

Response to reviewer comments on Manuscript PROOCE-D-20-00163 (Seasonal cycle of zooplankton standing stock inferred from ADCP backscatter measurements in the eastern Arabian Sea BY Aparna et al.)

Since this manuscript has undergone several changes and has been reviewed by different reviewers, we provide below a brief history of the changes made over time in response to the reviewer comments.

1. Original version (PROOCE_2017_108_Original_V0, May 2017): The backscatter climatology was computed from 75 kHz ADCP data and the use of 153 kHz ADCP data was restricted, but necessary owing to its use in the backscatter-biomass regression. Data from 75 kHz and 153 kHz ADCPs were combined for the regression, which was restricted to the data from January 2014 owing to non-availability of an MPN. Analysis of seasonality of standing stock was restricted to climatology and the column-integrated backscatter or CIB was estimated by integrating from the surface to $D20$, the depth of the 20°C isotherm. The use of backscatter and CIB in preference to biomass and standing stock, respectively was motivated by the limitation of the biomass data, which were restricted to a single cruise off Mumbai.

2. Revision 1 (PROOCE_2017_108_Revision 1_V0, April 2018): In this revised version, we followed the suggestion of a reviewer to use the variance to substantiate our claim that the backscatter change was higher across $D20$ than at other depths. The day-night distinction was introduced in response to a comment and shown to have no impact on the result. In this version, the climatology computed from 153 kHz ADCP data, but data from both 75 kHz and 153 kHz ADCPs were still combined for the regression, which was still restricted to the data from January 2014 owing to non-availability of an MPN. In response to a comment that the analysis should not be restricted to the climatology, the analysis of seasonality of standing stock was based on the time series as well as climatology. The standing stock or CIB was still estimated as an integral from the surface to $D20$.

3. Revision 2 (PROOCE_2017_108_Revision 2_V0, July 2018): One of the criticisms of this version (or even the original version) was that the role of $D20$ (and therefore of the physical forcing) was presumed; the reviewer suggested that the relation of $D20$ to the vertical variation of backscatter be allowed to emerge naturally from the data. In response to this comment, the manuscript was rewritten to allow the role of physics to emerge naturally from the variation of backscatter rather than it being imposed on the backscatter variation. In order to permit this inference, the CIB was estimated as the integral from surface to 200 m and from the surface to $D20$. Data from both 75 kHz and 153 kHz ADCPs were still combined for the regression, which was restricted to the data from January 2014 owing to non-availability of an MPN. Correlations were used first in this version following comments from a reviewer, but the correlations were largely restricted to the climatological variables; in earlier versions, we had not shown the correlations because the correlation is a global measure and does not permit separation of variability across time scales, unlike the wavelet transform.

The original manuscript and these two revised versions seem to have been reviewed by the same reviewers. This manuscript had a sequel (Shankar et al, 2019), which summarised its results and built on them to extend the analysis to the fisheries of the eastern Arabian Sea; this fisheries manuscript was submitted in November 2017 and its revised version was submitted in July 2018.

4. Re-submission 1 (PROOCE_2018_274_Original_V0, December 2018): The manuscript rewritten to quantify the seasonal change in CIB, which was computed as the integral from surface to 200 m and from the surface to $D20$. Data from both 75 kHz and 153 kHz ADCPs were still combined for the regression, which was restricted to the data from January 2014 owing to non-availability of an MPN. In response to reviewer comments asking for correlations for the time series in addition to the climatology, extensive use of correlations was introduced in this version in order to show the relation between the backscatter contour defining the bottom of the upper ocean and the physico-chemical forcing.

This version of the manuscript was reviewed by a new reviewer and the main criticism concerned the combination of data from 75 and 153 kHz ADCPs.

5. Re-submission 2 (PROOCE-D-20-00163, August 2020): The reviewer for the version submitted in December 2018 was different and the major criticism concerned the combination of data from 75 and 153 kHz ADCPs; the reviewer, who was new, also had comments on the dropping of the constants in the backscatter equation and the use of a constant coefficient for sound absorption. In response, we revised the manuscript completely to use only the 153 kHz data. To do so, we had to wait for over a year and a half to ensure we had sufficient biomass data. The availability of an MPN (multiple-plankton net) from the October 2018 cruise onwards made possible the collection of zooplankton samples over multiple depth ranges during the cruises in 2018 and 2019; the sampling was also extended to the other two stations, permitting a more representative regression. The availability of an MPN allowed a statistically reliable regression and the replacement of CIB by standing stock. The delay of almost 20 months was owing to the need to build a reliable regression, necessitating the wait for an additional year's zooplankton data. In this version, we also showed that the dropping of the constants in the backscatter equation or the use of a constant value for the sound absorption coefficient had no impact on the estimation of biomass (and therefore, of standing stock) because the constants were eliminated in the regression and allowing for variation in the sound absorption coefficient had a negligible impact on the biomass. ***It is this version dated August 2020 that was reviewed by the two reviewers to whom we are now responding.***

In the meantime, the fisheries paper (Shankar et al, 2019) had been published (in January 2019), but we considered it necessary to retain the basic structure of this manuscript owing to this link. For example, the CIB estimate was retained in this version (August 2020) owing to its use in Shankar et al (2019). Most of the comments, including the comments on the use of CIB, stem from the attempt to keep this link. Hence, in response to the comments, we have made one significant change. We have cut the length of the manuscript significantly by eliminating the discussion of the physico-chemical forcing. These processes are already discussed in Shankar et al (2019) and our reason for including it in the last version (August 2020), which followed the publication of the fisheries paper, was merely because the published paper referred to this manuscript. Now that two years have passed since the publication of this paper, it serves little purpose to retain this material.

What, then, is the use of this manuscript? The new version is built around the time series of zooplankton biomass that can be assembled using the ADCP data. The value of this time series, over six years long, has been recognised by all reviewers, including those who reviewed the August 2020 version. We address below, in response to several comments from Reviewer #1, the question of what this manuscript is about and what is its significance.

Response to Reviewer #1

General comment: In this paper, seasonal variability of zooplankton biomasses is investigated at three sites in the eastern Arabian Sea. At the first, the authors obtained a regression equation between backscattering strengths and zooplankton biomasses. Then, by using the equation, temporal changes in zooplankton biomasses were estimated from the backscattering strength obtained by moored ADCP. Main aim of this study was to describe seasonal variability of zooplankton biomasses in the eastern Arabian Sea. And the authors concluded that the minimum and the maximum zooplankton biomasses were observed during the summer and the winter monsoons, respectively.

Comment 1: Environmental variability was not observed in this study. So, the authors discussed environmental forcing by using the monthly mean values of environmental parameters previously reported in the eastern Arabian Sea.

Comment 3: The authors didn't collect environmental data during the study period. It is one of the weak points in this study.

Reply: The reviewer's contention in these two comments is that this study invokes "environmental forcing by using the monthly mean values of environmental parameters *previously reported [emphasis ours]* in the eastern Arabian Sea and does not report environmental data collected by the authors." In other words, the reviewer contends that it is necessary to collect simultaneous environmental data to make sense of the zooplankton data.

We present two points in support of our approach.

First, the collection of data during cruises provides snapshots on (at best) a given day at a location. Consider just two variables, phytoplankton and zooplankton. The cruise samples will permit the estimate of the biomass of phytoplankton and zooplankton, apart from providing bioinformatics (species composition) for both variables. Yet, the measures obtained are but snapshots at a given instant of time. There is no measure of either variable even for the previous or next day, leave alone an estimate of the evolution of either variable over a span of time (say, a week or two) that may be deemed reasonable to infer a cause-effect relationship. How does one infer grazing from these snapshots? Indeed, apart from listing the species, what else can be inferred directly from these data?

Second, consider the paper of Jyothibabu et al (2010), which the reviewer contends (see Comment 2) has "already described the seasonal variability of zooplankton biomass", leading to the reviewer's conclusion that this manuscript presents nothing new. The environmental variables plotted in their figures are as follows. Their Figure 4 shows the averaged QuikSCAT winds during the summer and winter monsoon, a schematic of the circulation based on Shankar et al (2002), and the salinity distribution based on the World Ocean Atlas; Figure 6 shows spatial maps of the monthly chl-*a* based on SeaWiFS data and Figure 7 shows the temporal variation of the average chl-*a* during 1999–2004 based on these data for the regions north and south of 15N; Figures 8 and 9 show the temporal variation of monthly mean SST from MODIS data and wind speed from QikSCAT data during 1999–2004; Figure 10 shows the time-averaged SeaWiFS chl-*a* for the EAS during the sampling periods. Contrast the figures for these "environmental variables", all of which are based on satellite data, with the in situ zooplankton data presented in their Figures 11–13.

The point sought to be made by the reviewer in these two comments is countered by this use of satellite data, not simultaneously collected environmental data, by Jyothibabu et al. Indeed, all the arguments of Jyothibabu et al (and practically all such papers based on in situ zooplankton data) relies on environmental data "previously reported" or available from other, usually satellite, sources. If this approach is acceptable for in situ data, we do not see any reason why it should not be permitted in this manuscript!

Comment 2: The mooring ADCP data was collected for seven years. This large data set is a strength of this study. In fact, interesting long-term trends of zooplankton biomasses are recognized from their acoustic data. For example, increasing trend of zooplankton biomasses can be seen from 2013 to 2019 off Mumbai, the northernmost mooring site. Zooplankton biomasses seemed to be high in 2016-2017 and 2016 off Goa and off Kollan, respectively. In this study, however, estimated zooplankton biomasses were averaged monthly, and only seasonal variability was examined and discussed. Of course, seasonal variability of zooplankton seems to be an important topic to understand the Arabian Sea ecosystem. However, Jyothibabu et al. (2010) already described seasonal variability of zooplankton biomasses which was almost in a similar way as that of phytoplankton stock in the eastern Arabian Sea. And they also concluded that concept of the Arabian Sea paradox is not logically applicable for the eastern Arabian Sea. So, importance of this study is not clear for me. What is the key take home message in this study? Please show us importance/necessity of this study in the introduction.

Reply: The reviewer's opinion, as expressed in this comment, is that (1) a set of snapshots of some variables, including zooplankton from net sampling, based on data collected during the cruises reported by, say, Jyothibabu et al. (2010), are sufficient to map seasonal variability, and (2) the report of Jyothibabu et al. (2010) is all that is there to know about the seasonal cycle of zooplankton standing stock in the EAS, leading to the conclusion that this manuscript does not present anything new or useful.

Essentially, the reviewer contends that zooplankton science lies in the in situ net-based sampling; a corollary of this statement is that the bulk biomass or standing stock inferred from ADCP backscatter data from moorings are of no use. Since this comment is the most serious criticism on this manuscript, we respond to it at length.

To test this statement of the reviewer, it is useful to evaluate what can be inferred from the data collected using these two methods. Therefore, we have plotted the information on zooplankton standing stock available from both methods in Figure R1. The abscissa shows time, but the years referred to are abstract. The axis spans six years (year 1 to year 6) because the longest continuous record available covers six years (off Goa, 2015–2020; see Figure 3 in the new version); the data set is longer (eight years, 2013–2020) off Mumbai and Kollam. Since the reviewer has commented on the trend off Mumbai, we show the data for Mumbai and the years covered in the figure (see the top axis) are 2013–2018. The green curve shows the daily biomass estimated from the backscatter data off Mumbai and the red curve superimposed on it uses the 30-day running mean to smooth out the high-frequency fluctuations and show the monthly variation. This abstract axis also allows us to overlay the data from the cruises reported by Madhupratap et al. (1996) and Jyothibabu et al. (2010). The ordinate is also abstract and is not meant to facilitate inter-comparison between the three different sets of data from the three papers because each of these papers uses a different depth range for the vertical integral; the data from each study are, however, plotted to scale. The range of the ordinate is fixed on the basis of the range in the standing stock inferred from the backscatter. The seasons indicated by the vertical shaded bars are as defined in our manuscript.

The three cruises reported by Madhupratap et al. (1996) were conducted during 12 April to 12 May 1994 (spring inter-monsoon), 3 February to 4 March 1995 (winter monsoon), and 20 July to 12 August 1995 (summer monsoon); the abscissa extends from 1994–1999 for Madhupratap et al. (bottom axis labelled "M"). These three cruises were conducted over 16 months (from 12 April 1994 to 12 August 1995) and are plotted accordingly in the first two years on the abscissa; for these data, the first two years represent the calendar years 1994–1995. Note that with the cruise data, a single sample from a particular day is considered representative of that location not just for that season in that year (this is an implicit assumption because it takes roughly a month to sample the entire EAS), but for that season in a climatological sense. In each of these cruises, the data from a given location are collected during a single day at best, but are smeared first over the duration of the cruise (to permit contouring the data in space), then over the specific season in that year (as shown in by the solid horizontal lines for these

cruise data in Figure R1), and finally over that season in all years (as shown by the dashed horizontal lines).

Jyothibabu et al. (2010) reported data from cruises conducted during 1 December 1999 to 5 January 2000 (representative of the winter monsoon), 15 April to 8 May 2004 (representative of the spring inter-monsoon), and 6–? (end date not listed) June 2001 and 20 May to 6 June 2002 (two cruises representing the summer monsoon); the abscissa extends from 1999–2004 for Jyothibabu et al. (bottom axis labelled “J”). The summer-monsoon sampling had to be conducted over two years because the onset of the monsoon in June 2001 made sampling difficult and restricted the 2001 sampling to only the 8°N transect, with the cruise during 2002 covering the rest of the domain. Since the period from 1 January 1999 to 31 December 2004 covers five years, the data from these cruises are plotted at appropriate times on this abstract abscissa.

This figure puts into perspective what can be inferred from the two methods with respect to the variation of zooplankton standing stock. We evaluate the implications of the backscatter data and the cruise-based data at three time scales: interannual, seasonal, and intraseasonal.

Since the in situ sampling combines data from different years to infer a seasonal variation, it is clear that these data cannot be used to analyse interannual variability. In contrast, the continuous variation traced by the daily or monthly curves showing the standing stock exhibit variation from year to year. An example is the trend evident at this mooring location off Mumbai in the NEAS. This trend is recognised by the reviewer in this comment: *“In fact, interesting long-term trends of zooplankton biomasses are recognized from their acoustic data. For example, increasing trend of zooplankton biomasses can be seen from 2013 to 2019 off Mumbai, the northernmost mooring site.”* This potential to analyse interannual variability and, by corollary, the possible effect of climate change on zooplankton standing stock, is a strength of the ADCP-derived biomass time series.

