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Interactive comment on "Dead zone or oasis in the open ocean? Zooplankton distribution and migration in low-oxygen modewater eddies" *by* H. Hauss et al.

Anonymous Referee #2

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Overall the manuscript provides novel data on distribution of planktonic species in an extreme environment, which may, as the authors suggest, have high importance especially in light of predicted future changes in the pelagic realm. The manuscript is largely clear and well presented, but I believe that the authors draw conclusions that are not (reliably) supported by the presented data, this can potentially be remedied by a shift of focus or by presenting additional data.

In general I hesitate in criticizing a study for not including additional factors (as that critique can almost always be made), but I feel that it may be warranted in this case,

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the way the present manuscript is written the authors seem to have a very narrow focus. My concern is that the manuscript relies on oxygen levels as the single explanatory variable, but the situation described is obviously advectively complex (e.g. fig. 1). Effects of other factors are largely omitted, for instance the manuscript refers to the ACME as having elevated chlorophyll levels, if the authors can suggest that evolutionary forces are at play inside these features, wouldn't a natural starting point be to look at whether the increased densities of zooplankton are affected by the enhanced production directly? In addition differences in chlorophyll levels have profound effects on the optical properties in the water-column, light levels are another main forcing factor for vertical distributions. The manuscript would be substantially improved if the authors included data that could shed light on also the other factors mentioned in the first line of the introduction. In addition it would be helpful with some more background information for those of us who are not experts on eddies: what is their average life-span, how ubiquitous are they, how old was this one etc.

My other main concern is that the authors are comparing apples and oranges: their physical sampling is geared towards the mesozooplankton components (Multinet and UVP), whereas their acoustic data is a mixture of components. For the 300 kHz ADCP data it could possibly be argued that smaller zooplankton forms (e.g. from upper mesozooplankton size range) are likely to contribute to the acoustics, the authors state that 5 mm (i.e. the transition between Rayleigh and geometric scattering regions) is the lower size range observable. This will however depend on actual organism densities and scattering characteristics, and for instance aggregates/siphonophores with gas inclusions could be expected to contribute at 300 kHz, as well as at the lower frequencies, especially close to resonant frequencies. It is not very controversial to suggest that for the 38 and 75 kHz data the contributions from crustacean plankton are not likely to be significant in the total, possibly with the exception of larger forms, but these are again not likely to be sampled by the Multinet or UVP, leaving the organisms behind the acoustic backscatter profiles largely unresolved. The authors do have a section discussing these topics (esp. for the 300 kHz data), but the conclusion from this section does not seem to have an impact on the interpretation of the rest of the results.

Both the multinet Midi and the UVP have too small sampling volumes to be expected to sample the acoustically observed components representatively, the organisms behind the vertical acoustic backscatter profiles are therefore largely unresolved. In itself this is not a major problem, oceanographic surveys are frequently undersampled, but the authors largely treat the vertical profiles from the acoustic instruments as zooplankton profiles, which is not likely to be correct. The authors actually address these factors in a satisfactory way in one section of the discussion, but they need to more clearly separate the analysis of zooplankton and acoustic data, and not use these datasets interchangably.

In my view, the manuscript needs major revision before publication, though that major revision may not actually be that much work. From my point of view, I would divide the discussion part the manuscript more clearly into 2 parts, one dealing with the groups where (multinet and UVP) data are of sufficient quality (e.g. smaller forms), and one part dealing with the acoustic data (and not bother with more than a cursory guess at what organisms contributed to the backscatter). The parts of the discussion pertaining to the vertical and horisontal distribution of euphausiids (and larger, quick swimming forms) does not seem credible to me, given the data currently presented. I suggest that the authors either drops this group, or presents further supporting data.

General comments:

Unfortunately nomenclature differs slightly between different fields of marine research, but normal usage of the term “Target Strength (TS)” in fisheries acoustics refers to the backscattering strength of a single organism. In this manuscript, as in some other studies using ADCP’s, this term (“TS”) appears to refer to Volume backscattering strength (e.g. Sv), i.e. a logarithm of the sum of backscattering from all organisms within some volume. I suggest that the authors change the wording to comply with standard usage of terminology in fisheries acoustics to avoid confusion, or at least define their usage in

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the methods section. I also assume that the authors work with relative values of these quantities (several instruments at different frequencies), though it is not very clear from the description. The authors should also ensure that their usage of "TS" is internally consistent, the range in figure 2B is 90 to 50 dB, while range presented in figure 3 is -90 to -50 dB (some form of inverse values presented in fig. 2?).

The authors mix the terms diurnal and diel, which is perfectly valid, but it would be preferable if they settle on a single term.

Specific comments: Abstract:

L. 4: I would rephrase to something like " are expected to decline under future expectations of global warming"

13-14: Sentence is unclear to me: reduction in values compared to daytime or outside of the ACME? Or low backscattering levels at OMZ depths during nighttime?

28 → As far as I see the habitat compression you observe is based on the acoustic data. As you note in the Methods section, the acoustic results probably reflect a wider range of organisms than just mesozooplankton (and the mesozooplankton is not covered well), so I would suggest moving this section out of the abstract, as it is speculative, given that your other data on the larger components is scarce. Still an important finding, and a good example, but I don't think you have shown it for the zooplankton component (in addition your N is low).

P. 18318, L 4: last part of sentence seems awkward to me, but english is not my first language.

P. 18321, L6, repeated information (e.g. 90 min)

P. 18326, L21-26. First you state that the Multinet and UVP do not quantitatively sample euphausiids, then you state that UVP data suggest that euphausiids avoided the OMZ. To me this is a bit sketchy. My claim is that neither UVP nor Multinet data is suitable for studies of euphausiid distribution, unless dealing with larva or very small forms: how many of the mean values – 1.96*sd presented for euphausiids in figs 4 and 6 would span 0? Your scale of aggregation seems to high for this group (in figure 4). Looking at the figures, figure 5 seems to support your conclusions (horisontally), but this is data based on a total scanned volume of < 7 m3 per profile, for a "normalized" volume of 600 m3, with a density of 100 equalling $\sim$1 observation, if I'm correct? This implies that the actual observations for figures 5 c,d,e,f are all considerably fewer than 40 observations per profile, which seem to be very low numbers to draw strong inference about distribution from, or have I misunderstood? Have you performed a power analysis? Why not use the Multinet data for this figure (fig. 5), or a combination of these 2 datasets, the multinet should at least have a significantly bigger volume sampled.

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