comment, we reorganized the whole part and moved formulas to the right place. Typos were corrected.
We did a choice of one of the methods available in literature, also more recent papers than Bozzano et al. still continue to use Deines approach. Also Potiris et al 2018 used Deines formula, and they had an experiment setup more similar to ours, than Bozzano et al. Our aim was not to compare methodologies. In addition, our dataset does not have low signal to noise ratios (<10), for which the method has been developed by Gostiaux and van Haren.

R: At line 18, page 9, you speak of biomass, but previously, at page 5, you said that you assume that the signal comes only from zooplankton. In many other parts of the text you use biomass in different meanings; this generates some misunderstanding on the word biomass. However, this item should be better explained in the whole text and/or define the terms at the beginning. Please explain and change accordingly.
A: Thank you for outlining this. We have added zooplanktonic to all "biomass" words where it was necessary.

R: lines from 21 to 28, page 9, are superfluous.
A: We cancelled them

R: Why do you speak of Deep Chlorophyll Maximum if it is seen in the surface layer (line 3 page 10)? The same in conclusions.
A: DCM is a widely accepted definition which refers to the region below the surface of water with the maximum concentration of chlorophyll. In the study area it is located between 20 and 100 meters, depending on the season. Sometimes it is also called subsurface Chlorophyll maximum, but more frequently DCM, regardless of depth. For clarity we moved this definition to section 3.3.

R: At line 19, page 10, you say that you use w and Sv "to characterize different migratory behaviors of different zooplanktonic migrator groups", but it doesn't seem to me that you perform this kind analysis, except saying that there are probably two different communities at surface and bottom. Moreover, the following sentence ("To this aim….") is very generic and should be rephrased and the concepts better explained.
A: Ok, we have canceled the part "to characterize…groups". The sentence "To this aim…" in our opinion explains in a straightforward way that without a proper calibration, we can use MVBS only as an indirect and qualitative indicator of zoopl. biomass. We have therefore left it, rephrasing it a bit: "Without the necessary net samples that would allow a proper calibration, MVBS is considered as an indirect and qualitative proxy of zooplanktonic biomass".

R: The lack of information you mention at page 10 line 27 concerns w and MVBS not biomass and migration, except as a consequence. It is quite confusing to a reader.
A: Ok, we replaced biomass and migration with MVBS and W. The consequence is obviously that nothing can be said about zooplanktonic biomass and migration in this layer.

R: At page 10, line 30, you speak of surface values, but you have just said that there is a lack of data in the surface layer. As this misunderstanding with the term "surface" is found quite often in the text, please fix it throughout the text.
A: OK, thanks, we replaced ''surface'' with ''in the upper part of investigated water column'' or "upper layer". Checked out also throughout the paper.

R: at line 31 page 10 you write "since MVBS is a proxy": since this is your assumption and not a general one, it should be changed to "since we use MVBS as a proxy"
A: Done

R: Why do you affirm that the behavior observed in MVBS (lines 8---9 page 11, fig.3b, surface layer) is consistent with twilight migrating organism? It is not consistent with the definition you give in the introduction.
The same is found at line 18 when speaking of intermediate layer, at line 24 page 13 and in conclusions. Please explain and/or change.
A: We removed the "twilight" part from the surface layer discussion. However, what we observe in the intermediate layer is consistent with the definition of twilight migration we gave in the introduction (upward motion right after sunrise can be due to twilight migrators and reverse migrators). In addition, we have cancelled the part at page 13 "During both periods, an upward motion is evident after sunrise, a feature that is characteristic of twilight and reverse migration." Conclusions has been rewritten accordingly

R: It seems to me that in the w plot (fig.3c) the persisting positive values are better seen in January---February than February---March (line 11 page 11).
A: There was a problem with the labels in Fig. 3b-f, they were misplaced, the correct ones were those of Fig. 3g. So February-March was correct.

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R: The sentence at line 22---23 of page 11 is not totally true (not for all periods daily vales are slightly higher…). And, are you sure there is no effect of the bottom (like resuspension or particulate) at this quote? please explain.

A: Actually, we analyzed several (this word has been added) profiles from transmissometers routinely mounted on our CTD-rosette system, and turbidity values at the depths above the ADCP were always very low. We corrected the sentence by adding "except during the zooplanktonic blooming period".

R: I am not so sure that in Fig.4a the MVBS has a peak in February –March that involves all the water column as you write at line 1 page 12. Maybe in March, but February is not very different from April, except at about 300m.