The contour plots shown by Jyothibabu et al. for the mixed layer and thermocline layer in their Figure 11 are based on the data from three (strictly four) cruises conducted over a period exceeding four years. Though the cruise takes a month to cover the domain and the data for each location are collected on a different day and therefore gloss over intraseasonal variation (including possible phytoplankton blooms and their consequences for the zooplankton), these contours are considered representative of a season. In fact, the data for the summer monsoon were collected over two cruises, one each in 2001 and 2002, because the cruise in 2001 had to be abandoned after the 8°N transect owing to inclement weather. Surprisingly, they do not seem to have repeated the 8°N transect in 2002 even though doing so would have allowed them to cover the domain in a single year instead of splitting it across two years. They considered it acceptable to combine data from two different years in a single contour plot. Though this assumption is central to the creation of a climatology, it is unusual for a climatology to be based on data for just one year each in part of a domain. Any possible trend or other interannual variation is glossed over, just as is the intraseasonal variation, in producing and interpreting these maps. The authors of Jyothibabu et al. (2010) and so also the reviewer seem to consider this approach acceptable for inferring seasonal variation!

Furthermore, consider the years in which these data were collected. In Figure R2, which shows the annual all-India summer-monsoon rainfall or AISMR during 1871–2017, 2002 is marked with a black blob. 2002 was an El Niño year and, with the rainfall deficit exceeding 20%, it is one of the worst droughts on record in India. In contrast, 1999 and 2000 were not only normal-monsoon years, with the rainfall deficit within 10%, but were also La Nina years. 2004 was also a deficit year, but not associated with an ENSO event. The “seasonal” variation mapped by Jyothibabu et al. includes data in different seasons from these years and the data are lumped together in spite of each year being different from the others. This difference is ignored in the kind of maps prepared from cruise-based sampling, as done by Jyothibabu et al, but can be analysed in the ADCP-derived biomass time series.

An even more severe constraint of the cruise-based data presented by Jyothibabu et al. is that these data represent snapshots at each location, but these snapshots collected over a month in the EAS are combined to present a map documenting the spatial variation considered valid for a given season. In doing so, this approach ignores variability within a season: this variability ranges from time scales of a few days, as seen in the bursts recorded in the green curves depicting the daily standing-stock time series, to a month or more, as depicted in the multiple peaks recorded at periods of the order of ~8–60 days in the wavelet power spectrum for the standing stock (Figure R3). Indeed, a critical assumption underlying the month-long sampling typical of the EAS cruises is that this variability can be ignored. In ignoring this high-frequency variation, the traditional, ship-based zooplankton sampling loses the ability to infer the zooplankton response to a phytoplankton bloom; as is evident from the wavelet spectrum for SeaWiFS chl-*a* in Figure R3, such high-frequency variation is common in satellite chl-*a* data. Just as blooms cannot be mapped from a cruise, save in a serendipitous time-series observation, the zooplankton response to these blooms cannot be studied using the snapshots collected during these cruises. A measure of the response of the zooplankton biomass and standing stock to such blooms can, however, be studied using the ADCP-derived biomass time series. Though we have not extended our analysis to the variation within a season, it is clear from Figures R1 and R3 that the ADCP data have this potential.

Therefore, the potential of the ADCP-derived biomass data lies in its ability to facilitate an analysis of the processes involved in forcing the observed variability of zooplankton biomass and standing stock. In the absence of a time series, as is true of the snapshots reported using cruise data, it is not possible to analyse processes save in the *ad hoc* and heuristic fashion done by Jyothibabu et al. (and most other papers).

More important, the time series of biomass and standing stock can feed into a decision-making or forecasting system. In contrast, the cruise-based, bioinformatics-rich snapshots of zooplankton species and their abundance and biomass cannot be used to make decisions or feed into a forecasting system. Their value is restricted to documenting the biodiversity as seen on that day (and maybe for a few days before and after the sampling day).

In summary, as stated in the concluding remarks in the manuscript (section 5.4 in the original version and section 4.3 in the new version), the ADCPs can enable a revolution in the study of zooplankton variation just as the satellite did for phytoplankton. It is not possible to study phytoplankton variability today without invoking satellite data: that's why the SeaWiFS data, not in situ data, are used by Jyothibabu et al. as well! Likewise, it is possible to foresee a time when ADCP moorings are used as the basic tool to study the variability of zooplankton biomass and standing stock, with cruise data being used to generate the bioinformatics to complement these time-series data for biomass and standing stock.

An analogy can be drawn with the stock market. The Dow Jones Index was around 28500 in January 2020 and around 31000 in January 2021 (Figure R4), suggesting a year-on-year increase of just under 9%. Likewise, the Bombay-Stock-Exchange index, Sensex, rose from around 41000 in mid-February 2020 to over 50000 in mid-February 2021, an increase of just under 22%. Note, however, the collapse in the Dow Jones index in March 2020; this collapse, mirrored in stock exchanges round the world, was caused by the pandemic and the related lockdowns. If the market had been sampled just once in January or February 2020 and again a year later, it would still have shown the increase noted in these indices. Yet, this sampling, typical of the reports like Jyothibabu et al. (2010), would miss the once-in-a-century black-swan event that has roiled the world's economy, ruining, among other things, the research plans of most scientists the world over.

Therefore, a time series enables decisions that are not viable with traditional sampling and it is this potential of the ADCP backscatter to map the evolution of the zooplankton biomass over time that constitutes the significance or take-home message of this study (and so also of Jiang et al., 2007).

Comment 4: The authors discussed relationship between vertical distributions of zooplankton and surface mixed layer depths. But there were mismatches between monthly mean values of MLD and observed MLD using profiling floats. It suggests that there are problems to use monthly mean values of MLD. Same as the MLD, use of the monthly mean of other environmental properties seems to be problematic, I think.

Reply: This comment is no longer relevant owing to the elimination of the material on the physico-chemical forcing, but a reply follows nonetheless.

The climatology of Chatterjee et al (2012) is based on a much larger database of temperature and salinity profiles and, in particular, includes a large number of profiles from the Indian EEZ; these data are not included in the World Ocean Atlas. Therefore, this climatology is more reliable in the EAS, which lies within the Indian EEZ. In contrast, Argo profiles are not as commonly available near the continental slope because they are generally deployed farther in the open ocean to ensure a longer life for the floats. Hence, though the gridded data based on the Argo floats provides an estimate of the temperature and salinity variability for a grid cell in the region of interest, the value for a cell is based on sparse sampling in these grid cells. Nevertheless, since we had to discuss the variation of MLD for the duration of the ADCP record (owing to reviewer comments insisting on a discussion of the variability seen in the time series in addition to the climatology), we had no option but to use this data set based on Argo. Therefore, the “mismatch” between the MLD estimated from the climatology and the Argo floats is not surprising because the former includes much more data from the grid cells around the mooring locations. In contrast, Argo profiles from the continental slope are not common and the Argo gridded data sets tend to extrapolate onto the slope to provide a complete data set.

Furthermore, the MLD estimated for a particular year (floats) is not expected to match the climatological MLD: the former is, what is obtained in a given year, and the latter is the expectation.

We refrain from replying to the sweeping generalisation to “*the monthly mean of other environmental properties (seems to be problematic, I think)*” because the data presented are based on well-known data sets and the seasonal cycle presented, whether yearly or climatological, follows the norm. Unless concrete evidence is presented to show that these data are suspect (or the methods used are incorrect), a formal response is not possible.

Comment 5: Although the authors show us relationship between zooplankton biomasses and backscattering strengths, these data set were primarily collected from autumn. Zooplankton community might have changed seasonally and over years. These changes will likely have changed the backscattering signals. I know difficulty to collect the data set of acoustic and net samples simultaneously through the seasons and over years. But this is also a weak point of the present study, the authors should fully discuss on this problem as a limitation of this study.

Reply: The restriction of the calibration to zooplankton samples from October–November was discussed in section 5.3.2 (section 4.1.2 of the new version). This limitation was, and is, explicitly acknowledged and the need for cruises in other seasons was, and is, explicitly mentioned. It is not clear what can be added to this section because the point is made and the solution suggested. As noted by the reviewer in the above comment, the solution is obvious, but logistically tough. We hope that the value of the ADCP backscatter data for studying zooplankton attracts a larger community so that calibration data are collected more regularly, not merely on cruises that are restricted to servicing the moorings.

Comment 6: Although I felt that this paper was redundant, some of important information were not described and there were many mistakes in the methodological descriptions. The mooring depths of ADCPs were described as up to 150 m in the text, but this contradicted Table 1.

Reply: Line 202, to which this comment probably refers, reads as follows: “The depth of the ADCP was ~150 m.” Note the use of the symbol “~” to indicate “roughly”. The target depth of the top ADCP was 150 m, but a mooring does not land at the same depth particularly on the rapidly topography of the continental slope. In spite of the challenge posed by the topography of the slope, most deployments were within a reasonable range around the target depth. It is only off Kollam, where the changes in topography around the mooring location is more rapid, that the top ADCP for a couple of deployments landed at a depth exceeding 200 m. The actual depth, based on the pressure sensor in the ADCP, is listed in table 1. The use of “~150 m” does not contradict this table because the symbol “~” means “roughly”.

Comment 7: Detailed description of the multiple plankton net is needed. Estimation methods for filtering volume of water were not shown. What is an open-close-open zooplankton net at the line 214?

Reply: Line 214 read as follows: “The samples were collected using a 100 µm mesh size open-close-open zooplankton net hauled vertically from different depths.”

The close-open-close net was used to collect the samples off Mumbai during the 2014 cruise because an MPN (multiple-plankton net) was not available to us. The close-open-close net is a zooplankton net manufactured by General Oceanics Inc. for the collection of zooplankton from the desired water column depth. A unique double trip mechanical device is attached to the cable to which the zooplankton net is fastened in the closed position. The net is lowered to the desired depth and a messenger is dropped down the cable which will hit the double trip mechanical device to open the net. The net is hauled-up vertically to the desired depth and the second messenger is released which will hit the double trip mechanical device and enables the closure of net. Such a net enables the collection of zooplankton samples from the required water column depth.

In the new version, the zooplankton data from this 2014 cruise are no longer used and the calibration is done using samples collected from 2018 onwards. These samples were collected using an MPN, whose details and operation are provided in the new version of the manuscript (lines 213–218).

“The zooplankton samples were collected using a multiple-plankton net (MPN; Make: MultiNet; Hydro-Bios, Kiel, Germany) with a 100 µm mesh size and 0.5 m² mouth area. The net was hauled vertically from different depths and the volume of water filtered through the net was calculated by multiplying the sampling depth with the mouth area of the net (Fernandes and Ramaiah, 2013).”

Comment 8: In the description of the equation for the backscattering strength, the authors noted sound absorption was an empirical parameter but it was wrong. The authors also said that the sound absorption was a function of temperature and salinity. But the sound absorption defined by Francois and Garrison (1982) is a function of temperature, salinity, pH, and depth.

Reply: There are two parts to this comment. First, it is not clear why the reviewer objects to the phrase “empirical parameter”; the sound absorption coefficient is a function of environmental variables and the equation is based on empirical analysis.

The second part of the comment concerns the function itself. The following text is from Mullison (Backscatter Estimation Using Broadband Acoustic Doppler Current Profilers – Updated, ASCE Hydraulic Measurements & Experimental Methods Conference, Durham, NH, July 9-12, 2017; pp. 4-5).

Absorption. At TRDI the absorption coefficient for water used in performance models is based largely on principles described by Francois and Garrison (1982). Absorption depends strongly on frequency, and in the ocean is stronger than in fresh water. There is also a depth dependency that is neglected here as minor, but whose existence is noted for completeness. TRDI models specify typical ocean values as 5°C and 35 psu and typical fresh water values as 15°C and 0.01 psu. The absorption

coefficient, α , will change with temperature and salinity, and the actual temperature and salinity for each measurement should be used to calculate α for use in Eq. (3).” (Eq. (3) is the backscatter equation.)

Mullison points out that the depth dependency is minor. The dependence on depth is due to the compressibility of water, including seawater, but this effect is negligible for the depths considered here (not more than 200 m from the surface, implying a pressure change of ~20 bar). As an example, note that one uses only temperature, not potential temperature, which includes the effect of compression and therefore is important at large depths. The pH used for computing the coefficient is 8, but changing it has a negligible impact. Indeed, as shown in the earlier response (August 2020) and repeated below (but the figure numbers refer to this response), even using a constant value for the sound absorption coefficient yields the same results.

“The analysis, carried out for the ADCP off Mumbai during November 2014 to January 2016, shows that α varies over a small range, 0.060–0.069 dB/m, in the top ~150 m (Figure R5). To isolate the effect of the seasonal variation of α as a consequence of its seasonal variation, we picked two depths, 40 m (range of α is ~0.066–0.068 dB/m because the seasonal variation of temperature and salinity is low at this depth) and 120 m (range of α is ~0.0615–0.065 dB/m, higher than at ~40 m owing to the larger variation of temperature and salinity during a year), and checked how the backscatter varied with α . Figure R6 shows the variation of backscatter as a function of echo intensity and α ; both echo intensity and α are functions of time and this temporal variation during November 2014 to January 2016 at the ADCP mooring location off Mumbai is captured in this figure. Two distinct regimes appear in the figure because the α values cover different ranges at 40 m and 120 m, but at both depths, the value of backscatter is practically constant for a given echo intensity. In other words, as suggested by Heywood et al. (1991), the variation of backscatter due to α is negligible. Though we have presented this analysis only for the ADCP off Mumbai during November 2014 to January 2016, the same result is obtained off Mumbai for other years and for the ADCPs off Goa and Kollam as well. We also tested the impact of other methods of estimating α (Kinsler, 1982; Ainslie and McColm, 1998) and found that they yielded practically the same value of α as the method of Francois and Garrison (1982). In summary, though we have used the climatology of Chatterjee et al. (2012) and the method of Francois and Garrison (1982) to estimate α in the new version of the manuscript, the variation of α has a negligible impact on the backscatter values. Hence, it may be more prudent to use a constant α , as suggested by Heywood et al. (1991) and Deines (1999), because it is difficult to obtain temperature and salinity profiles commensurate with an ADCP backscatter time series.”

In summary, even a constant value can be used for the sound absorption coefficient because its variation has a negligible effect on the backscatter values. Yet, as noted earlier, we have used a variable coefficient from the August 2020 version onwards.

