

Interactive comment on “Role of *Calanus sinicus* (Copepoda, Calanoida) on dimethylsulfide production in Jiaozhou Bay” by Juan Yu et al.

Anonymous Referee #2

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The manuscript “Role of *Calanus sinicus* (Copepoda, Calanoida) on dimethylsulfide production in Jiaozhou Bay” by Juan et al attempts to understand the role of copepod *Calanus sinicus* in DMS production in Jiaozhou Bay through insitu observations and lab experiments. The authors followed a yearly cycle of insitu observations on temperature, salinity, Chl a, TBC, zooplankton enumeration and speciation, DMS, DMSPp and DMSPd at 10 stations in the Jiaozhou Bay. They also performed lab experiments wherein they conducted zooplankton grazing experiments on select phytoplankton species to see the impact on DMS production. Though the hard work put in by the authors is commendable, there is a major disconnect between observations and lab experiments. Their observation on DMSP transfer from phytoplankton to copepod body, fecal pellet to seawater is not new and has been proposed quite some time back (Tang et al;

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Belviso et al). The authors mention that data from the field measurements showed that *Calanus sinicus* did not have any apparent effect on DMS/DMSP production and then the authors go ahead to perform a complex grazing experiment to see the impact of grazing on DMS production. If the field data did not show any connect, what was the aim to perform the lab experiments? Perhaps the authors should have checked the gut content to see what species the copepod preferred to feed on? This might have given some clues on how to proceed. Also, the insitu observation part is not clear. Some of the concerns with regards to this work is jotted below. The authors mention time series sampling at 10 locations, but figure 2 shows data for only one site, which is this site? Or is this averaged data? If averaged then include standard deviation. What is the reason for the increase in DMSPp&d (and marginal increase in DMS) during September 2010? What is the major phytoplankton species during spring? As this might have answered the high DMSP observed during that time. In terms of copepod, the authors mention *Calanus sinicus* as the predominant copepod, but that does not seem to be the case as *Eurytemora pacific* was also dominant during three sampling with April 2011 showing maximum abundance. In the feed (diet) experiment it is clear that the copepods prefer *I. galbana* and *C. curvisetus* compared to *E. Huxleyi* and *Gymnodinium* sp. There is not much difference in DMS production in the treatment when compared to the control. On the contrary DMS production dropped in the treatment in the case of *E. Huxleyi* and *Gymnodinium* sp. in comparison to *I. galbana* and *C. curvisetus* which showed marginal increase in DMS production. More queries and corrections are mentioned below. Line 26: ...in May 2011) and had no apparent Introduction: Line 37-38: The authors mention that there was close scrutiny on DMSP, what kind of scrutiny, please elaborate. Line 48: what kind of biotic and abiotic factors? Elaborate. Line 49: replace 'account' with 'consume' Line 51: the authors mentioned that role of zooplankton grazing on DMSP biogeochemical processes are scarce. And later in the same paragraph include detailed studies on the impact of zooplankton grazing on DMS production. The aim for carrying out the grazing work needs to clearly spelt out. Line 62: delete 'Brotsky' Line 65: what field experiments were performed? Are the

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authors referring to field measurements? Materials and methods: The authors mention collection of samples from 10 stations. I assume these are surface samples? How were the samples collected? Niskin sampler or any other sampler? Zooplankton samples were collected by vertical tows, mention depth or range of depth from where the vertical tows were done Line 91: spelling correction 'Whatman' GF/F filters There are many grammatical errors in the manuscript. Only a few are pointed out. The authors need to correct these. Line 334: Did the authors measure acrylic acid as a deterrent against grazing? Line 345-346: 'In this study, which in turn reduced DMS/DMSP production'. This does not seem to be the case as DMS production was high in *Gymnodinium* sp and *E. Huxleyi* as seen from control and did not depend on grazing. Figure 1: Include latitude and longitude or specify 'N' and 'E' Figure 3: (B) may be deleted as species wise in shown in (C) One of the important parameters that this work lacks is phytoplankton speciation of the natural samples. In the absence of that data, understanding DMSP variation becomes difficult. Grazing by zooplankton on phytoplankton is an important part which results in DMSP going from particulate (within cell) to dissolved (outside) and further the action by DMSP lyase (both by phytoplankton as well bacterial lysis) results in high DMS production. Grazing is studied by looking at the gut content or by isotopic work, neither being done in the present study, it's difficult to address DMS production to grazing. And finally, there is a complete disconnect between field results and the basis for performing grazing experiments in the laboratory. Thus, I do not recommend the manuscript for publication in its present form.

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