Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2017-89-RC2, 2017

Ocean Science
Discussions

Interactive comment on "Turbulence and hypoxia contribute to dense zooplankton scattering layers in Patagonian Fjord System" *by* Iván Pérez-Santos et al.

Anonymous Referee #2

Received and published: 18 December 2017

The manuscript aims at relating the vertical distribution and migration of zooplankton to physical structures and turbulence in Chilean fjord system. This is a timely and interesting focus. However, I find the manuscript in its present form preliminary and of local interest only. The difficulties are: The objectives of the study appear primarily of technical nature. The relevance and implications of studying the vertical distribution in relation to fine scale properties and turbulent mixing needs to be highlighted in more detail. The introduction and the discussion basically lack scientific questions related to the physical-biological interactions and do not relate to an already large body of literature about detecting zooplankton with acoustic methods or the influence of physical (turbulence) or chemical (oxygen-minimum zones) properties on zooplankton distribu-

tion. The implied effects on reproduction, growth and life cycles in the introduction are not sufficient and appear redundant because the physical and biological processes occur on very different time cycles. Reference to previous work is largely restricted Chilean fjords.

In addition, the material and methods are incomplete and inconsistent. Many details can be found below. It is unclear to me, why hydrographical data from 1995-2015 is presented, while zooplankton sampling is restricted to a few occasions. Data on zooplankton from net sampling in August 2014 is not presented although samples were apparently taken; instead physical data from 2016 is presented although not described in the Methods.

Finally, the authors make very little use of their own data, particularly with regard to the identification of the primary groups responsible for the detected backscattering signals. The zooplankton depth resolved data should be presented and analysed. Data from 2013 suggests that copepods contribute very little to the signal, but the authors treat the backscatter data as equivalent to zooplankton throughout the manuscript. Tremendous differences in the abundance of zooplankton despite similar backscatter signal strength needs to be explained.

In its present form, I cannot recommend considering the manuscript for publication and suggest that the authors revise it considerably.

Detailed comments:

Introduction Line 70: Palma (2008) is missing the reference list Line 74: Landaeta et al. (2013) is missing in the reference list. When microzooplankton and fish larvae were studied, copepods (meso- and macrozooplankton) cannot dominate. Line 80 following: Rephrase the sentence. Why 'although'? What is meant by accurate results? Nets and acoustic methods provide principally different results with high taxonomic resolution in the first and high spatial resolution in the second. Thus, they are used to study different aspects and differ largely in their size resolution. Line 88: Please specify:

Norwegian Channel or Kattegat? Line 89: Buchholz et al. 1995, Zhou and Dorland 2004 are missing in the reference list. Line 89 following: The necessity and need for studying the vertical distribution or migration in relation to physical properties needs to be described better. They are themselves not a scientific question. Line 97: Please specify the implications for reproduction and growth. Yamasaki et al. 2002 is missing in reference list. Line 101: The influence of the described processes (short-term) on biological life cycles (different time scales) needs to be explained. Line 108: What is meant by 'survival strategies present in these organisms'? Line 111: It is unclear to me what the stage-specific migration patterns of Rhincalanus have to do with the effect of fine scale turbulence patterns. Acoustics cannot be used to resolve the stages of this species. Line 115: The introduction lacks a review of the present knowledge about the effect on turbulent mixing and the oxygen conditions on the distribution of zooplankton. What are the scientific questions? What zooplankton is targeted at? Nets and acoustic profilers provide largely different type of data.

Material Methods Line 146: Specify the depths for nutrient samples. Line 201: The description of the echo sounder needs to be checked. On line 188, it says SIMRAD CX 34 at 38 kHz, here it says EK-60 at 38 and 120 kHz. Please specify also how the echo intensity was combined Line 210: Please explain the units (what is n and mi<square>. Zooplankton abundance is usually presented per unit volume, thus the full units should be presented (also of T) Line 217: What is 'Tx' in the formula? Line 238: The sampling in 2013 covered only the upper 50 m but not the watercoulmn of 100 m scanned by the ADCP. Why? Line 249: No information is presented on the analysis of the sampling. From the data presented in the study no differentiation into size classes was performed. Why?

