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Interactive comment on "The effects of different environmental factors on biochemical composition of particulate organic matters in Gwangyang Bay, South Korea" *by* Jang Han Lee et al.

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Interactive comment on "The effects of different environmental factors on biochemical composition of particulate organic matters in Gwangyang Bay, South Korea" by Jang Han Lee et al.

Anonymous Referee #1

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Review of Biogeosciences Discuss., doi:10.5194/bg-2016-347 The effects of differ-

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Discussion paper

ent environmental factors on biochemical composition of particulate organic matter in Gwangyang Bay, South Korea written by Jang Han Lee, Dabin Lee, Jae Joong Kang, Hui Tae Joo, Jae Hyung Lee, Ho Won Lee, So Hyun Ahn and Sang Heon Lee In the submitted article the authors analyzed seasonal changes of the biochemical composition (proteins, lipids, carbohydrates) of the particulate organic matter and linked it to environmental factors in order to determine the major environmental factor influencing the changes of biochemical composition and the origin of particulate organic carbon. In general, the paper has a scientific potential and some parts of the paper are fairly discussed (biochemical composition) and linked to the relevant literature. However, some parts of the sections Materials and methods, Results and Discussion are not clearly outlined or missed important information that complicate understanding of the text and question the purpose of applied experimental design. The conclusions are mostly repeating of the results so it should be also rewritten and the last paragraph omitted, it is too general and does not contain the conclusion of the paper. The major revision and resubmission is recommended. =>We revised each section throughout the manuscript, deleted most of repeating results in conclusions and revised carefully our manuscript based on referee # 1 comments as below (see supplement).

The experimental design was based on three different light intensity depths along three stations in bay and all results were pooled together on the monthly basis since no significant differences between vertical and spatial distributions were found. It was mentioned in the Material and methods that some statistical tests (ANOVA, t-test) were used, but it is not clear which test they used, where and which parameters they tested and how (there is 1 concentration per 1 depth at 1 station). => We did ANOVA test for each depth from 3 stations based on an assumption of no spatial difference and another ANOVA test for a spatial difference by pooling of 3 light depths at one station and comparing each station based on an assumption of no difference in light depths. But, we found to realize that there are statistical errors by doing that. So, we deleted no significant differences between vertical and spatial distributions.

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The authors used very often describing results the word significant but did not specified the name of test, F-value or t-value. =>We revised our results.

Details and reference about determination and/or calculation of the 30% and 1% of the photon flux based on Secchi disc depths should be added. =>We added details and reference for light depth determination in line 87-90, page 4-5.

It was only mentioned that the samples were incubated and later on in discussion it was written that the incubation time was too short? =>Our main purpose of the PAR measurements was calculating hourly primary productivity executed for 4∼5 hours as a parallel study. Therefore, the irradiance values measured in this study were not representative for our sampling periods. We mentioned this in the method section in line 92-97, page 5 and further discussed on the issue in line 370-379, page 16.

Further on, the light intensity and its impact (or no impact) on the biochemical composition is not discussed, particularly considering 10 times difference in light intensity between April 2012 and April 2013. These findings should be discussed with regard to a body of literature in which the influence of light was investigated and found. =>We added the discussion on the light intensity impact on the biochemical compositions, especially 10 times difference in light intensity between April 2012 and April 2013 in line 370-379, page 16.

In the Table 1 there is irradiance expressed as ave±S.D.; I wonder if given average contains measurements from all stations on the day of sampling? =>We measured irradiance one time per each cruise at every 30 seconds during the incubation hours for primary productivity executed for 4∼5 hours during day time around local noon time. So, ave±S.D. values in Table 1 are averages from every 30 seconds for 4-5 hours a day each season. We described the details in line 95-97, page 5.

Details about particulate organic carbon and nitrogen analysis such as volume of filtered water and station where the sample was taken should be added (only one result per month was presented). This is very important since the origin of POM is not typ-

ical for the estuaries. =>We measured POC, PON, and δ13C of POM collected from surface at the 3 stations at every sampling time. They varied but did not show large differences in POC, PON, and δ13C among the different stations. We described the sampling details in line 114-120, page 6.

It is very interesting that riverine terrestrially derived organic matter is not an important component of the particulate organic matter in the Gwangyang Bay system, which has a large river runoff. One would expect partly organic matter of a terrestrial origin and not such clear phytoplankton fingerprint since the water column is very turbid and euphotic layer very thin (3- 11 m). Also this peculiarity and these results should be discussed and compared with other estuaries like the authors did for biochemical composition. =>We further discussed on the issue in line 300-310, page 13-14.

Nutrient limitation, the use of the ratios (lines 301-305): it is not clear why the authors use for the interpretation of phosphorus and nitrogen limitation only the ratios with dissolved silica (DSi) and not between these two components (N: P). If it was not a random error, the reference should be added for listed criteria. Anyway, in criteria b) for nitrogen limitation instead of DSi:DIP ratio >16 should stand < 16, if it was presumed that DSi and DIN appear in similar concentrations, though not always the case. =>We revised them in line 335-338, page 15 based on Dortch and Whitledge (1992).

References: Listed but not cited in the text: Adolf and Harding, 2006; Choi and Noh, 1998; De Oliveira et al 1999; Julian and David, 1966; Kim et al., 2016 Cited in the text but not listed: Choi et al., 1998; Kim et al., 2014; Kwon et al, 2001; Marsh and Weinstein 1966; Paerl et al., 2006; Yun et al., 2014 Cited or listed with different year of publication: Pirt 1975 (cited in the text), listed in references as Pirt 1976 Some references are written in uppercase. To the references published in the same year a, b should be added =>We revised the references.

In Tables 1 and 4 in April 2012 appears st. 1 which is not marked on the map (Fig.1) =>We revised the map in Fig. 1.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2016-347/bg-2016-347-AC1-supplement.pdf

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