A: OK, we wrote only "March"

R: Are you sure that the reference of Pinot et Jansà 2001 at line 10 page 12 is correct? Their measurements reach 220m depth.

A: Yes, because besides the difference of depth, we as well hypothesize the possible presence of two communities (like Pinot and Jansà)

R: It would be clearer if you define what you mean by blooming period and no---blooming period, at the beginning of the discussion on the differences in MVBS between the periods (i.e. end of page 11). Moreover, it is not clear what do you mean with the definition of the blooming and no---blooming periods given at page 12 lines 19---22. How do you calculate the periods? Please explain.

A: The definition of the two periods has been moved to the part that was at the end of page 11 as you suggested.

R: At line 6 page 13 you mention the fact that the timing of the downward motion in the blooming period is later than in the no---blooming situation due to the later sunrise. But what about the upward motion that happens at the same time in the blooming and no--- blooming periods? And what is the timing of sunset in the blooming period: the time written in red in the figures? This is also related to what you write at lines 22---23: it would mean a different timing of reverse migration in the two periods, i.e. 4 hours and 2 hours after upward motion. Please explain.

A: We commented the upward motion already, it happens at the same time but is more intense during the blooming. Sunset and sunrise times varies during the blooming period and the non-blooming period since it is a period of about 6 months. In red is only written the timing that was found to be the most evident in W. We added this information on the figure caption. We can not be more precise because of the 2h sampling, and also because the figures are averages of long periods.

R: It is hard to understand your affirmation at lines 7---9 page 13, after the discussion just done: DVM is not just presence of more zooplankton in the water column (here again, are you using the term "biomass" instead of zooplankton?); moreover, the upward vertical velocities are stronger in the blooming period, but during the no---blooming period the downward velocities are stronger. What do you exactly mean with "DVM is intensified"? Please explain.

A: We have added "zooplanktonic" before "biomass" to be clearer. DVM is not the presence of more zooplankton and the comment in fact refers to intensified W values. We replaced the concept of intensified DVM with intensified active upward motion.

R: The affirmation ("During both periods…") at lines 23---24 page 13, is not so evident to me when looking at figure 4d and 4f: in the no---blooming period it is really weak and should be taken with caution taking into account errors; moreover, these values cover the entire daytime.

A: You are right, we have deleted the sentence

R: In order to calculate the FFT, did you interpolate linearly the time series between one deployment and the other? Are you sure that the peaks you find in Fig. 5a and 5b are related to physical phenomena and are not fictitious features? And what about the error bar? Because some of the peaks are really small. The 12---hour peak at 353m is quite evident in w power spectra (lines 32---33 page 13).

What do you mean with "taken singularly" at line 33 page 13?

Moreover, your discussion at the beginning of page 14 is not convincing me: the reverse migration should be masked by the nocturnal one if it happens exactly at the same time and it is weaker (the bins measure the average movement). Please explain it and give more evidence.

Also, the discussion on 4.75 and 8 hours peak (lines 8---10 page 14) is not convincing: the variability you discuss seems not to be a cyclic one with that period. Finally, the spectra of low pass data contain peaks that sometimes are not very evident, and as there are no error bars it is difficult to distinguish them from the surroundings. Please re---do all this part regarding power spectra.

A: We interpolated with the matlab function inpaint_nans which is based on a PDE that is assumed to apply in the domain of the artifact to be interpolated. Then the PDE is approximated using finite difference methods and is solved for the NaN elements in the array. This method did not produce any peaks in Fig. 5a and b. We added this information in section 3.5.

We specified at which periods and depths peaks are really small (added this information where it was missing).

We removed "taken singularly", the sentence should be clearer. The meaning was that if reverse migration occurs alone the peak would be at 24 h, the same is true for nocturnal migration. But if they occur both the resulting peaks are at both 24h and 12h. It is quite difficult to explain the 12h peak existence, but the most plausible explanation is that reverse and nocturnal occur both, even though not at the exact same time (which would be rather strange indeed). The other peaks are representative of some cyclic variability, although it is rather difficult to identify which kind of migration is responsible for it. We smooth out the text a bit to take into account that we don't know how much we can trust these peaks. Finally, the spectrum was computed with a straightforward FFT, without segmentation and overlapping. Especially when looking for the long periods, segmentation would not have allowed to detect them. This is why we could not compute the confidence intervals here. Also here we smooth out the text a bit to take into account that we don't know how much we can trust these peaks.