Results: Line 254 following: In Fig 2, the top 100 m should be resolved because the size of the graphs make it very difficult to extract the information on T, S, and the other variables. The legend should be self-explanatory, but it is not. The T at the surface is 15 degrees, the x-axis stops at 14 degrees. The text and figures do not always

match: during Puy V hypoxic water occurred at a depth > 200m and not as implied by the text at >100 m. Line 263: The MatMeth indicate that the sampling covered the period 1995-2015. Now data from 2016 are presented. This is confusing. Why was this data included? Line 275: Sampling was conducted in layers of 10 m depth, but data on zooplankton distribution is averaged. Why? Information on size classes should be presented. In addition: was the abundance integrated as indicated in the figure legend? Then m-3 is wrong. Fig. 4 c does not allow extracting quantitative information on siphonophores. Figure 5: Apparently, zooplankton was analysed in size categories; this needs to be described in the methods. A lot of information is lost by averaging/integration (this is not clear to me; it looks like averaging but integration is stated). I suggest to present the zooplankton data (size, taxa) as in Figure 5a despite a courser resolution. Then, signal and zooplankton distribution can be compared. Negative abundances in Fig c-e are odd. Zooplankton abundance in Jan 2014 (daytime) is several orders of magnitude lower than in 2013 (daytime), but signal strength appears similar or even higher. This needs explanation. Line 303: The authors describe here that copepods and others together contribute to the signal in backscatter. This is not conclusive until the data is shown in high resolution as described above. In addition, a similar analysis needs to be done with the 2013 data, whit apparently strongly diverging results (copepods apparently do NOT contribute to the signal). Line 308: To which depth do the Euphausids migrate to? Hypoxic water? Figure 6: The material and methods say that the signal of ADCP and Simrad were combined (38 and 120 kHz). Which signals were used for the along fjord transects? Is the analysis comparable to the fixed stations? Line 318: '. . . demonstrated a uniform distribution of zooplankton'. This statement implies that the echo sounder provides a quantitative estimate of the groups studied, which is very likely not the case (see comments above). The authors should be more careful. He signal does not show any variation. Line 321: Why figure 6? Line 325: The NASC in the small figure included in Fig 6 is barely readable. How was the signal for fish and zooplankton obtained? The methods do not provide sufficient detail. There seem to be little differences at greater depth depending on the stations. Line

342: this is interpretation of the results, and should not be presented here. Again, I advise to avoid the general term zooplankton because the back-scattering likely represent only a part of the zooplankton. This needs to be extracted from the ADCP and zooplankton sampling. Line 347: The methods state that zooplankton was analysed in August 2014. The data is not presented. Again, avoid to assign zooplankton in general to the backscattering signal. Figure 7: the legend says zooplankton and fish, the figure shows 38FL, 38 BN and fish. How was noise identified? Line 359: Describing the signal is not a confirmation. Line 362: The in-situ (nets?) data is not shown. How can Euphausiids attributed to the signal? Line 371: What is meant by 'in-situ plankton sampling'? The echo-sound data? Zooplankton sampling with nets was conducted. The data is not shown. Line 373: The analysis needs explanation in the introduction and the methods. Why is a correlation between Sv and T to be expected? Fig 10 e and f: The methods do not describe how the measurements of energy dissipation with a resolution of 1mm where integrated to match the resolution of the backscatter analysis of 1m. Fig 11: Why is this presented?

Discussion: There is quite some literature on the relationship of zooplankton distribution in relation to oxygen minima and the relationships of zooplankton distribution and echo-sounder signals. These need to be explored. What do the present results add to these studies? The authors use their own data very little to explore the identity of the backscatter signal and to provide an analysis of general interest about the influence of physical factors and zooplankton beyond a local description. This needs to be conducted before any conclusions on implications of their relation to turbulence and implications for vertical flux can be made. Line 508: How the authors come to the conclusion that copepods cause the backscattering signal in the deep, hypoxic layer is unclear to me.

Interactive comment on Ocean Sci. Discuss., https://doi.org/10.5194/os-2017-89, 2017.