

I thank the authors the revision and the manuscript has been improved, however, the more revisions will be needed to accept this manuscript.

1. I cannot agree the conclusions that upward flux of nitrate controls phytoplankton production and patchy distributions. At least, the authors should discuss why Chl *a* amount remained low while the nutrients were supplied richly at station C6 if the data removed. Additionally, the Chl *a* amount at C11 was outlier in the relationship. The relationship between Chl *a* amount and the nitrate upward flux except C6, C9 and C11 showed the significant positive relationship. This relationship indicate that nutrient upward flux basically controls the phytoplankton abundance, however, the patchy Chl *a* variations (for example at C11 and C6) are not controlled by the upward flux. That is, I consider that the patchiness of Chl *a* concentration is not largely controlled by the upward flux.
2. The connectivity of the between the line observations (C1-C12) and the stations A, B was unclear. In particular, the area near “station B was well documented for its high turbulent mixing” (L358-360), and so the station B is not the representative station of nSCS. I consider that the observation values such as the phytoplankton growth and the grazing rates in the nSCS cannot be discussed based on the station B data as I pointed out in previous reviews.
3. The abstract is the only description of the results and is not organized. I think what new or innovative is necessary to publish in Biogeosciences.

Specific Comments

L126: Not only the company, but also the product name is necessary.

L136: Bran+Luebbe

L192: I understood the calculation. However, the calculated nutrient gradient is not at the depth of Z_i . It was at the depth of $(Z_i + Z_{i+1})/2$.

L471: “ $n =$ ”