Comment 9: Although the authors cited transmit power as 23.8 W from Deines (1999), he reported not the transmit power but the $10\log_{10}(\text{transmit power})$.

Reply: Thank you for pointing out this error, which has been corrected in the new version. This change does not, however, have any impact on the estimate of biomass because this constant, like all other constants in the equation for backscatter, is eliminated during the regression. This point has been noted earlier in the preamble to this response.

Comment 10: I cannot follow from line 257 to 265. (These lines referred to the three constants in the square brackets in Eqn. (1) and their irrelevance for estimating biomass because the regression eliminates all such constants.)

Reply: The discussion of these constants was a consequence of the comment from the reviewer of the December 2018 version (Resubmission 1) that these terms cannot be ignored as doing so changes the sign of the backscatter. All constants in the backscatter are eliminated in the regression as their role is

restricted to shifting the backscatter by a bias; hence, these constants do not change the biomass. These lines were included to note the effect of these constants, which were ignored by Shankar et al (2019), but included in this manuscript in the August 2020 version.

Since the link to Shankar et al (2019) is no longer relevant (as noted in the preamble to this response), we have deleted all such material to avoid confusion. The new version is therefore independent of the fisheries paper of Shankar et al (2019).

Comment 11: The authors noted that previous studies using ADCP indicated dominance of copepods (such as *Oncaea*, *Oithona*, ...), macroplankton and ichthyoplankton. But we cannot obtain composition of acoustic scatterers from the ADCP backscattering. Species compositions of zooplankton are different spatially and temporally. So, I think that the text from line 271 to 279 is meaningless.

Reply: We are aware that the ADCP cannot yield information on the species composition and had stated as much in lines 806–810 (“Therefore, though the backscatter has the limitation in that it can be used only to infer the biomass and standing stock, but provides no information on the zooplankton species distribution, it can be a vital tool for linking physical processes to the second trophic level in the marine ecosystem.”).

The samples were analysed under a microscope to observe the species composition. We provided the list of zooplankton species seen in our samples merely to show that this list is similar to those reported earlier from the eastern Arabian Sea. It is easy to delete this list, but doing so would eliminate the only information provided in this paper about the zooplankton species. Hence, this species list based on the net sampling is retained in the new version.

Comment 12: In this paper, the standing stock of zooplankton was defined by vertical integration of zooplankton biomasses. And seasonal cycles of zooplankton biomasses and the standing stocks were described in different sections each other. I felt that separate descriptions for biomass and standing stock was meaningless. In addition, descriptions of seasonal cycle of zooplankton biomass (or standing stock) at each station and comparison of the seasonality among the three sites were not clear, I think.

Reply: The description of the seasonal cycle has been cut significantly in the new version. As noted earlier, no attempt is now made to link the observed variation of biomass or standing stock to the physico-chemical forcing.

Comment 13: The authors noted that the seasonal cycle of standing stock of zooplankton in the EAS is driven by the physico-chemical forcing. However, results of this study suggests that the physico-chemical properties influence not the standing stock but the distribution depths of zooplankton. Increasing or decreasing of zooplankton abundance/biomass seems to be influenced by other forcing, I think. What do you think about?

Reply: It is precisely this point — that the physico-chemical forcing decides the location of *D215* and therefore the vertical distribution of zooplankton biomass — that was made in this manuscript! The standing stock is just the vertical integral of the biomass. Since the biomass drops sharply from the upper ocean (the layer above *D215*) to the thermocline (the layer below *D215*), the variation of its vertical integral or the standing stock correlates well with that of *D215*. Off Kollam, where this decrease in biomass below *D215* is weaker than off Mumbai or Goa, the standing stock exhibits a weaker seasonal cycle. Hence, the physico-chemical forcing, by deciding the vertical distribution of zooplankton biomass, drives the seasonal cycle of zooplankton standing stock.

The key elements of this physico-chemical forcing of zooplankton standing stock or the vertical distribution of biomass is rather straightforward and we refer repeatedly to the literature to make the case. The vertical movement of the isotherms are associated with the seasonal reversal of the circulation

and the vertical movement of the isolines of oxygen (or the oxycline). The existence of the oxygen-minimum zone (OMZ) in the EAS leads to the decrease in zooplankton biomass with increasing depth. This vertical distribution, reported earlier in the literature based on in situ sampling, is clearly seen in the biomass data derived from the ADCP backscatter. Again, as reported in the literature, this decrease in biomass with depth is weaker off Kollam owing to the OMZ not extending as far south as the mooring location in the SEAS. This vertical movement of the oxycline that increases (during downwelling) or decreases (during upwelling) the vertical space available above it. Accordingly, the depth of the upper ocean or *D215* changes with season: it is lower during the summer monsoon, when upwelling occurs, and higher during the winter monsoon, when downwelling occurs. This oscillation of *D215* drives the seasonal variation of zooplankton standing stock because the physico-chemical forcing determines the vertical distribution of zooplankton biomass and therefore of its vertical integral, the standing stock.

It is not clear why this set of simple explanations is difficult to accept. Yet, as noted earlier, we have deleted this entire discussion of the physico-chemical forcing to restrict the focus to the potential of this tool for mapping the variability of zooplankton in the EAS. Till this revision, we had retained this element owing to the link between this manuscript and Shankar et al (2019). For the reasons stated earlier, we see little purpose in doing so now, but this material is deleted from the new version not because it is incorrect, but it is perhaps best published separately.

Comment 14: Through the paper, discussion on the biological aspects of zooplankton was limited.

Reply: The ADCP yields backscatter, from which biomass can be derived if in situ zooplankton sampling is carried out. It cannot, as stated in the manuscript, provide bioinformatics data. We have limited data from the zooplankton net sampling and, as acknowledged in the manuscript (both original and new versions) and by the reviewer (see Comment 5), the sampling is limited to October–December, when the moorings in the EAS have been serviced. One reason for choosing this season is that servicing of moorings requires fair weather and the conditions at this time of the year have been reasonable during most of our EAS cruises. (Note that Jyothibabu et al had to abandon a cruise in June 2001 as they were unable even to deploy the nets for zooplankton sampling. Servicing moorings is a more delicate operation.)

The objective of this manuscript is to present a time series of zooplankton biomass and standing stock in the EAS. The manuscript does not aim at discussing bioinformatics based on snapshots collected in different seasons in different years.

Comment 15: I think that this paper is not acceptable to the Progress in Oceanography.

Reply: We have rebutted all criticisms in the above comments. The potential of the ADCP backscatter for revolutionising zooplankton studies, as the satellite did for phytoplankton, is obvious from Figure R1. It is not possible to study phytoplankton variability today without invoking satellite data: that's why the SeaWiFS data are used by Jyothibabu et al as well!

Likewise, there is more to zooplankton studies than the bioinformatics snapshots resulting from net sampling. The contrast between the snapshots from the cruise data and the continuous time series from the moorings is, in our opinion (and that of Jiang et al, 2007), sufficient to showcase the potential of the ADCP. It is possible to foresee a time when ADCP moorings are used as the basic tool to study the variability of zooplankton biomass and standing stock, with cruise data being used to generate the bioinformatics to complement these time-series data for biomass and standing stock. It is only such a time series that can be used for decision-making or for forecasting or for answering a more basic science question like the energy flux through the marine ecosystem.

This manuscript shows this potential in the specific case of the EAS, which we think is sufficient to merit publication in this journal.

Response to comments of Reviewer #2

Overview and general recommendation: The paper deals with the calculation of zooplankton biomass and standing stock from backscattering data from three ADCPs positioned on three moorings in the Eastern Arabian sea. It shows the seasonal variability of the zooplankton standing stock obtained from long-term ADCP backscatter data, indicating that the Arabian paradox is not correct.

The subject of the paper is interesting, but the paper is really hard to read, with missing explaining parts, with inaccuracies, uncorrected use of some tools, several repetition and things spread around in the text. Moreover it is in general too long. It can be shorten without losing specificity, in particular the introduction and the description of seasonal cycle (chapter 3). In addition, the description of the content of each section at the beginning of each section is superfluous and can be cut (for example the summary of chapter 2 or the summary of the Summary and Discussion, chapter 5) . There are many parts that are irrelevant, while other are not developed enough.

Data and methods are lacking in many parts, while there are some parts in the text or in the captions that should be moved to data and methods: this section should contain all the elements on the data analysis that are necessary to the reader to understand the paper. Not always the references are appropriate to what they are referred to.

I therefore cannot recommend accepting this paper for publication in this Journal, even with a major revision. I think this should be rewritten and reorganized.

Reply: As noted in the preamble to this response, we have restricted this version of the manuscript to the time series of zooplankton biomass and standing stock. Many of the comments were due to the part on the physico-chemical forcing , which is not present in the new version. Nevertheless, we reply to all comments, including those not relevant to the new version.

Major comments:

(I will consider the line number given by the authors, not the ones set by the system)

In replying to the comments, we have used the same ordering scheme as the reviewer: the comments on each section are given the same Roman numeral, but each comment on a section has a letter suffixed to the numeral.

Comment 1a: Data and Methods: this section is lacking in many parts, while some description of the methods are spread in figure caption or in other sections of the text. In this way it is very hard to read and understand which are the methods and the analysis performed.

Reply: We have rewritten this section and trust it is now easier to follow.

Elements of methods contained in the figure captions have not, however, been deleted from these captions because the description contained in these captions was restricted to whatever was relevant to the figure in question. Our preference for seemingly long captions is inspired by what one of us (DS) learned in 1995 from Prof. Julian McCreary (earlier at NSU, Florida and IPRC and University of Hawaii). In replying to the same comment (lengthy figure captions) from a reviewer, Prof. McCreary said that he was told by Prof. Adrian Gill (one of the most remarkable and communicative oceanographers and meteorologists of all time) that a potential reader typically glances through the title, abstract, figures, and

conclusion in order to decide if it should be read with more attention. Therefore, Prof Gill said that the figures should tell the story as completely as possible, necessitating sufficient information in the captions to follow the paper. This lesson is one that the corresponding author has followed throughout his career because it makes sense and does so even more today when the number of published papers is an order of magnitude or two higher than in the 1980s.

Hence, while we have copied to the methods section such elements of the methods that were earlier restricted to the captions, we have not removed them from the captions because these elements matter mostly in the context of the figures and are required to follow the story contained in them. This manuscript is a story contained in these figures because it is not a typical ADCP backscatter manuscript discussing the method; instead, the focus is on the use of this tool to map the seasonal variation of zooplankton standing stock in the EAS.

Comment 1b: the MLD used in the analysis is both the climatological one and the one calculated from ARGO data (fig. 4--5--6), but it is not clear in the text (Lines 220--226 and some sentences in the figure captions) where do they come from or how they are calculated, if you have calculated them. Moreover, you refer to Chatterjee et al 2012 for the climatological MLD, but in that paper only climatology for T and S are presented. Did you calculate climatological MLD? How? This point must be clarified in the Data and Methods section.

Reply: The MLD was estimated as the depth at which sigma-t (σ_t) exceeds that at the surface by the increase in sigma-t caused by a 1°C change in temperature. This definition includes the effect of salinity on the stratification. For both data sets (climatology and Argo), we used the gridded temperature and salinity profiles to estimate the MLD using the above criterion.

In the new version, we no longer use the MLD because the integral used to estimate the standing stock is carried out from 24–120 m.

Comment 1c: You say on line 229-231 that the ADCP data were corrected for the vertical movement of the mooring line, but I can see from figures that the upper limit of you data is a straight line in time at about 20 m, while it should be variable: did you get rid of the data above that depth (about 20m)?

Reply: Yes, the upper limit in the earlier version was kept uniform at 20 m, which was the common upper depth for most of the data.

Comment 1d: The formula you use for the calculation of the backscattering strength is the one of Deines (1999), but it has been upgraded by Gostiaux and van Haren (Journal of atmospheric and oceanic technology, 2010) and further by Mullison (RDI note presented at ASCE, 2017) in order to take correctly into account the signal to noise ratio. I think that Sv calculations should be updated or you should justify in detail why you use the older formulation.

Reply: In the new version, we retain the method of Deines (1999) because the difference in backscatter values is less than 1 dB throughout and is close to zero over most of the record if we switch to the method of Mullison (2017); this difference, shown in Figure R7, is smaller than the contour interval used for the plots.

Following Mullison (2017),

$$S_v = C + 10 \log_{10} \left(\left| T_x + 273.16 \right| R^2 \right) - L_{DBM} - P_{DBW} + 2 \alpha R + 10 \log_{10} \left(10^{\frac{K_s (E - E_s)}{10}} - 1 \right) \quad \text{or}$$

$$S_v = |other terms| + 10 \log_{10} \left(10^{\frac{K_c (E - E_r)}{10}} - 1 \right)$$

Since $K_c \sim O(1)$ (see Table 1), $E \gg E_r$ implies $10^{\frac{K_c (E - E_r)}{10}} \gg 1$.

Then, neglecting 1 yields

$$S_v = |other terms| + 10 \log_{10} \left(10^{\frac{K_c (E - E_r)}{10}} \right), \text{ or}$$

$$S_v = |other terms| + 10 \cdot \frac{K_c (E - E_r)}{10}, \text{ or}$$

$$S_v = |other terms| + K_c (E - E_r), \text{ which is the same as in Deines (1999).}$$

Indeed, here lies a useful test or rule of thumb for the use of ADCP backscatter for inferring temporal (or even spatial) variation of zooplankton biomass or standing stock: if the results depend on the choice of such parameters, then the results should be discarded. It is only when the results are robust with respect to the choice of parameters (representing the ADCP electronics, etc.), which are generally impossible to determine for each time stamp, that the results should be considered to pass the threshold of acceptance.

Comment 1e: In my opinion, there is no need of the S_v formula in the data and methods section, as it has been already published in several papers, which you can refer to.

Reply: We have retained the formula simply because this manuscript has gone through several versions and there have been several comments on the computation of backscatter.

Comment 1f: I want hereafter to comment on your stressing on the fact that the 3 constant terms (C, Ldbm, Pdbm) are superfluous. However, the fact that the difference in the backscatter with or without them is constant (fig.4c of the answers to a previous referee) is obvious, as the three terms are constants. First of all, backscattering strength is commonly defined with the three terms. If you want to drop the three terms, you should define another variable name for that quantity. Second, and more important, these terms are not the same for all the 150kHz ADCP, but they change depending on the model and sometimes on instrument. This is important if you want to compare S_v from different instruments (that can change for different sites and different deployments). Do you have the same model of ADCP for all deployments and sites? And are you sure that the constant are exactly the same for all of them? Maybe the difference are not big, but it is the correct way to do it. You can ask the values of the constants to RDI, sending them the serial number. Moreover, they can provide you the K_c values that can be different for the 4 beams (depending on the instruments you have) and that comes out from factory calibration.

