

Interactive comment on “Growth and production of the copepod community in the southern area of the Humboldt Current System” by R. Escribano et al.

Anonymous Referee #2

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The submitted work describes the spatial and temporal variation of copepod biomass in an upwelling and in further offshore areas in the southern Humboldt Current. Using environmental temperature as the main predictor for copepod production and hitherto largely unpublished data on growth rates of some copepod species occurring in the area, the authors compute the annual production of copepods with a large focus on a fixed station in the upwelling area. Results show that biomass and production is not related to environmental variables and that production is largely determined by copepod biomass which might be controlled by advection processes. Although very interesting, I find the submitted manuscript in its present state only preliminary as indicated by the large number of questions and remarks related to methods, procedures and interpreta-

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tion. Of these, lacking detail in the description of the estimation of growth rate and the lacking inclusion of food conditions in the spatio-temporal analysis is my major concern. The growth rates used in this study to estimate production are largely unpublished, but central to the understanding of the study. I miss, therefore, detail on the conditions and methods used to determine these. Moreover, the estimates are largely for calanoid copepods while cyclopids and other small copepods were not included although they partly form an important part of the copepod assemblages. Although explicitly stated that the growth rate g was estimated at variable food conditions, no data is provided and apparently not included in the estimates of production although food conditions were much more variable than any other environmental factor. Because of this, the study contributes only little to the discussion whether temperature alone or in combination with other factors determines copepod production – a major question raised in the introduction. Apart from these fundamental issues, there are many other smaller issues that need improved before the quality of the study could completely assessed.

Introduction:

The introduction would benefit from a more explicit description of state- f the art or results which are generally obtained: (see for instance remarks p 3059, line 11)

p 3059, line 1: ‘not’ instead of ‘no’; ‘making it. . .’ instead of ‘making. . .’ (Spelling should be checked throughout the MS)

p 3059, line 11: ‘. . .different results obtained. . .’: Please specify what is meant here; do comparative studies provide evidence that results depend on the method applied. How large is the variation?

p 3059, line 14: Mitra et al is missing in the references

p 3059, line 29: Please specify what is meant by ‘specific approaches should be adopted. . .’ I am not sure if the authors mean that methods specific for an area should be developed (although there are plenty. . .) or if a general method should be sought

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that could integrate the effects of T, body size and food conditions.

p 3060, line 3: Please clarify: Do you argue here that methods applying to the needs of different groups of zooplankton in a specific area are required? Otherwise there is a contradiction between the statement that T is a general predictor for growth as described in the previous paragraph and the statement here that direct estimates of growth barely reflects this relationship.

p 3061, line 11: Were different food conditions not tested?

Material & Methods

p 3061, line 23: I find Table 1 a bit redundant as it repeats much information given already in the text. The period and seasonal condition could be easily accommodated into the text.

p 3061, p 3062: I wonder why wind data and upwelling index is important information for the assessment of growth and production. This information might be available elsewhere and thus referred to if necessary.

p 3062, line 19: I miss some information on how vertical heterogeneous data for T and Chla was treated to comparison to the integrating zooplankton samples to establish those environmental factors used in growth and production estimates. A few sentences on how the zooplankton abundance was estimated would be helpful, too.

p 3062 line 23: With the length-weight regressions at hand, I wonder why the authors did not cumulated the biomass from each stage because using the mean length is much more inaccurate. From which environment do the length weight regressions come from with regarding the effect of environmental conditions on length – comparable to the conditions in the present study? Table 2 does not provide the mean body size as indicated in the text.

p 3063 line 6: In the introduction, the authors state that the first goal of the study was to assess growth rates in relation to environmental conditions. Here, however, they state

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that growth was estimated from the application of published growth rates, which – except for one species – have been determined for one T condition. Later on learns that especially the coastal zone is partly dominated by cyclopoids and poecilostomatoids for which no data is provided here (which also could give rise to a lacking relationship of copepod C-specific growth and size).. How can the effect of size and T on the C-specific growth rate be tested for environmental conditions with variable food and T conditions (<12 - >16), then? This does not make sense to me. What about food conditions in the published estimates? This is more important because food conditions are much more variable together with DO than temperature on a spatial scale. Moreover, since much of the data is unpublished, more information on the methods of which 'most applied the molting rate method' should be provided.

p 3063 line 22: Please provide information on the amplitude of seasonal variation of T and food at the time series station.

p 3064 line 5: May be I missed the point, but how can the relationship between g and temperature and body size be tested when a) g is not estimated in relation to the environmental variability in the area but taken from the literature and when b) the underlying estimate of g originates from a fixed T? As a results production is expected to be driven by biomass. In addition, it remains largely unclear how the effect of body size is tested here.