R: I do not understand what is the sense of table2 with the list of all the species, if this feature is not used for the discussion in relation to MVBS and w, and if it is just a snapshot of a summer situation. Also, the small discussion at page 14 is superfluous. The affirmation at lines 30---31 is quite strong and partially not true (evidences of the contrary are found in literature).

A: We decided to keep this table, to summarize the net sample findings, which not entirely are described in the text, even if they are just a snapshot, compare to the acoustic data these represent a sort of ground truth, which is important to account for (this remark has been added to the text). In addition, it could be a useful reference for future studies of the communities in this area. It shows that the characteristics of this community are mainly epipelagic, which are not strong vertical migrators, as is evident also from the acoustic data of august. It is not clear to us what the reviewer wants to criticize about L30-31, and to which references in the literature he is referring to.

R: It is not evident to me the descent during the day between 150 and 250m (fig.6b) as described at line 4 page 15. Moreover, at lines 6---7 page 15, you explain the migration from 100 to 300m citing two references, but you do not say whether you found these organisms in your sampling. You should use your data at least in this discussion Finally, the sentences at lines 9---12, page15, should be better explained: where do you see the zooplankton descent? Which behavior do you mean? A reference for it is needed. Here you use "biomass" for phytoplankton.

A: We have removed Fig. 6a and now the plot 6b should be more readable. We replaced with "low MVBS levels" instead of "descent". We have specified that the groups of organisms we hypothesized have although not been sampled, stressing that the sample, even if it is necessary ground-truth base, is just a snapshot of a summer situation during day.

Line 9-12 page 15: there are probably zooplanktonic organisms in the upper layer we do not see except during their descent when DCM deepens. We add the reference to Fig. 4a where this is visible. We added ''phytoplankton'' before "biomass ''.

R: From line 31 page15 to line 13 page 16, you try to explain the unexpected result (zero--- lag correlation) with different considerations. It is not clear to me why one of these arguments is the lack of data in the

very surface layer if the correlation is the surface layer is ok (by the way, which is the depth of the euphotic layer?). Moreover, why do you cite Warren et al 2004 if they found no correlation or negative one, contrary to your results? Finally, why the changes in the amount of zooplankton should be related to lateral current only in the bottom layer and not in the surface one where the correlation is as you expect? Maybe the MVBS is not always a good proxy for zooplankton biomass, in the sense that it can include other signals? Please, rewrite this part explaining better the concepts.

A: This part has been completely rearranged (some results change after correction on the computing method suggested by the reviewer#2). We think that now the part is clearer. We mentioned the fact that satellite data are just surface data because as we know from Fig. 2d the DCM can be as deep as 100 m and, in this case, the phytoplanktonic bloom might not be correctly sampled by satellites or its timing might be different to what is seen from satellites. The depth of the euphotic layer is approx. the lower limit of the DCM, Fig. 2d. Reference to Warren 2004 has been removed as well as the sentence containing it. We do however do not follow the reviewer's reasoning when he says that lateral currents influence only the bottom layer and that MVBS is representing also other signals. Lateral currents are able to influence the whole water column (and this is the sense of what is written in the ms). Nepheloid layers have already been excluded in the paper, based on transmissometer data analysis.

R: At page16 line you mention an analysis of the behavior of zooplankton in relation with oxygen concentration, but in reality, this kind of analysis is not performed.

A: Oxygenation has been described for different season, so yes an in depth analysis has not been performed, but it was part of the description of the water mass properties. This part on oxygen has been deleted from the conclusions.

R: Taking into consideration all the points above, please change conclusions.
A: Conclusions have been rewritten accordingly

Minor changes:

Comments	Corrections
Line 14: "Biomass evolution": maybe do you mean "biomass distribution"?	*We replace "evolution" with "temporal distribution".*
Line 20: cancel "near"	*Done.*
Line22: "others" is quite too general. Please rewrite the sentence.	*Replaced with "other factors, like lunar cycle and primary production, are taken in consideration"*
Page1: Line 26: "At dawn…." It seems a general feature, indeed it is just one type of migration. Please rewrite the paragraph.	*Replaced with "During nocturnal migration at dawn…".*
Line 31: you are speaking of twilight migration, aren't you? It is not clear.	*Replaced with "The typical descent of twilight migration that…".*
Page 2 Line 28: does phytoplankton perform vertical migration?	*Some of them do it, especially dinoflagellate. Phytoplankton plays a role in the vertical flows of matter / energy and the range of autotrophic organisms capable of prolonged direct vertical movements also involves flagellated phytoplankton*
Page 3: Line3: instead of "an ADCP" write "an upward---looking ADCP"	*Done.*
Line 15: cancel "are"	*Done, this part has been rewritten according to your comment #3*
Page 4: Line 5: please correct "by the depth of the depth of"	*Done*
Line 7: change "calibration" in "calibrate"	*Done.*
Page 5: Line 4 and 5: I think that the units are m/s and not cm/s.	*Yes you're right, thanks*