Reply: The rationale for discussing these terms was the link of this paper to the fisheries paper of Shankar et al (2019), who dropped the terms in the square brackets and obtained a positive value for the backscatter. The discussion on these terms has been dropped from the new version as we no longer seek to link this paper to Shankar et al.

We are aware that these constants are model-dependent. As noted in all versions of the manuscript, the instrument used in all deployments is the same RDI broadband ADCP. The earlier mix of 75 kHz and 153 kHz ADCPs was eliminated in the last version (August 2020) as we finally had appropriate zooplankton samples to enable a regression without the use of the second ADCP.

In the new version, we have recalculated the backscatter using the K_c values provided by RDI. The change in backscatter is simply too small to affect the results. Therefore, as noted in the reply to your Comment 1d, the results are robust with respect to the choice of parameters and should therefore be considered to pass the threshold of acceptance. In your comment, you note pretty much the same inference: that the changes due to changes in the parameter values are too small to affect our results.

Comment 1g: You calculate the slope and intercept for the linear relation between backscatter and $\log(\text{biomass})$ but you don't give the errors on slope and intercept, which would give an indication of the error on biomass. This could be briefly discussed.

Reply: Thank you for this comment. In the new version, we have added these errors on the slope and intercept to show the RMSE (Figures 2 and R8). The error in $\log(B)$, where B is the biomass, is ~ 0.25 , which is lower than the apparent error of ~ 0.6 in Jiang et al. (2007), who used a similar, multi-year data set from the BTM (Bermuda Testbed Mooring) site. The correlation we obtain is 0.55, twice that of Jiang et al. We think the reason for the higher correlation and lower error in our regression stems from the higher range of backscatter (~ 40 dB) sampled in our study compared to Jiang et al (~ 20 dB). This higher backscatter range in our study is a direct consequence of the decrease in backscatter from the "upper ocean" (above z_{215} or D_{215} , the depth of the 215 units biomass contour) to the thermocline, whose depth is now represented only by z_{215} , but also had a physical counterpart (D_{20} or the depth of the 20°C isotherm) in the earlier versions of the manuscript.

Comment 1h: With the solely ADCP you cannot determine the species in zooplankton biomass, but you can be helped by using more ADCPs working at different frequencies and sampling with nets, as also done in the paper you cite (Luo et al. 2000....). With just an ADCP you can only define the minimum detectable scatter size, that depends on the ADCP working frequency (which for a 150kHz ADCP is about 1cm). Therefore the sentence at line 267 is incorrect and I would rearrange the entire paragraph from 267 to 279.

Reply: Line 267 of the earlier version read as follows: "Earlier studies with the 153 kHz ADCPs indicate that the zooplankton biomass was dominated by copepod species ...". This sentence implies that it is the "studies" that listed the species; it does not imply that the list of species were estimated from the ADCP. In support of this contention, we point to lines 806-809, which read as follows: "Therefore, though the backscatter has the limitation in that it can be used only to infer the biomass and standing stock, but provides no information on the zooplankton species distribution ...".

The rest of the para (lines 267–279) merely compared the species seen in the samples collected by us and the species listed in the literature for the region. In the new version, we have modified the text (see lines 222–228 of the new version) and trust the new wording makes this point without any ambiguity.

"The major groups of zooplankton observed contributing to the zooplankton biomass in the samples we collected include calanoid copepods, cyclopoid copepods, Poecilostomatoida, Harpacticoida, appendicularians, euphausiids, ostracods, and chaetognaths (Table 2). The species in this list match those reported earlier in the EAS (Smith and Madhupratap, 2005) and constitute the major scatterers for the ADCP (Batchelder et al., 1995; Anonymous, 2011)."

Comment 1i: Line 287: how did you extrapolate? Repeating the ADCP uppermost backscatter value in the surface layer (0–20/24 m) divided in cells of 4 m?

Reply: Yes, the backscatter in the topmost bin at 20 m was extrapolated to the surface in the earlier version. We do not extrapolate in the new version of the manuscript: the standing stock is now estimated only over the depth range over which the backscatter data are available, but estimating the standing

stock by extrapolation to the surface, as done earlier, leads to a curve that is practically parallel to the one without this extrapolation.

Comment 1j: The content of paragraph 2.5--second half is confusing. You indicate that the regridding on monthly base is useful to eliminate day/night variability, but then you plot the day and night backscatter (fig.4 c and d) regridded on the monthly interval (as indicated in the caption), which obviously, due to the way it is calculated, present differences; however, you say that the day/night difference does not affect the analysis and that the daily backscatter (you did not mention it before) is “further averaged over a month”: “further” with respect of what? It is really hard to follow the logical thread. You should clarify.

Reply: The separate analysis for daytime and night-time data was introduced in the second version of this manuscript (item 2 in the preamble: Revision 1 dated April 2018) in response to a question from a reviewer. To make it clear that the results based on the daily backscatter, which averaged all the values during a day, were not affected by using daytime and night-time data separately, we introduced this text and supporting figure. Since our interest is restricted to the variability seen in monthly data, the backscatter, whether for the daytime values, night-time values, or the average over a day, we showed the day-night distinction by averaging all three estimates of the backscatter using a 30-day running mean. Since the daily backscatter is already the average over a day, the phrase “further averaged over a month” was used to indicate that the panel referred to showed the 30-day running mean of the daily backscatter.

In the new version, we have moved this discussion of the day-night difference to the supplement (Figure S1). The analysis in the manuscript is restricted to the daily backscatter and its average using either a 30-day running mean or a climatology.

Comment 1k: In figure 4d caption you write that backscatter is calculated from 19pm to 7am, but further on you write that you exclude times of migration, i.e. 6 am. I suppose it is calculated from 19pm to 5am, am I right?

Reply: Yes, you are right. This error in the text has been corrected.

Comment 1l: Moreover, all the description within the figure caption from “In defining day...” (line 51) to the end is better to be moved to Data and Methods.

Reply: Though this figure has been moved to the supplement and the panels in the revised figure are no longer the same, we have refrained from shortening the figure captions for the reason given in the reply to Comment 1a.

Comment 1m: Figure 5 is never used in the text nor discussed. If you don't use the figure, you can move it to the supplementary material. The same features for what concerns the seasonal variability are found both in the backscatter and in the biomass.

Reply: We presented the backscatter data because the backscatter was the basic variable and it is the backscatter time series that was discussed in earlier versions owing to the non-availability of an MPN. In the new version, we have accepted your suggestion and present only the biomass time series.

Comment 2a: Seasonal cycle: this section is rather long with repetitions and confused parts.

Reply: Elimination of the figure for backscatter has led to a much shorter section. Since we have also deleted the discussion of the physico-chemical forcing, the manuscript is now much shorter.

-- You use two estimations of MLD (climatology and from Argo) which sometimes are coherent and sometimes not. You use both, depending on the context, but you never discuss the differences between them and justify the use of one instead of the other.

Reply: This comment is no longer relevant owing to the elimination of the material on the physico-chemical forcing. Nevertheless, we reply to this comment.

The climatology of Chatterjee et al (2012) is based on a much larger database of temperature and salinity profiles and, in particular, includes a large number of profiles from the Indian EEZ; these data are not included in the World Ocean Atlas. Therefore, this climatology is more reliable in the eastern Arabian Sea because this region falls within the Indian EEZ. In contrast, Argo profiles are not as commonly available near the continental slope because they are generally deployed farther in the open ocean to ensure a longer life for the floats. Hence, though the gridded data based on the Argo floats provides an estimate of the temperature and salinity variability for a grid cell in the region of interest, it is based on sparse sampling in these grid cells. Nevertheless, since we had to discuss the variation of MLD for the duration of the ADCP record (owing to a reviewer comment), we had no option but to use this data set based on Argo. The climatological MLD was repeated in these figure panels for each year. In the text, we generally used the Argo MLD, but pointed to the difference between this estimate and the climatology whenever required, leading to the text often mentioning the variability seen in both estimates of MLD.

Comment 2b: The discussion at lines 343-354 is confused and difficult to understand and sometimes speculative.

Reply: In the literature, most discussions of MLD for a time series are similarly speculative because long-term concurrent measures of temperature and salinity are invariably not available. If one were to restrict all research merely to concurrent measurements, as is implied here, much of oceanography would be simply unviable! Most progress comes from reasonable speculations based on available data. If the speculations lead to a clear, testable hypothesis, then observations or experiments can be planned to test the hypothesis. Speculations or theories often precede data in not just oceanography in particular, but science in general.

Yet, as noted earlier, this entire discussion of the MLD has been deleted in the new version.

Comment 2c: No biomass data are available off Mumbai from the end of 2018 and during 2019 (Fig.6a), while backscatter data are available: why? Moreover, you discuss the biomass during this period at lines 397-400.

Reply: The backscatter data off Mumbai for the 2018–2019 deployment were not available for the entire range from 24–140 m. As shown in Figure 5a, the backscatter data off Mumbai were not available for the bottom few bins. We should have plotted the biomass for these missing bins in Figure 6a and did not realise that the Mumbai panels for backscatter and biomass were not similar. Thank you for pointing to this omission.

We had extrapolated from the last available bin to the bottom to estimate the standing stock. Note that we had done the same off Kollam, where too the backscatter data were not available for the bottom few bins in the same year. For Kollam, however, we had plotted the biomass correctly in Figure 6e.

Our approach in the new version is more conservative. We have refrained from extrapolating from the topmost data bin to the surface and, likewise, from the last bin to the bottom. The standing stock is therefore estimated over the depth range 24–120 m.

Comment 2d: Variance is plotted in Fig. 6b,d,f but you don't explain how it is calculated. Of course it depends on the type of calculation used for biomass, but it is not clear.

Reply: The estimate of variance was introduced in version 2 (item 2: Revision 1, April 2018), following the suggestion of a reviewer, in order to show that the temporal variation in backscatter was higher in the neighbourhood of the backscatter contour used to mark the bottom of the upper ocean (or *D20*). The analysis showed that the variance, as expected from the decrease in backscatter below the upper ocean and the relatively weak variation in time in the upper ocean, was indeed higher around this contour. This variance was estimated as the ratio of the $(x - \langle x \rangle)$ and $(x - x_{SD})$, $\langle x \rangle$ is the mean of the time series and x_{SD} is its standard deviation; this definition was also provided by the reviewer.

In the new version, we do not use this measure owing to the significant changes in the text. The Arabian Sea paradox, which was discussed at reasonable length in the earlier versions, is no longer given the same importance because it is not as relevant to this manuscript following the elimination of the discussion on the physico-chemical forcing.

Comment 2e: The description of *D215* is too long and it is difficult to follow due to some confusing terms or crossing parts in discussion.

Reply: This discussion has been rewritten completely and we trust it is no longer confusing.

Comment 2f: And at lines 374-375 you say that in order to compute the mean you have filled the gaps in *D215* with “140 m” value, but then at lines 379-382 you say that to compute standard deviation the length of time series is restricted: did you not use the same series used to compute mean? why?

Reply: This comment is no longer relevant because we no longer discuss the mean and standard deviation of *D215*. This discussion has been deleted because we no longer discuss the physico-chemical forcing, for which we had shown the connection between *D215* and *D20*. Yet, we answer the question since a doubt has been raised regarding the method used in the earlier version.

Since the data were available only till 140 m and *D215* exceeded this maximum depth over a part of the record off Mumbai, we set it to 140 m whenever it was deeper. Doing so resulted in an underestimate of the mean *D215*, but not by much as the deepening beyond 140 m did not occur over a long part of the record off Mumbai. For estimating the standard deviation, however, the part of the time series over which *D215* exceeded 140 m was not used. The filling of the gap with the value at 140 m was necessitated for wavelet analysis.

Please see also the reply to your Comment 2m.

Comment 2g: Line 384 and following lines: which standard deviation? The one of the time averaged biomass? The term “standard deviation” is used many times in the text referred to different quantities and it is not always clear to what it is referred to.

Reply: The standard deviation referred to in the text was always with reference to the table and it meant the standard deviation over the time series of the variable. In the case of line 384, the variable was the biomass at 40 m and the standard deviation was estimated for the time series.

Comment 2h: Line 388 what is “the depth regime over which *D215* varies”?

Reply: “The depth regime over which *D215* varies” was seen in Figure 6d, which was referred to in the sentence.

Comment 2i: Lines 390-392: it is not clear: you compare std calculated (I suppose: look at the comment above) over time with a decrease of biomass with depth. All this section (383-394) needs to be clarified.

Reply: We cannot understand the confusion. As stated, the standard deviation of biomass was estimated at 40 m and 105 m over the time series. To compare the temporal variation of biomass at these two depths, of which one (40 m) lies in the upper ocean and the other (105 m) is a depth across which *D215* varies, we compared the two standard deviations for a given station. Likewise, the standard deviation at a given depth was compared for the three stations.

Comment 2k: Lines 405-409: this is already said at lines 338-339 and 286-288. Lines 407-409 should be moved above to replace lines 286-288, because they explain more clearly what you did.

Reply: This extrapolation to the surface is no longer done in the new version.

Comment 2l: Lines 409--417: should be moved to data and methods. ("Likewise, the vagaries of deployment at sea and the rapid change in topography across the continental slope imply that the mooring may land in water at a slightly higher or lower depth than intended. The deployment off Mumbai and Kollam during 2018–2019 presents one such case where the mooring landed at a depth roughly 30 m shallower than intended, with the result that the transducer of the ADCP was located at 123 m off Mumbai and 120 m off Kollam (Table 1). A consequence is a gap in the data in the bottom few bins for these two deployments from October 2018 (Fig. 6a,e)."

Reply: We have excluded the data below 120 m from the standing-stock analysis presented in the new version. These data have been kept out to keep the analysis simple and to drive home the key point, the usefulness of the ADCP backscatter as a measure of zooplankton biomass and standing stock.

Comment 2m: Lines 417--421: why overestimate? ("We note that extrapolation to the bottom off Mumbai may lead to an overestimate of biomass in the bottom ~20 m during October 2018 to April 2019 because *D215* exceeds this depth during this period (Fig. 6a). Off Kollam, where *D215* is shallower than off Mumbai, this potential overestimate of biomass is restricted to November–December 2018 (Fig. 6e)."

