

## ***Interactive comment on “The Effect of the 2013–2016 High Temperature Anomaly in the Subarctic Northeast Pacific (The “Blob”) on Net Community Production” by Bo Yang et al.***

### **Anonymous Referee #2**

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This paper gathered results of comprehensive measurements on biogeochemical and biological changes in OSP associated to the appearance of the "Blob." The authors skillfully estimated temporal variation of net community production by compiling the data that comes from various state-of-the-art platforms and sensors deployed among OSP. I agree most of their model calculation and their obtained ANCP estimation. Observed ANCP discrepancies dependent on the materials used (ca. oxygen and/or DIC) are acceptable, when considering their assumption that ignores DIC variation between mixed layer depth and pycnocline. However, I feel that there are several insufficiencies in the present manuscript on their interpretation of the results.

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[major comment] 1. They attributed the observed ANCP decrease between 2012-13 and 2013-14 to the changes in gross primary production, by the process of elimination. However, there is no direct evidence in the present manuscript that the estimated decrease of gross primary production is caused by the low plankton biomass observed in 2013-14. Plankton biomass may regulate production rate, but this does not necessarily mean reduction of ANCP. In principle, ANCP is regulated by annual availability of limitation factor, likely iron in the case of OSP. If the amount of available iron had been same in each year, low plankton biomass in 2013-2014 would have diminished rate of primary production, but that simultaneously enhanced duration of high-production period by postponing the timing of iron exhaustion, and integrated amount of gross primary production would become just same amount. This is always true, as long as both iron availability and biological stoichiometry (Fe: O : C) are same. If the authors want to link observed low plankton biomass in 2013-14 and estimated low ANCP at that time, therefore, they need to prove that 1) there is no significant difference in iron availability during the observation period, and 2) there may be significant plankton-dependency in Fe:O:C stoichiometry.

2. The model used in this paper is only effective when there is no change of water mass during the calculation period. However, significant increase of water temperature at the emergence of the Blob implies the readers that there may be some intrusion of warm water. As not all readers are familiar with the physics of the Blob, the authors should mention briefly about that and declare continuity of water mass during the study period.

3. The authors raised several possible cause about the discrepancy between oxygen-based ANCP and that based on DIC, but none was mentioned about the inter-annual variation of POC/PIC production ratio that can make such discrepancy. Although we can understand from Figure 5 that no significant bloom of coccolithophore during the observation period, the authors should clearly mention about this in the text.

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