Page 5: Line 20: change "proportional to how fast particles move and it is used to infer the velocity" in "proportional to the velocity of the moving particles and it is used to infer the speed "	*Done.*
Line 23: change "how much sound reflection" in "how much of the sound reflected signal"	*Done.*
Lines 23---26: what you are saying is certainly true, but there is some confusion on the terms you use here (reflection and scatter) and above/below (back---scatter). Please, try to uniform the language.	*Back-scatter vs backscatter have been uniformed throughout the paper. The confusion between reflection and scattering is not evident to us, since they are two different physical processes. However, it is true that what is commonly called "backscatter" in the water column includes also those particles that reflect the sound wave, and not only those that scatter it.*
Page 9: Line 2: add ":" after "These parameters are"	*Done.*
Lines 2---10: the list of parameters would be more readable if a list number/letter is added (i.e. i) ii) etc.) or it is listed in bullets.	*We decided not to use a list, but keep a text.*
Line 4: cancel "here"	*Done.*
Line 8: cancel "here"	*Done.*
Lines 9---10: moon phases are obtained from where?	*We have added the source "(retrieved from https://aa.usno.navy.mil/data/docs/MoonPhase.php)"*
Page 10: Line 13: "Fig 2d---2e" should be "Fig.2e---2g"	*Done.*
Line 24: the acronym has already been defined at page 7.	*Deleted.*
Line 29: add "approximatively" before "between"	*Done.*
Line 30: change "whole" in "the greatest part of the";	*We replaced it with "most of the"*
Page 11: Line 3: "Less evident in fig.3a": I think that it is impossible to see the daily cycle in this panel.	*You're right and we removed this, changing this and the following sentence.*
Line 11: change "persisting" in "quite persisting".	*Done.*
Line 19: change "very high" in "quite high"	*Done.*
Line 21---22: cancel "which is below the depth of the ADCP."	*Done*
Line 24: change "is much lower" in "is hardly seen", also because of what you write few lines below at line 27 ("is not clearly correlated with sunlight etc…").	*Done.*
Line 29: change "from noon to sunset and" in "from noon to sunset in some periods and"	*Done.*
Lines 30---32: the sentence is redundant. Please rewrite it. Moreover, what are DVM parameters?	*We rewrote the sentence.*
Line 33: cancel "integrated over the whole investigated water column": the figs.3a and 4a show MVBSs that vary along the water column.	*Done, thanks*
Page 12: Line 14: cancel "which the ADCP data …..": it is a repetition	*Done.*
Page13: Line20: change "Fig 3b---3g" in "Fig3b---g"	*Corrected to ''in Fig.3b-g and Fig4c-f''.*
Line 20: what is "Fig 4---4f"?	*corrected*

Page 14: Line 9: maybe "Fig.4d"?	*Yes. Done, thanks*
Page16: Line 33: a reference for the last part of the sentence would be appreciated	*We modified the text a bit and added the reference Tarling et al., 2002*
Page17: Line 1: change "surface" with "upper"	*Done.*
Line6: change "daily" with "diurnal"	*Done.*
Line 20: maybe 2000 is 2001?	*Done.*
Figures:	
Fig. 3: in panels b→g numbers, letters and labels are unreadable. Also, the lines with times of sunset and sunrise are difficult to see.	*Corrected*
Fig 4c→f: numbers and letters are too small and units are missing.	*Corrected*
Fig. 5: units on the y axis are missing.	*Corrected*
Fig. 6 the moon, the sunset and the sunrise symbols are not visible. Fig 6b can be a bit larger and 6a smaller. The use of symbols for sunset and sunrise at the base of the plot makes the plot difficult to interpret.	*We have modified the sizes of the two plots, and used bigger symbols for the moon phase. We could not make the symbols of sunset and sunrise larger, because of lacking space. If one zooms into the pdf of the ms they result visible.*
In general: units are missing in various figures.	*Corrected*
Potiris et al. 2018: the reference is not complete	*Corrected*
Ringelberg 2009: the reference is not complete	*Corrected*