Reply: As noted above, the data for 2018–2019 are no longer used. Notwithstanding this change, we reply to the above comment.

During October 2018 to April 2019, *D215* exceeds the depth up to which the backscatter data are available. Therefore, we do not know if *D215* lies between the last bin for which data are available and 140 m or *D215* exceeds 140 m. Given the observed variation of *D215* in other years, it is more likely that *D215* is less than 140 m, but the extrapolation from the last available bin to 140 m will push it to 140 m (because *D215* is set to 140 m when it exceeds 140 m). When one views this overestimate of *D215* in conjunction with the tendency of biomass to decrease below it, we infer that this extrapolation of the biomass from the last available bin to 140 m is likely to lead to an overestimate of the biomass in the extrapolated bins. At Kollam, *D215* is shallower than off Mumbai, implying that the extrapolation from the last available bin to 140 m would lead to an even higher overestimate of biomass in the extrapolated bins. This point was made without elaboration in the above para.

Comment 2n: Lines 423--424 are obvious. ("it is multiplied by 10⁻³ before plotting to change the units from mg m⁻² to g m⁻²")

Reply: This phrase is dropped in the new version.

Comment 2o: Lines 435--439 are not clear. ("In spite of this evident seasonality, the standard deviation off Goa, at ~11.8% of the mean, is smaller than that off Mumbai or Kollam, but it stands out in a

wavelet analysis. High, statistically significant wavelet power occurs in the annual band off Goa (Fig. 7d).”)

Reply: The seasonal cycle of standing stock off Goa is evident in Figure 7b (red curve). The seasonal cycle stands out at Goa in all the years for which data are available and it shows up as a strong annual band in the wavelet power (Figure 7d). Yet, in spite of this striking annual or seasonal variability in all years, the standing stock off Goa shows a weaker standard deviation compared to Kollam or Mumbai, where the seasonality is not as striking in Figure 7b or in the wavelet power. It is this discrepancy between the standard deviation and the inference from a visual inspection or from a wavelet analysis that we referred to in the above para. This “discrepancy” arises because the standard deviation does not measure at a given frequency: it includes the trend off Mumbai and the quasi-biennial signal off Kollam (as you note in Comment 3b below).

Comment 2p: For what concerns wavelet analysis: you cannot discuss the wavelet power spectra values that are outside the cone of influence (see for example, the description of the variability at 12 months in 2012-2013, Line 441), because they are contaminated by edge-effect artifacts. And the area outside the cone of influence in fig. 7c-d-e should be masked. Moreover, the discussion from line 440 to line 450 is not clear, and I cannot understand the explanation at lines 453-458 concerning low values in wavelet power spectra at 12 months off Kollam.

Reply: We are aware that the cone of influence imposes a statistical limit on the interpretation of the wavelet power; the curve was plotted to show what can be interpreted with some statistical confidence and what cannot be relied upon as much. In spite of this limitation, we have, as you note in this comment, extended the interpretation of the wavelet spectrum beyond the cone of influence. The only reason we believe it is not unreasonable — provided the cone of influence is plotted to show clearly what lies within it — because of the continuity of the contours during 2012–2014, of which one year lies within the cone of influence and one year lies outside it.

The para in lines 440–450 linked the variation of *D215* to the variation of standing stock. These two variables are related owing to the decrease in biomass below *D215*, which, as noted in the manuscript, is taken to mark the bottom of the regime called the upper ocean.

The relatively low change in biomass across *D215* off Kollam (the biomass does not decrease as much below *D215* off Kollam as it does off Goa or Mumbai) leads to a weaker link between the two variables off Kollam. This weaker link is the point made in lines 453–458.

In the new version, we have not elaborated on the role of *D215* and much of this text has been deleted to keep the focus on the variation in standing stock. If we are deleting the explanation for this variation, the physico-chemical forcing, there is little purpose in discussing the role of *D215* as elaborately as done in the earlier versions.

Comment 2q: Lines 467-469: and what about the comparable high values in power spectra at 4 months at the end of 2016?

Reply: The higher power off Goa in the four-month band towards the end of 2016 is not related to *D215*, but is likely to be related to the changes in biomass in this period band above *z215*. A much more detailed analysis will be required to explain all such peaks in the wavelet spectrum and was therefore not attempted: all we stated were relations that were clear without a more detailed depth-wise analysis.

Please note that these descriptions have been deleted from the new version. Since the revision excludes the discussion of physico-chemical forcing, it serves little purpose to discuss the variation of *D215*: we had discussed *D215* in detail earlier owing to its connection to *D20* and the oxycline.

Comment 2r: The chapter 3.3 is too long and it repeats what already known from the time series of biomass and SS. You should just tell that the features are similar and discuss only eventual differences from that behavior. Moreover, some of the numbers in lines 510-512 are not correct.

Reply: We have deleted much of Section 3 and it is completely rewritten. We have also checked carefully all numbers reported in the manuscript.

Comment 2s: Line 474: how do you compute the monthly climatology of biomass? Averaging all the values for January, all the one for February, etc? Maybe the term climatology is not appropriate. In Fig. 8 it is misleading to plot data for January two times (on the left and on the right of the plot).

Reply: The climatology is computed by averaging all values for a given month. In contrast, when we present the monthly time series, the averaging uses a 30-day running mean. This distinction is explained clearly in the new version.

Comment 3a: The physico-chemical forcing: there are repetition of statements already contained in the introduction.

Reply: The entire discussion of the physico-chemical forcing has been deleted in the new version. We present the time series of standing stock and its climatology to make the case for using ADCP backscatter as a tool to monitor the temporal variation of zooplankton biomass and standing stock. The earlier versions included a discussion of the forcing, but we have removed it even though we consider it to be based on perfectly reasonable and well-accepted processes.

Comment 3b: Correlation considers the signal as a whole and comparing it with the power spectra at just one period may be tricky or misleading (line 567 and following). Do you calculate correlation between *D215* and the other variables excluding points below 140m, don't you? Therefore is it calculated over less points than the total length of data time series?

Reply: It is precisely for this reason — that the correlation considers the signal as a whole, unlike the wavelet spectrum — that we did not use correlations in the first two versions of the manuscript and restricted its use to the climatology in the third version. The use of correlations was necessitated by reviewer comments and so was its extension to the time series.

Yes, the length of the time series used to estimate the correlations depends on the data available.

In the new version, the elimination of the discussion on the physico-chemical forcing has led to the elimination of most correlation estimates. Only the correlation between *D215* and standing stock is presented in Section 3.2.

Comment 3c: Why you do not calculate correlation between *Chl a* and SS?

Reply: We did not calculate this correlation because the curves for *chl-a* and standing stock were visually uncorrelated. We could have shown these numbers too, but did not do so as the two variables are clearly not co-varying.

Comment 3d: In addition, you widely discuss correlation (lines 567 and following), separating periods etc..., and sometimes your arguments are not so straightforward and totally convincing. Moreover, you just describe the correlations without giving a physical-biological-chemical explanation for what you observe.

Reply: We have deleted most correlations from the new version, but find it difficult to accept the criticism that these correlations were presented without giving a physical-biological-chemical explanation. Each of

the processes involved in the chain from the physical forcing (movement of the thermocline or *D20*) to the standing stock via the intermediate variables (oxygen and chlorophyll) was clearly discussed.

The key elements of this physico-chemical forcing of zooplankton standing stock or the vertical distribution of biomass is rather straightforward and we refer repeatedly to the literature to make the case. The vertical movement of the isotherms are associated with the seasonal reversal of the circulation and the vertical movement of the isolines of oxygen (or the oxycline). The existence of the oxygen-minimum zone (OMZ) in the EAS leads to the decrease in zooplankton biomass with increasing depth. This vertical distribution, reported earlier in the literature based on in situ sampling, is clearly seen in the biomass data derived from the ADCP backscatter. Again, as reported in the literature, this decrease in biomass with depth is weaker off Kollam owing to the OMZ not extending as far south as the mooring location in the SEAS. This vertical movement of the oxycline that increases (during downwelling) or decreases (during upwelling) the vertical space available above it. Accordingly, the depth of the upper ocean or *D215* changes with season: it is lower during the summer monsoon, when upwelling occurs, and higher during the winter monsoon, when downwelling occurs. This oscillation of *D215* drives the seasonal variation of zooplankton standing stock because the physico-chemical forcing determines the vertical distribution of zooplankton biomass and therefore of its vertical integral, the standing stock.

Indeed, the reasonableness of the arguments stated earlier for the physico-chemical forcing is borne out by the following statement in your Comment 4b: “*Moreover, from the climatology (Fig. 8) D215 and thermocline are really close.*”

Comment 3e: At lines 664-672 you discuss the discrepancy in time between the peak in phyto and zooplankton, considering satellite Chla measurements which are however related only to surface layer: do you know if there are deep chlorophyll maxima (DCM) which are not captured by satellite? Do you have profiles of Chla?

Reply: No, we do not have profiles of chl-*a*. The satellite data are supposed to provide an integral estimate of the chl-*a* in the top 30–45 m of the water column; the depth to which the satellite can see is a function of the turbidity. Yet, the satellite data have been used throughout the literature to map the variability of phytoplankton biomass across time scales ranging from blooms (bursts with a time scale of a few days) to interannual variability. None of these studies can do anything about deep chlorophyll maxima that are not sampled by satellites. Yet, as we note repeatedly in this manuscript, in situ sampling using ships is not a viable means to map the temporal variability of phytoplankton (or zooplankton) biomass.

It is now possible to use profiling floats like bio-Argo to estimate the chl-*a* in the water column up to a few hundred metres (and therefore covering any possible deep chlorophyll maximum), but this tool is not yet widely used in the north Indian Ocean.

We hope that such sampling, in spite of the known problems associated with the fluorescence measurements from floats, will complement sampling of zooplankton biomass and standing stock using moored ADCPs in the years to come.

Comment 4a: Summary and conclusions: The summary of the summary at the beginning of the section is useless.

Reply: When we do not include such a summary, reviewers ask us to include it, but when we include it, we are asked to remove it! A brief summary of the results is retained in the new version.

Comment 4b: Lines 621-625: why do you say that the backscatter “decreases from the upper ocean, whose bottom is denoted..., to the thermocline”? But you don’t have the time series of the thermocline

(and there is no line in fig. 5) but just the climatology. Moreover, from the climatology (Fig. 8) *D215* and thermocline are really close.

Reply: The altimeter SLAs (sea-level anomalies) are known to correlate well with *D20* or a similar measure of the thermocline: when *D20* deepens (shallows), SLA increases (decreases). That's why we presented the correlations between SLA and *D215* for the entire time series in table 3. This link between SLA and *D20* was pointed out in the introduction (lines 59–63): “This seasonal movement of the thermocline is mirrored in the sea level measured using tide gauges and the altimeter, with the altimeter sea-level anomalies (SLAs) decreasing (increasing) during the summer (winter) monsoon (Fig. 2c; Shankar, 1998, 2000; Shankar et al., 2002).”

Comment 4b: Lines 711–715: why do you write this here? It is not connected with the rest of the discussion. (“The decrease in biomass with depth therefore occurs much below 30–40 m, the maximum depth that can be sensed by the satellite sensors for chlorophyll-a; this depth varies from ~40–45 m for very clear waters (surface concentration ~0.01 mg m⁻³ to ~20–25 m for surface chlorophyll-a concentration of the order of 1 mg m⁻³ (Andre, 1992).”)

Reply: In the new version, since we have deleted the discussion of the physico-chemical forcing, most of the discussion on MLD has also been deleted and the vertical integral of biomass is now restricted to the topmost bin for which backscatter data are available. Hence, this comment is no longer relevant.

Notwithstanding this change, the point made in the above lines concerned the MLD, which tends to be shallow in the EAS, and its potential implication for the estimate of standing stock. The significant decrease in biomass tended to occur around *D215*, which was much deeper than the depth of ~30 m associated with the satellite chl-*a* estimates. Therefore, this decrease was related to *D20* rather than to MLD.

Comment 4c: Lines 719: where does this information come from? (“high standing stock in the upper ocean, which contributes ~85% to the standing stock.”)

Reply: This estimate of ~85% was based on the data presented in the figures and tables. This subsection (5.2) has been deleted in the new version and so has this sentence.

Comment 4d: Paragraphs 5.3.1 and 5.3.2 are not appropriate in the conclusions, and totally out of place after the discussion of the part before. They should be moved in Data and Method.

Reply: These two subsections (4.1.1 and 4.1.2 in the new version) discuss limitations of the methods used and are therefore retained in the discussion. We do not see why the limitations should be discussed before the results are presented. Both limitations and potential of the methods are therefore presented in the discussion.

Comment 4e: Why do you introduce CIB (line 759 and following) which is totally uncorrelated with the on-going discussion?

Reply: The CIB was introduced because it is the measure of standing stock that was used by Shankar et al. (2019). They did not have access to an MPN and were forced to use the limited biomass data from just the mooring location off Mumbai. To avoid using data from just one station to convert backscatter to biomass at all three locations, they integrated the backscatter itself in the vertical. This integral is the limiting case in which the relation between backscatter and biomass is linear instead of logarithmic.

Since the close link between this manuscript and Shankar et al. (2019) has been almost eliminated in the new version, we do not refer to the CIB until section 4.2, in which this point is made explicitly. It is only this link between the two papers that is retained in the new version.

Comment 4f: Paragraph 5.3.3 is decontextualized and somehow speculative.

Reply: This para or subsection on the continental shelf was included to make a comment on the continental shelf because the fisheries paper of Shankar et al. (2019) also commented on the extension of the inferences from ADCP data to the shelf owing to the fisheries being richer on the shelf compared to the slope. With the elimination of the text on physico-chemical forcing and the link between the two manuscripts, this para is no longer relevant and has been deleted in the new version.

Comment 4g: Line 794: why do you say that the mooring locations in the EAS are interchangeable? You have found differences among them.

Reply: This sentence meant that each of the mooring locations could be considered representative of its specific location within the EAS, i.e., NEAS or CEAS or SEAS. It did not mean that the mooring locations could be used interchangeably, but that the term “NEAS” and the Mumbai location could be interchanged owing to the averaging over roughly a month, which eliminates the low wavelengths in a spatial analysis of zooplankton biomass or standing stock.

We agree that this statement could be misunderstood to mean what you interpreted and have therefore modified it in the new version (lines 596–599): “it is this link that led us to use the specific mooring locations (Mumbai, Goa, and Kollam) as representative of a larger regime within the EAS (NEAS, CEAS, and SEAS, respectively).”

