

## ***Interactive comment on “Nitrogen and phosphorus recycling mediated by copepods in Western Tropical South Pacific” by Valentina Valdés et al.***

### **Anonymous Referee #1**

Received and published: 28 February 2018

In the role of zooplankton in nutrient regeneration, metabolic studies are always important and useful, especially when they concern areas with relatively few published data, measurements of multiple parameters, and effects on the microbial community. Generally, there are some nice parts in the manuscript, drawing together from diverse sets of original information. However, there are important issues to be dealt with, prior to publication.

#### 1) MAJOR COMMENTS

My main concern is the use of data from only 3 time-space points (3 stations in spring 2015 and water collected only from DCM) to make conclusions on the importance of

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copepods in N and P recycling for the whole Western Tropical South Pacific (title, p. 13 lines 22 to 24). These 3 stations are located (stations coordinates not given) on a transect > 1300NM long, thus covering a very vast area. To support the potential in situ applicability of their conclusions, the authors should give strong evidence (based on other manuscripts of the same issue or at least based on previous bibliography) about the temporal and spatial (horizontal and vertical) homogeneity of this vast area.

My second concern, which enhances the first one, is using an experimental ratio of copepods to bacteria > 13:1 compared to the one in the field (p.12 lines 2 & 3), to make conclusions on the in situ effect of zooplankton on the microbial community. It is fully understandable that high experimental densities of copepods are necessary to obtain a signal in nutrients within a few hours. However, it is doubtful that if the nutrients available to bacteria per time unit were much less, that the remineralisation effects and shifts in bacterial composition would be the same (especially considering also the role of the other player - phytoplankton).

In brief, the transfer of lab observations under such experimental conditions and from only 3 space-time points to processes occurring in the field at a very different copepod : bacteria ratio and over vast spatio-temporal scales is not at all straightforward. My suggestion is to rewrite conclusions in a much more conservative way. Then the manuscript including title should be adapted in consequence.

#### 2) SPECIFIC COMMENTS

##### 2a) Introduction :

p.2 lines 20, 25, 26. Copepods may excrete much more than 53% of their body nitrogen in the form of ammonia and this percentage is highly variable (as said in line 25). There are many papers on this subject, including review papers.

p.3 lines 16-17: How is the presence of an oligotrophic gradient supported? Nutrient values from the only three stations are insufficient. Nutrient and/or Chla data from more

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stations would be helpful.

#### 2b) Methodology:

General remark for the experimental set up: an additional control with copepods only would have been very helpful.

p.3 line 22 : please define "long duration"

p.3 line 22 : please add coordinates of the stations

p.3 lines 24-25 : please add maximum and minimum values of chlorophyll-a

p.4 lines 2-4 : please specify if tow was vertical (or oblique), tow speed, net diameter

p.4 line 13 : the field composition of copepods and other zooplankton should be also shown in Table S1

p.4 lines 23-24 : please specify % of mortality

p.8 lines 21 : check that Redfield ratio of organic nutrients is < 16:1

p.9 lines 7-8 : unclear meaning

#### 2c) Results:

General remark. Do not repeat values that can be found in tables or figures unless necessary.

Table 1 : use either 2 or 3 decimals depending on the precision of the method for each parameter

Table 1 : add Temperature, Chla, DON, DOP, values at DCM

p.7 line 25 : "first treatment", replace by "treatment with copepods"

p.8 line 4 : "significant difference in time,". Please add results of statistical test

#### 2d) Discussion:

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General remark. Do not repeat detailed description of results, but only briefly giving outcome in connection with related literature.

p.10 line 15 and line 17: Table 1 shows that MA stations (LD A & LD B) are characterized by higher inorganic nitrogen but not higher inorganic phosphorus concentration than SG (LD C).

p.10 line 25: "influence on biogeochemical variability": not clear.

p.10 lines 31-32: Since there was no significant difference between treatments in LD A for bacterioplankton abundance (p.8 lines 26-27), a conclusion on an effect of zooplankton cannot be made.

p.11 lines 7 to 10. LD B is not in the South Pacific Gyre (p.1 line 24). Check and eventually move this part to discussion on LD C which is in the South Pacific Gyre.

p.13 lines 33-34: Too general conclusion on the role of zooplankton metabolism, not supported by manuscript's observations.

#### 2e) Bibliography:

References to articles in preparation should be avoided

### 3 ) TECHNICAL CORRECTIONS

p.2 line 2, p.8 line 18, p.10 line 12, p.10 line 15, p.12 line 32, p.13 lines 8-9: English grammar mistakes

p.2 line 10 : change order of references

p.3 line 3 : "...in a phosphorus limited..."

p.3 line 5 : "...Gasol, 2007"

p.3 line 8 : "...zooplankton-produced DOM by bacterial communities..." : unclear expression

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p.4 line 14 : "acclimation" instead of "acclimatization"

p.4 line 21 : "such that 6 bottles were added in that case" : not clear

p.5 line 23 : correct "u sing"

Please correct typing mistakes and journal names abbreviations in bibliography

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Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-563>, 2018.