Results:

p 3064 line 23: The isolines in Fig 3 are very difficult to read. Why are DO conditions described in detail here, when they are of no relevance for interpretation of growth/production estimates?

p 3065 lines 11-24: Similarly, results on upwelling could be condensed to the necessary.

p 3066 line 16: According to Table 2, g has been studied at fixed T conditions, while

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the food conditions are not defined and body sizes are likely given for the different developmental stages. According to the text, however, g has been studied at variable T and food conditions. As most of the data is unpublished, Table 2 should provide g for the variable conditions. This is also necessary to calculate g and production over the seasonal/spatial scales. Moreover, length of copepods (especially those present the whole year round) will vary according to T and food conditions. Is this irrelevant for the estimates of growth and production? Please indicate also which studies are those which are those indicated as 'most' simulating the growth in the laboratory and for which species. Finally, - as already mentioned earlier - it is quite evident from the table that only 50% of the species present in the area are represented by the species listed in Table 2, of which none belong to cyclopoids or other smaller groups.. One species is listed which apparently is not a dominant species in the area (*E. inermis*).

p 3066 lines 17: This figure is confusing. First of all, is the data used here from the laboratory experiments indicated in Table 2? This is not clear as it is stated in legend of Fig 6 for 'copepods from the upwelling zone'. Were food conditions saturating or irrelevant because they are ignored? However, the text states (line 13) that the studies simulated a variety of temperature and food conditions in the field. Why is the size of adult females basis for the estimate? Are the growth estimates in Tab 2 also based on egg production measurements? I also wonder if any allometric relationship should be tested with body mass rather body length. This all needs clarification in the Material and Methods.

p 3066 line 21: But seasonal data shows that T is much less variable than for instance food conditions (see Figure 5), so why is T thought to be main variable determining growth? I am again confused: g was estimated in the laboratory, so how can the 'influence of in situ temperature be tested'? This would be possible if field data to estimate g is used, but what about food conditions then? This should be clarified in the Material and Methods. Fig 6b shows quite a big variability regarding the estimates of growth for one species at a given temperature (by a factor of 8, which is very high), is

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this because variable food conditions have been included?

p 3067 line 4: The calculation of annual means is unclear: Cruises in Nov-Dec were used as the basis for the calculation of spatial variability. How are annual means produced from this data? And I wonder why any spatial trends is removed by this procedure which could give more, interesting insights into upwelling vs off shore differences in the control of production. Moreover, the time series offers the opportunity to describe the temporal evolution of production in relation to the environmental variability described in such a great detail in the earlier results.

p 3067 line 8: That copepod biomass is the main driver of production is not surprising considering the low variation in T and that in principle the grand mean of estimates of g is used to calculate production. However, the coastal upwelling zone is characterized by large temporal fluctuations in food concentrations. Is this irrelevant?

p 3067 line 8: Zooplankton sampling was integrating depths from 0-200 m, but apparently surface environmental conditions have been used to calculate growth and production. This needs to be justified.

p 3067 line 8: Again, potentially interesting spatial trends in the size composition of the copepods are removed.

p 3067 line 21: If I understood correctly, not integrated annual production, but average production was determined.

p 3067 line24: What is the difference of annual and daily P/B ratios and how can there be a difference of one order of magnitude? Since they are averages, shouldn't they have a similar magnitude?

p 3068 line 2: The procedures of the stepwise linear regression need to be explained in the material and methods. The rationale behind this procedure need to be specified, especially because neither Chla as food nor dissolved oxygen were variables in the estimation of environmental factors on growth!

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Discussion:

p 3068 line 22: This description would benefit from less general, but more specific summary of the species distribution because production is driven by species. Which are the dominating species in the respective areas/zones? The used methods need to be critically evaluated as well: 200 μm nets largely underestimate the biomass of small cyclopoids and poecilostomatoida (Oithona, Corycaeus, Oncaea) as well as that of the smaller copepod stages of calanoid copepods. This can introduce a large error into production estimates.

p 3069 line 1: For me it is largely unclear how food conditions are reflected in production estimates because food is – based on the present description of methods – not a factor in the calculation of production although it is the most variable factor. Food conditions offshore also vary by a factor of 20 (0.2 - > 4 mg Chl a /m 3 and I wonder if this has no effect on production estimates.

p 3069 line 1: Actually, it is correct that production is the highest in the upwelling zone; however, this production is driven by biomass partially at very low food concentrations. Is this realistic? Moreover, the spatial study has been performed during 1-2 months only. So very little can be said about maxima and the variability occurring offshore..

p 3069 line 13: The time series data on CP does not show any relation to Chl a which contrasts with the statements here; I wonder also whether this statement is correct as annual means (based on sampling in one month) were apparently used to establish the relationship. Please specify in more detail those processes by which oxygen and OMZ could affect productivity and potentially explain the lacking correlation with Chl a .

p 3069 line 20: Here, the method to estimate production should be critically evaluated especially with regard to serious underestimation of small copepod biomass and lacking estimates of g representing this group.

p 3070 line 10: Food showed a much more pronounced seasonal, spatial and interan-

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nual variation: Why is this factor ignored although there is evidence that food conditions could have an influence on growth and production as stated in the introduction.

p 3070 line 13: Adult copepods have been apparently used in estimates of growth (at least from table 2). How was this done?

p 3071 line 1: Although it is appealing to attribute the variation in production to dilution of the biomass, the lacking relationship of biomass to any of the studied environmental variables needs explanation.

Figure 13 needs more explanation

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