Biogeosciences Discuss., 10, C1264–C1267, 2013
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Biogeosciences Discussions



# *Interactive comment on* "Inter-shelf nutrient transport from the East China Sea as a major nutrient source supporting winter primary production on the northeast South China Sea shelf" *by* A. Han et al.

**Anonymous Referee #2**

Received and published: 26 April 2013

General Comments:

Han et al. studied the nutrient transport between the shelves of the two major marginal seas of mainland China, the East China Sea (ECS) and the northeast South China Sea (SCS). Based on combining results from field data and modeled volume transport rates, they estimated a DIN flux of 1430±260 mol/s from ECS to SCS via Taiwan Strait, about 6 fold higher than that from the Pearl River. They argued that the along-shelf nutrient transport was the major driver for primary production in the northeast South China Sea. These results are interesting to oceanographic community and potentially important



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for local marine environmental protections and managements. However, after carefully reading the manuscript, I found there were several issues in their analyses (please refer to my specific comments for detail). Given the large uncertainty of the flux estimate and the limited area of the CCC in the sampling section, the impact of inter-shelf nutrient flux of CCC on primary production in the northeast SCS may be overestimated. Given these concerns, I have to suggest a major revision of the paper before publication.

Specific Comments:

1. Page 3897, Lines 1-9: It would be interesting to plot the T/S diagram for all the water masses discussed in the area, which would strengthen the argument of water mixing scenario they proposed.

2. Page 3898, Lines 3-6: Low chlorophyll but high nutrients in the surface of ECS than SCS (Fig. 2c-e) during the cruises may not be simply attributed to low temperature. Comparing the mean sea surface temperature (Fig.1a) and sea surface chlorophyll (Fig.7a) from satellite data, one can easily find that high chlorophyll in the ECS with lower temperature compared to northeast SCS.

3. Page 3899, Lines 1-14: The comparison of the transect data of ECS (Fig.3) and SCS (Fig.4) is interesting and may need further discussion. Could the difference of chlorophyll in the outer shelves of Transect PN and Transect 2 come from other factors influencing the growth efficiency of phytoplankton such as iron limitation or light-limitation?

4. Page 3901, Line 14: biological consumption will lead to increase but not decrease of the N:P ratio in the water columns in this particular case. For example, N:P ratio of 50:1 in source water (100 uM of DIN, 2 uM of DIP) will be changed to a ratio of 84:1 (84 uM of DIN, 1 uM of DIP) after Redfield biological removal of 16 uM of DIN and 1 uM of DIP.

5. Page 3901, Lines 16-28: Discussion of the varying DIN:SRP ratios for different

waters (Fig.6) should be compared to water-mass analyses (T/S diagram).

6. Page 3902, Lines 21-24: “almost comparable to” should be changed to “higher than”

7. Page 3902, Lines 25-28: According to Fig.1a-b, the influence of CCC is only limited to the inner shelf of the northeast SCS (<50m isobaths). However, the field data of Transect 2 (Fig.4f) suggested that chlorophyll is also high in the upper layers of the northeast SCS (~145 km away from the coast). Apparently, the high biomass there was not due to the mixing of CCC and SCS as the chlorophyll is much lower in the outer shelf (~105 km from the coast). I expect the diapycnal diffusion fluxes of nutrients would be important there, which however are not quantified in the paper. The authors should address this issue in the revised manuscript.

8. Page 3903, Line 20-25: The calculation of net southward total-volume-transport rates (-0.23 Sv and -0.07 Sv) may not be appropriate given the large error-bars (±0.37Sv and ±0.24 Sv). The author should conduct significant test for the associated data.

9. Page 3903-3905, Section 4.1.1 and 4.1.2: It is quite confusing here since the authors compare net TT and southward TT at the same time. The direction of current velocity in the two stations varied frequently through the time-series (Fig.8) suggesting a periodic injection of the high-nutrient CCC water to the west TWS. Therefore, I think it may better to estimate TT by using just the southward value but not the net value (southward minus northward) since only southward transport will bring nutrients to the SCS.

10. Page 3905, Line 16: The author should provide the error-bar for the estimated TT of 0.13 Sv for CCC in TWS.

11. Page 3905, Line 20: The flux estimate of ~1430±260 mol/s is incorrect. The author should also include the error bar of TT in the calculations. Fore example, if we use the observed southward TT of 0.14±0.09 Sv (Page 3904, Line 11), then the flux is (11.0±2.0 uM) ïĆť (0.14±0.09 Sv) = 1540±1450 mol/s.



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12. Page 3905, Lines 26: The authors should discuss the source of uncertainty to their flux estimate of CCC before comparing it to nutrient fluxes from Changjiang and Pearl River.

13. Page 3906, Section 4.3: The estimates of CCC-supported new and primary productions are misleading here. Statistical analyses are needed here in order to provide an unbiased estimate. The authors also need to consider error propagation in their calculations.

14. Page 3906, Line 23: It is inappropriate to express approximate estimates as "$\sim$>58$\pm$10%" and "$\sim$38$\pm$7–24$\pm$4%". I would suggest change them to "$\sim$58%" and "38-24%". On the other hand, the values of 58$\pm$10% , 38$\pm$7%, and 24$\pm$4% are pretty conservative numbers, while the total area of the NESCS shelf is only a rough calculated number. How much can we trust these estimates?

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Interactive comment on Biogeosciences Discuss., 10, 3891, 2013.