Comment 4h: A final comment: you use data from the net samples just to calculate intercept and slope of the linear regression between backscatter and biomass. Maybe the information coming from the analysis of those net sample (i.e. zooplankton composition species) could help you in interpreting your results in order to explain some features or differences you observe in the time series, even if the sampling is only performed in autumn--early winter.

Reply: The backscatter and the zooplankton biomass and standing stock estimated from it are used to map the seasonal variability. As noted by Jiang et al. (2007) and in all versions of our manuscript, it is not possible to map the variability of zooplankton biomass and standing stock in this manner using net samples. The net sampling is not irrelevant, but it is better used to add bio-information (bioinformatics) to the variability mapped using an autonomous tool like the ADCP. The details of zooplankton species would be of little use in this manuscript because all we would be able to tabulate is the composition during the cruise period of September-November of different years. The limited sampling will not suffice even to comment on the variability across years and the restriction of this sampling to the same months each year rules out any comment on seasonal variability. As stated in our reply to the major comments of Reviewer #1 and depicted in Figure R1, we do not think the data reported in Jyothibabu et al. (2010) or similar papers can map the (seasonal) variability of biomass or standing stock, leave alone the changes in species across years or seasons or within a season.

Minor comments: (Note that we have included the text in the lines mentioned in these comments to enable this response to be read without referring to the original or new manuscripts.)

Comment 1: Lines 115--119: in part are a repetition and in part superfluous. (“Yet, this question of the seasonal variability of zooplankton standing stock is important because of the large seasonal changes in phytoplankton biomass revealed by satellite data. Unfortunately, satellites cannot sense zooplankton; hence, the spatial coverage and resolution now possible for chlorophyll is not possible for zooplankton.”)

Reply: The repetition element has been eliminated by editing the earlier reference to the seasonal variability of chlorophyll and zooplankton. This sentence has, however, been retained in the new version

because it makes a concise statement of the vast differences that now prevail in the way these two trophic levels (phytoplankton and zooplankton) are now studied. It is no longer possible to envisage a study of phytoplankton variability without invoking satellite data; in contrast, zooplankton continue to be studied the same way as phytoplankton were studied before the SeaWiFS era by in situ sampling alone. As noted by Jiang et al. (2007) in (their) section 4.1: “*The nearly continuous sampling afforded by the ADCP technique is effective for making good seasonal estimates, especially because of the degree of short-term variability in the biology that cannot be adequately sampled using traditional ship-based net tows, which can generally be done only for a few days per month and not during inclement ocean conditions.*”

Comment 2: Lines 128--134: these lines are not necessary (“Early use of acoustic backscatter was based on echosounders, not ADCPs. Greenlaw (1979) described the relationship between the backscatter and the size and abundance of zooplankton, but it was the availability of ADCPs that revolutionised our ability to study temporal variation in zooplankton biomass because they operate at a frequency higher than conventionally used for echosounders and can therefore pick zooplankton at much smaller sizes than is possible at the echosounder frequencies.”)

Reply: These two sentences merely summarise the background for the use of acoustics to map changes in zooplankton biomass. Given that we are being asked why this manuscript should be published, we think this background is required.

Comment 3: Lines 139--140: there are several more recent papers dealing with DVM (“The high temporal resolution of these data has also been exploited to characterise diel migration patterns (see, for example, Plueddemann and Pinkel, 1989; Roe and Griffiths, 1993; Inoue et al., 2016).”)

Reply: The DVM is not the subject of this manuscript, but it happens to be the most studied aspect of zooplankton using ADCP backscatter. That’s the main reason for referring to these studies. We have referred to a couple of early, pioneering papers and to a more recent paper. We can add more references, but they would be superfluous. If, however, you feel a specific paper should be reported in preference to those cited owing to its superior quality, we would gladly do so.

Comment 4: Lines 168--175: all these lines are superfluous: just define the two quantities. (“to describe the seasonal variability of zooplankton biomass and standing stock in the EAS. Since these terms are often used interchangeably and the distinction between them tends to be vague, it is helpful to define them here in order to link them to the ADCP backscatter. We use the term “zooplankton biomass” to refer to the amount of zooplankton in a unit volume; its unit is wet weight mg m^{-3} and its ADCP equivalent is backscatter (unit: dB). “Standing stock” (of zooplankton) refers to the column-integrated zooplankton biomass; its unit is wet weight mg m^{-2} or wet weight g m^{-2} , and its ADCP equivalent is column-integrated backscatter (unit: dB m).”)

Reply: The problem is that these two terms, “zooplankton biomass” and “zooplankton standing stock”, are used interchangeably in the literature. Examples include three papers referred to in our manuscript; the relevant material is reproduced as Figure R9.

Jiang et al. (2007), who used ADCP data from the Bermuda Test Mooring site, referred to the depth-integrated zooplankton biomass estimated from the ADCP backscatter as well as the net samples as “biomass”; they used the prefix “depth-integrated” on occasions, but, as may be verified by a search, did not use the term “standing stock”. In Figure R9a, we reproduce their Figure 5 as an example of this usage: note that the caption refers to biomass in units of per m^2 .

Madhupratap et al. (1996) used both terms, “average column standing stock of zooplankton ($\text{g dry weight per m}^2$)” and “average zooplankton biomass ($\text{g dry weight per m}^2$)” in the caption of their table 1

(reproduced as Figure R9b). Since the same unit (per unit area) is used for both terms, it is clear that the two terms, “biomass” and “standing stock”, are used interchangeably.

Jyothibabu et al. (2010) also used the term “MSP biomass” to refer to the mesozooplankton standing stock presented in the figures. An example is the discussion of figure 12a on page 247 (reproduced as Figure R9d); in the para immediately above the figure, they used the term “MSP standing stock” (one of the few occurrences of this prefix), but the figure caption used the term “MSP biomass”. They did use the term “standing stock” on occasions, but this use was largely restricted to references to the literature. Curiously, the term “standing stock” was used by them more for phytoplankton than for zooplankton (see Figure R9c).

We make clear the distinction between biomass and standing stock. We use “biomass” to refer to the “density” equivalent of zooplankton (per unit volume) and standing stock to refer to the vertical integral of biomass.

Comment 5: Lines 186--189: these lines are superfluous and obvious. (“In this section, we describe the data sets used in the paper and the methods used to process the ADCP data and to estimate backscatter and zooplankton biomass. We end the section by clarifying the backscatter and biomass time series that are used for further analysis.”)

Reply: We have retained one sentence at the start of the section before moving to the subsection; it looks ugly (to many readers and reviewers) if a subsection starts immediately after the section title.

Comment 6: Line 200: which is/are the model of the ADCPs? Do you use the same model of ADCP for all moorings and deployments?

Reply: Yes, the same ADCP model is used for all deployments. As noted in section 2 and in the caption of table 1, all ADCPs used are broadband, have a frequency of 153 kHz, and are of make RD Instruments.

Comment 7: Line 294: the title is inappropriate with the content of the paragraph. (“2.5. The backscatter and biomass time series”)

Reply: This title was used to separate the presentation of the time series from the estimation of zooplankton biomass. In the new version, since we no longer present the backscatter time series (following your Comment 1m), the two subsections have been merged into section 2.3 (Estimation of zooplankton biomass).

Comment 8: Lines 319--322: these lines are not necessary (“In this section, we describe the seasonal cycle of the zooplankton standing stock. We begin with a description of the seasonal variation of biomass and standing stock during 2012–2019 and end the section by describing the climatological seasonal cycle.”)

Reply: We have retained one sentence at the start of the section before moving to the subsection; it looks ugly (to many readers and reviewers) if a subsection starts immediately after the section title. With the significant changes made to section 3, even this opening para has undergone changes.

Comment 9: In each figure caption the same information is repeated two times. The first one is contained in the first part of the caption and it is superfluous.

Reply: It is not clear which captions were repeating text within the caption, but we have checked all captions in the new version to eliminate such redundancy. As noted in the reply to Comment 1a, however, we have retained a few explanatory sentences in the figure captions.

Comment 10: In several figures sentences on data process is contained: they should be moved to data and methods.

Reply: We have retained these sentences for the reason given in the reply to Comment 1a, but have checked section 2 to ensure that it does not exclude material contained in figure or table captions.

Comment 11: Table 1: The sentence in the caption “The moorings, located availability” is not directly connected with table content and it is already specified in the text. Please remove. The sentence “The depth range....for most of the time series” is not appropriate here: move it to the Data and Methods. This also because in the text there is no explanation for the reason of the maximum depth at 140m.

Reply: We have retained this sentence in the caption to link it to the figures, which cut the data at 140 m, for the same reason given in our reply to Comment 1a (on figure captions). This sentence has, however, been moved to the end of the caption as it does not refer directly to the table contents, but links the table to the figures.

Comment 12: Table 2: as you specify in the text that all the samplings are about 10 km far away from the moorings, it would be easier for the reader to have a column with the referring--mooring for each sampling collection instead of latitude and longitude.

Reply: Thank you for this suggestion. In the new version, we have added a column to table 1 (mooring details); this column lists a deployment identifier (M1, etc. for Mumbai), which is added in a new column in table 2 (zooplankton sampling) to link the two tables. We have retained, however, the position information in the zooplankton table.

Comment 13: Table 3: it would be more readable if : 1) you add a line at the top with the titles “time series” and “climatology” above the corresponding r; 2) instead of r1 r2 r3 etc, I think it would be better to write D215--SS,SLA--D215, SLA--SS etc.

Reply: Most of the correlations involved *D20*, the SLA, the oxycline, and *D215*. With the elimination of the material on the physico-chemical forcing, most correlations are now eliminated and table 3 has been deleted. A few correlations are reported, but they are mentioned in the text.

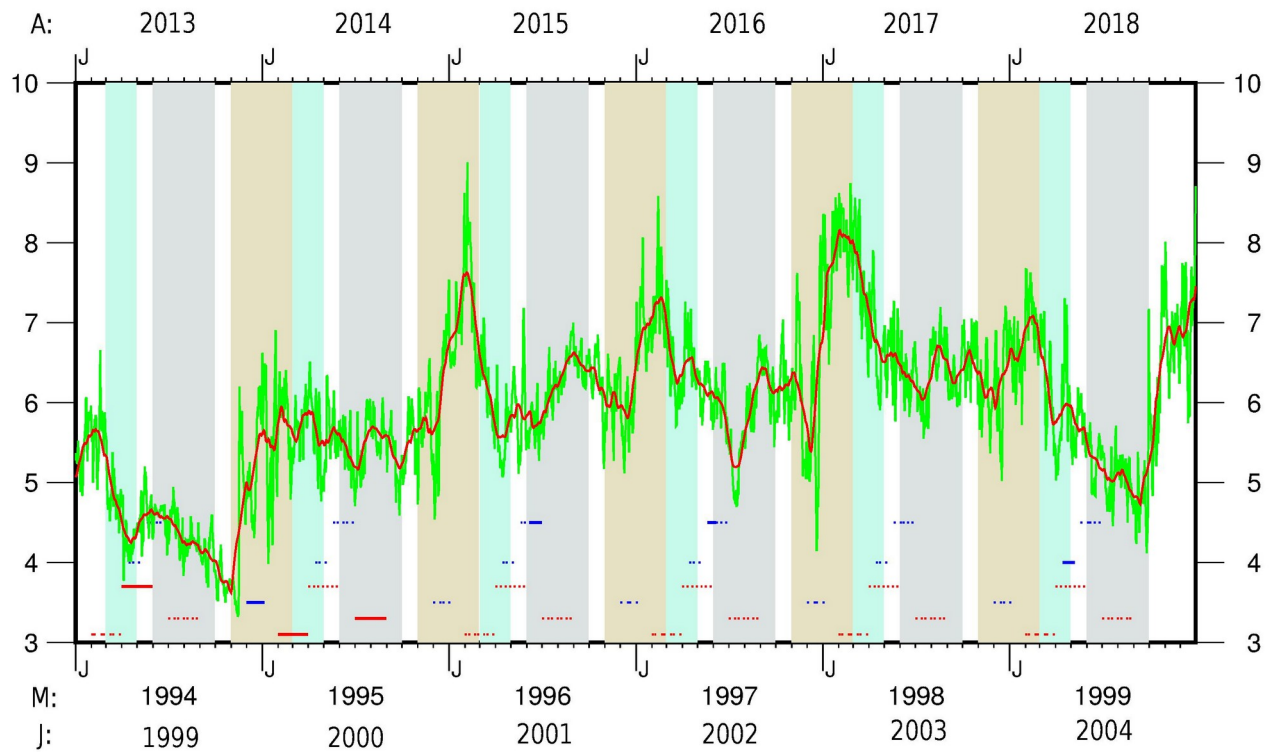


Figure R1. This figure shows the zooplankton standing stock available from both methods (ADCP backscatter and in situ sampling). The abscissa shows time, but the years referred to are abstract. The axis spans six years (year 1 to year 6) because the longest continuous record available covers six years (off Goa, 2015–2020; see Figure 3 in the new version); the data set is longer (eight years, 2013–2020) off Mumbai and Kollam. Since the reviewer has commented on the trend off Mumbai, we show the data for Mumbai and the years covered in the figure (see the top axis) are 2013–2018. The green curve shows the daily biomass estimated from the backscatter data off Mumbai and the red curve superimposed on it uses the 30-day running mean to smooth out the high-frequency fluctuations and show the monthly variation. This abstract axis also allows us to overlay the data from the cruises reported by Jyothibabu et al. (2010) and Madhupratap et al. (1996). The abscissa extends from 1994–1999 for Madhupratap et al. (bottom axis labelled “M”) and from 1999–2004 for Jyothibabu et al. (bottom axis labelled “J”). The ordinate is also abstract and is not meant to facilitate inter-comparison between the three different sets of data from the three papers because each of these papers uses a different depth range for the vertical integral; the data from each study are, however, plotted to scale. The range of the ordinate is fixed on the basis of the range in the standing stock inferred from the backscatter. The seasons indicated by the vertical shaded bars are as defined in our manuscript. The three cruises reported by Madhupratap et al. (1996) were conducted over 16 months (from 12 April 1994 to 12 August 1995) and are plotted accordingly in the first two years on the abscissa; for these data, the first two years represent the calendar years 1994–1995. In each of these cruises, the data from a given location are collected during a single day at best, but are considered representative of that season in that year (as shown in by the solid, red horizontal lines for these cruise data) and also over that season in all years (as shown by the dashed, red horizontal lines). Likewise, Jyothibabu et al. (2010) reported data from four cruises conducted during 1 December 1999 to 8 May 2004. Since the period from 1 January 1999 to 31 December 2004 covers five years, the data from these cruises are plotted at appropriate times on this abstract abscissa. As with Madhupratap et al., the solid horizontal (blue) lines represent the cruise when the sampling was carried out and the dashed (blue) horizontal lines their extension to the same season in other years. See the replies to Comments 2 and 15 of Reviewer #1 and Comment 4h of Reviewer #2.

All-India Summer Monsoon Rainfall, 1871-2017

(Based on IITM Homogeneous Indian Monthly Rainfall Data Set)

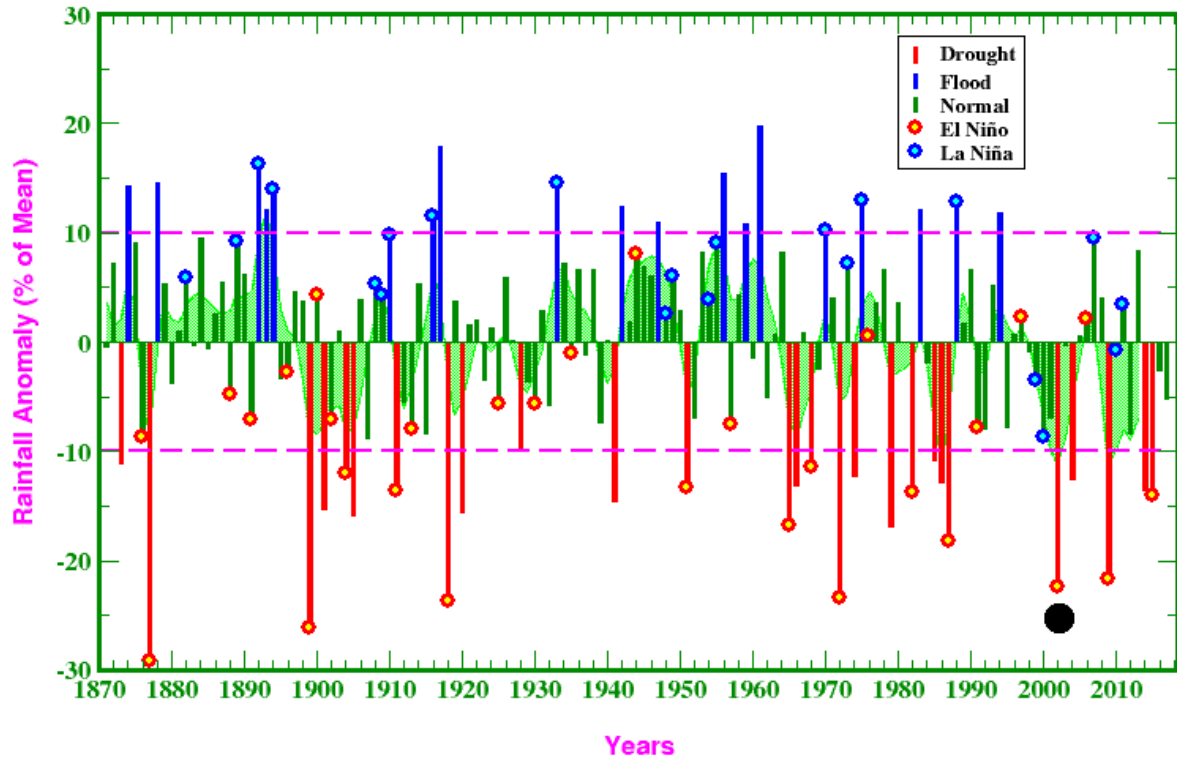


Figure R2. All-India summer-monsoon rainfall (AISMR), based on a homogeneous rainfall data set of 306 raingauges in India, developed by the Indian Institute of Tropical Meteorology, is widely considered as a reliable index of summer monsoon activity over the Indian region. This figure, downloaded from <https://www.tropmet.res.in/~kolli/MOL/Monsoon/Historical/air.html>, is shown to point to the differences between 1999, 2000, 2001, 2002, and 2004, the years for which zooplankton data were collected by Jyothibabu et al. (2010) in different seasons. See the reply to Comment 2 of Reviewer #1.

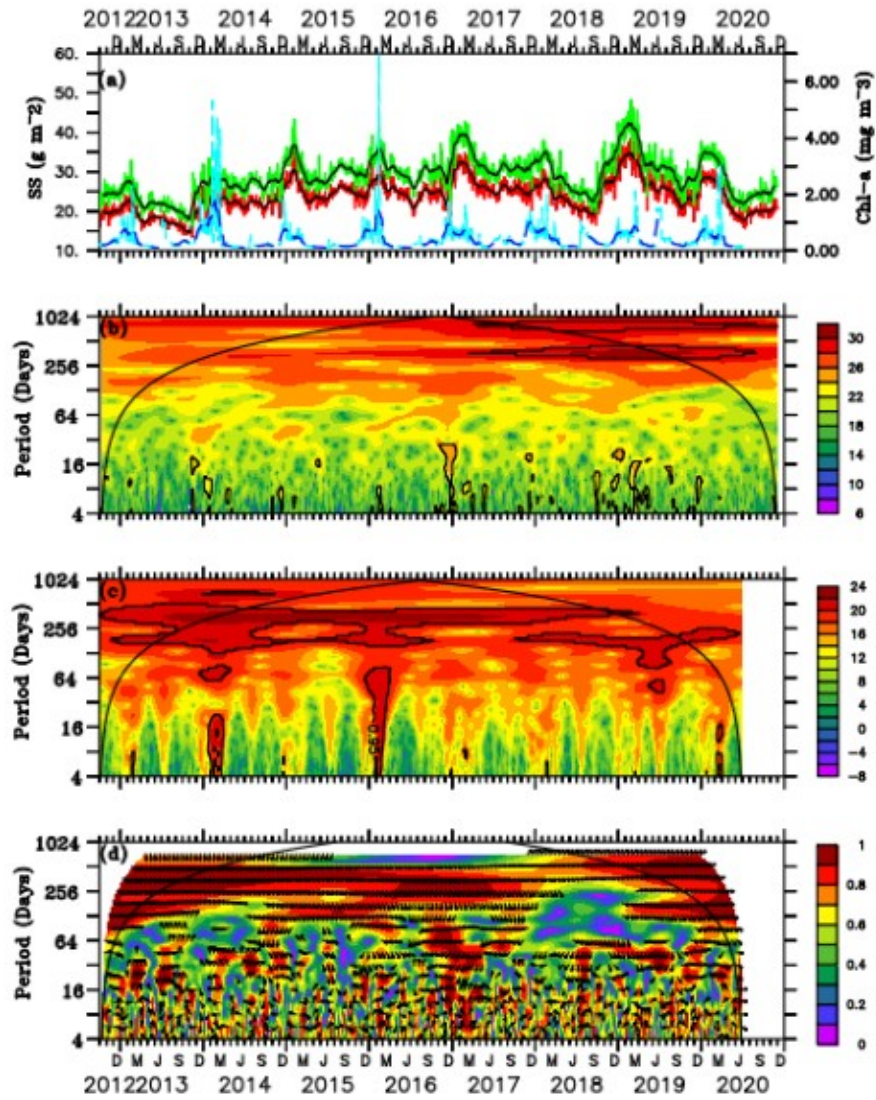


Figure R3. (a) Time series of daily standing stock (red curve), standing stock with the biomass data extrapolated to the surface (green curve), and chl-a (cyan curve); the black curves superimposed on the curves for standing stock and the blue curve superimposed on the curve for chl-a are the 30-day running means. The unit for standing stock is g wet wt per m^2 and the unit for chl-a is mg/m^3 . All the data are for the mooring location off Mumbai (NEAS). (b) Morlet wavelet spectrum for standing stock (red curve in (a)). The black curve is the cone of influence and the ordinate (period in days) and the colour scale use a logarithmic scale (\log_2). (c) Morlet wavelet spectrum for chl-a. (d) Wavelet coherence between standing stock and chl-a. The phase difference is shown by the arrows, which are marked only if the coherence exceeds 0.5. A positive angle in the anticlockwise direction is the angle by which standing stock leads chl-a, with 360° representing one period. See the reply to Comment 2 of Reviewer #1.



Figure R4. The Dow Jones Index from 1 January 2020 to the end of January 2021. This chart was downloaded from <https://www.statista.com/statistics/1104278/weekly-performance-of-djia-index/>. See the reply to Comment 2 of Reviewer #1.

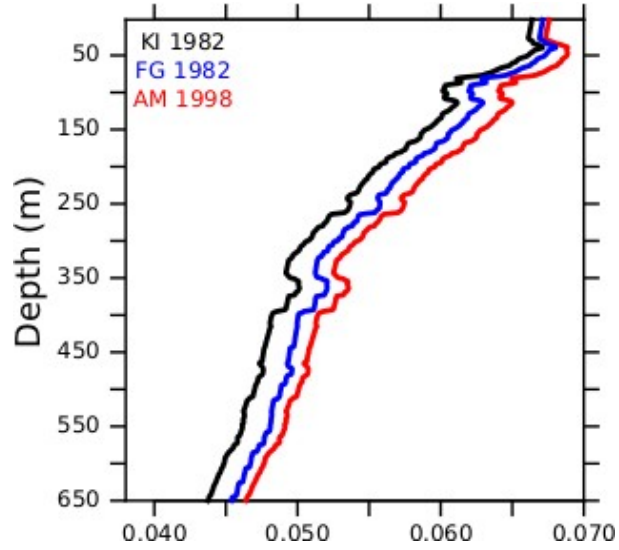


Figure R5. Profile of the sound-absorption coefficient α (in dB m^{-1}) estimated using three different equations (Francois and Garrison, 1982; Kinsler, 1982; Ainslie and McColm, 1998). This vertical profile of α is estimated using the CTD temperature and salinity profiles near the ADCP mooring off Mumbai on 9 November 2017. See the reply to Comment 8 of Reviewer #1.

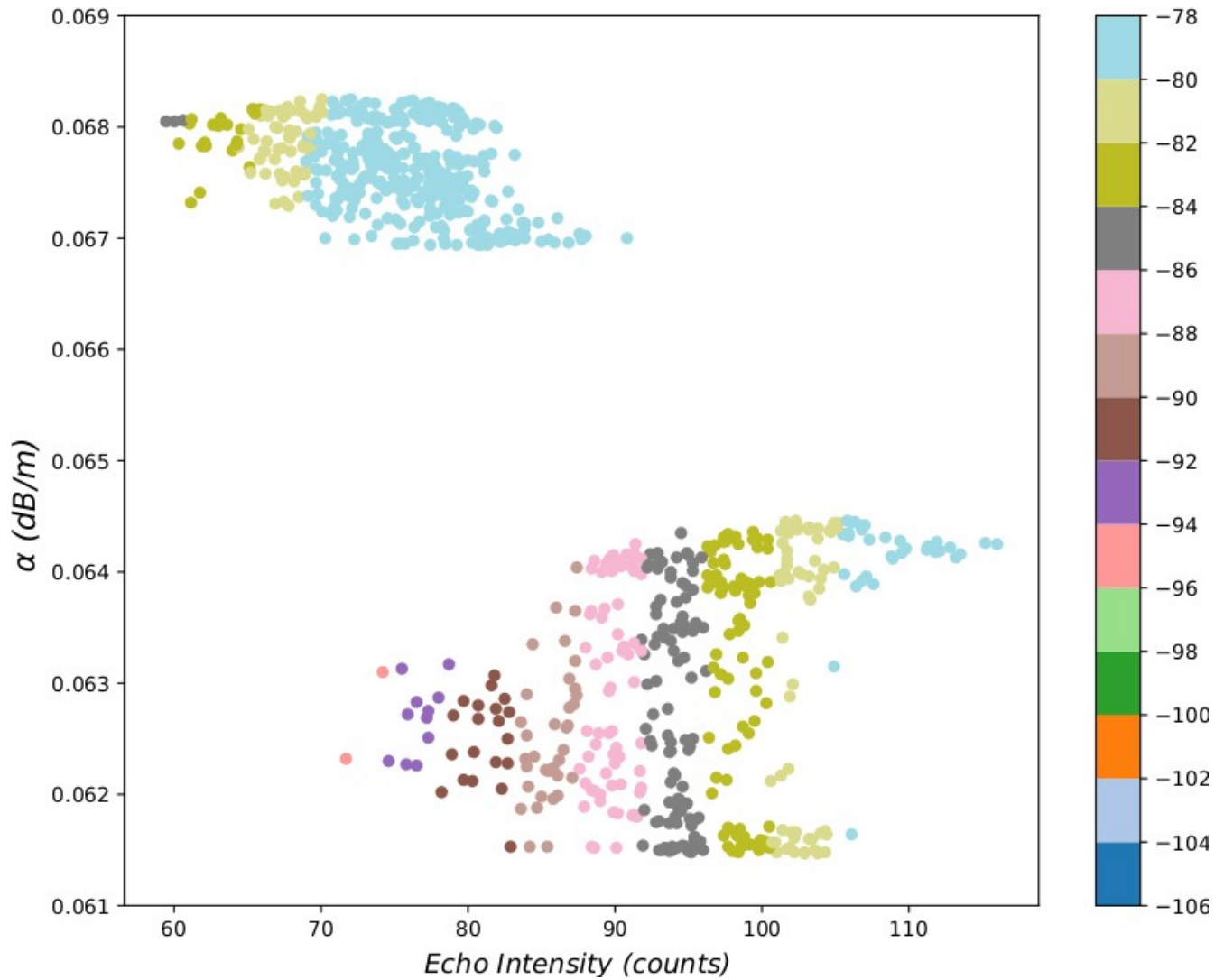


Figure R6. The variation of backscatter off Mumbai during 2014–2016 as a function of α (ordinate) and echo intensity (abscissa). This data in this figure get divided to two layers, with the top layer representing the values of backscatter at 40 m and the bottom layer the backscatter values at 120 m. These two depths are separated on the α axis because α is higher at higher temperature (see Figure R5) and the temperature is higher at 40 m than at 120 m. The figure shows that the backscatter does not change much with α at a given value of echo intensity. Therefore, the impact of α on the backscatter is negligible, as suggested by Heywood et al. (1991). See the reply to Comment 8 of Reviewer #1.

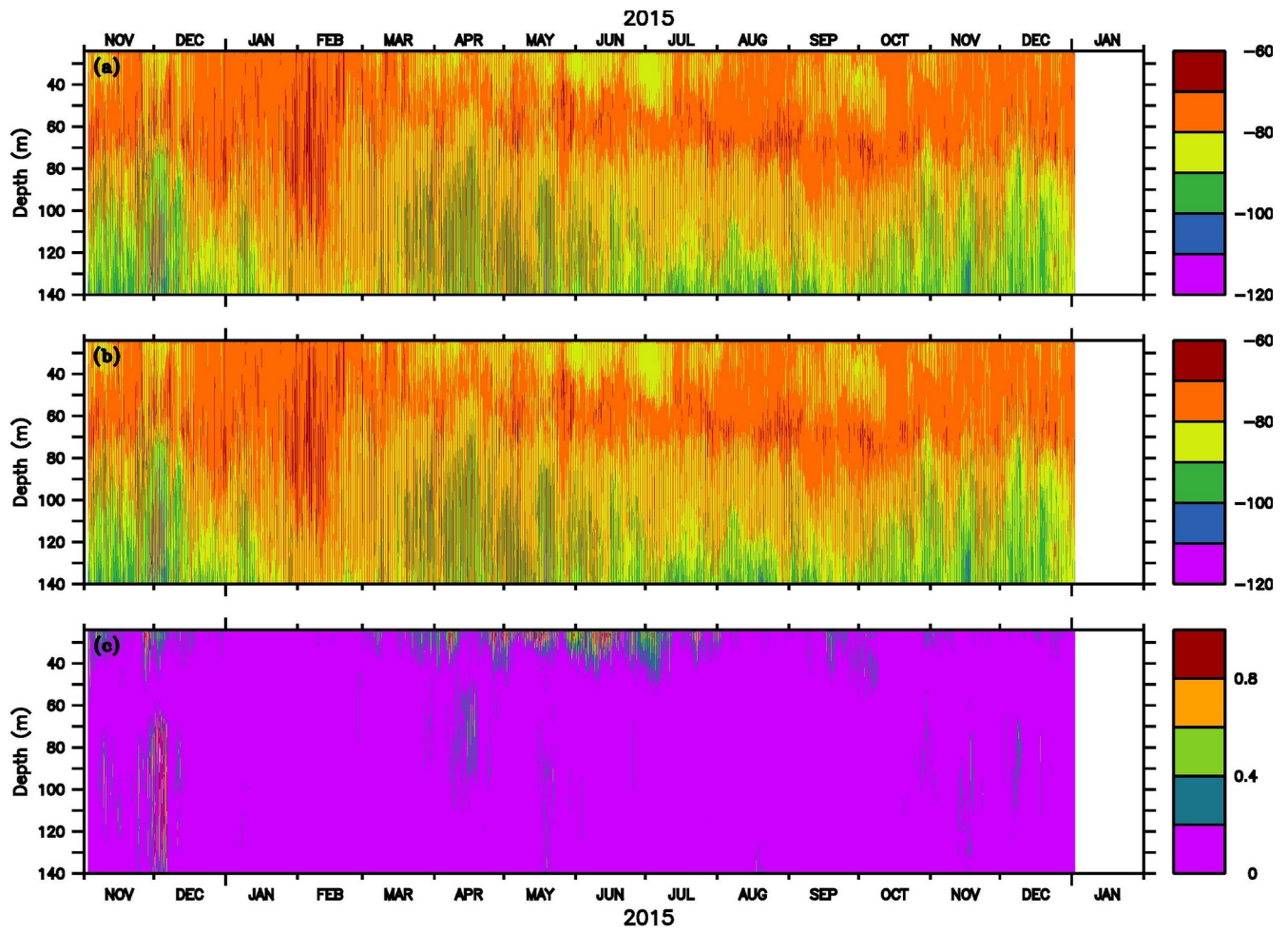


Figure R7. The difference in backscatter for the methods of Deines (1999) and Mullison (2017). **(a)** Backscatter estimated using formula provided by Deines (1999). **(b)** Backscatter estimated using the formula provided by Mullison (2017). **(c)** Difference between the magnitude of backscatter in (a) and (b). All the above figures are for the ADCP off Mumbai for the period November 2014 to January 2016. See the reply to Comment 1d of Reviewer #2.

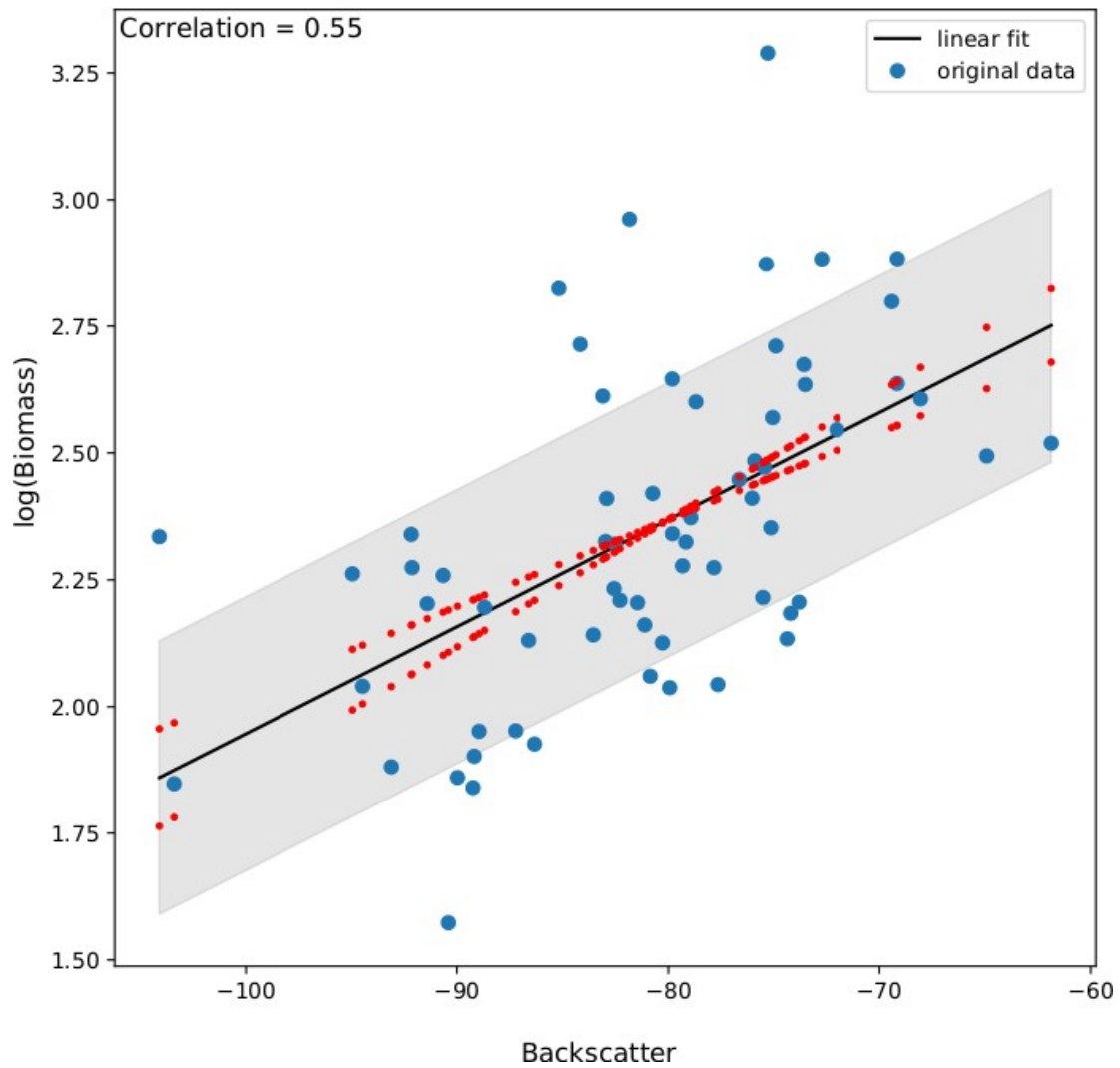


Figure R8. The correlation between the bulk zooplankton biomass (wet weight mg per m³, ordinate) and the vertically averaged ADCP backscatter (dB, abscissa). The zooplankton biomass was estimated from samples collected near the ADCP mooring off Mumbai, Goa, and Kollam during the deployment and retrieval of the ADCPs (see Table 2). The ordinate is logarithmic (log₁₀). The correlation is $r = 0.55$ and the regression equation is $y = (0.02 \pm 0.004)x + (4.06 \pm 0.32)$, where x is the backscatter; the best-fit line is shown by the black curve and the red dots show the fits obtained using the range of the slope and intercept. The grey band shows the root-mean-square error or RMSE. See the reply to Comment 1g of Reviewer #2.

(This figure is identical to Figure 2 in the new version of the manuscript.)

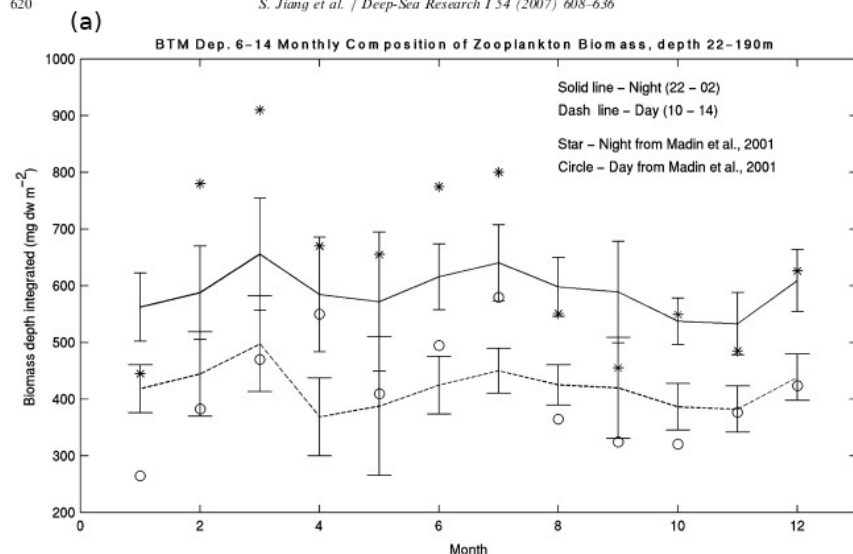


Fig. 5. Monthly composite of depth-integrated zooplankton biomass at BTM compared to similar analysis by Madin et al. (2001) for net tow data. Values are means (± 1 standard deviation) of day and night DW mg dw m⁻² for each month, 1996–2000.

Table 1. Average column standing stock of zooplankton (g dry weight m⁻², day and night combined, column depths as in Figures 2 and 3) in coastal and open ocean waters during different seasons. Average zooplankton biomass (g dry weight m⁻²) for mixed layer is given in parentheses

	April–May	February–March	July–August
Coastal	3.7 \pm 1.7 (1.4 \pm 0.7)	3.1 \pm 2.8 (1.6 \pm 1.3)	3.3 \pm 0.7 (1.3 \pm 0.8)
Oceanic	5.7 \pm 2.7 (1.5 \pm 0.9)	3.1 \pm 1.4 (1.7 \pm 1.2)	3.3 (1.8)

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(b)

(c)

to temperature. Similarly, a seasonal study conducted in the Gulf of Kachchh area showed that the MSP biomass and abundance during December were markedly lesser compared to the collections during February and August (Paulinose et al., 1998). These observations point to a regulatory effect of temperature on zooplankton standing stock in the NEAS. If this could be true, then what would be the fate of the high winter phytoplankton biomass in the north? It is coincidental to notice the occurrence of large number of diatoms floating in the sediment traps, deployed in the winter bloom areas of the Arabian Sea (Sawant and Madhupratap, 1996). This underlines the fact that zooplankton grazing may not be effectively controlling the winter blooms (Sawant and Madhupratap, 1996). This high under grazed winter phytoplankton standing stock could be significantly contributing to the highest carbon fluxes in the northern Arabian Sea (Sarma et al., 2004). However, more work would be needed to understand the extent of temperature regulation on MSP biomass in the northern Arabian Sea.

The most important feature in Fig. 12a is the statistically insignificant variability in MSP standing stock in the mixed layer and thermocline layer during different seasons ($p > 0.05$). Similarly, the seasonal difference in the mixed layer of the inshore and offshore

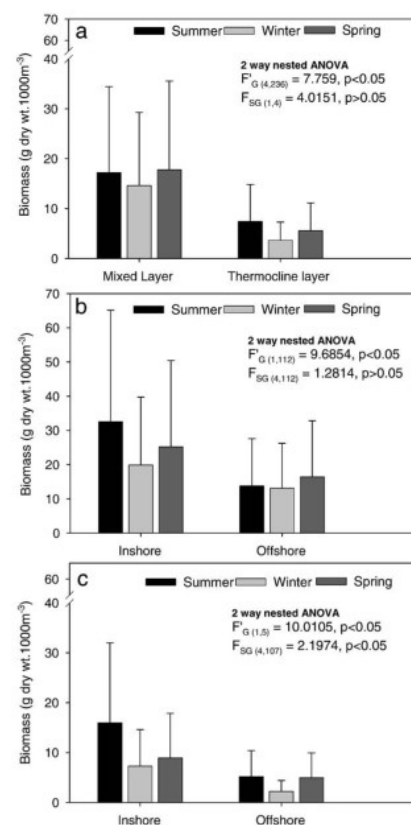


Fig. 12. Basin scale comparison of MSP biomass in the (a) mixed layer and thermocline layer (b) inshore and offshore regions of the mixed layer and (c) inshore and offshore regions of the thermocline layer.

(d)

Figure R9. Text and figures reproduced from Jiang et al. (2007), Madhupratap et al. (1996), and Jyothibabu et al. (2010) in reply to minor comment 4 or Reviewer #2. (a) Figure 5 from Jiang et al. (2007). (b) Table 1 from Madhupratap et al. (1996). (c) The first para on page 247 of Jyothibabu et al. (2010), showing the use of the term “phytoplankton standing stock” and the interchangeable use of “biomass” and “standing stock” for zooplankton; these phrases are highlighted. (d) Figure 12 and the relevant text from Jyothibabu et al. (2010), showing the use of both “standing stock” (text) and “biomass” (caption) for the same figure; these phrases are highlighted. See the reply to Minor Comment 4 of Reviewer #2.