Biogeosciences Discuss., 10, C2724–C2725, 2013
www.biogeosciences-discuss.net/10/C2724/2013/
© Author(s) 2013. This work is distributed under
the Creative Commons Attribute 3.0 License.

Biogeosciences Discussions
Open Access

Interactive comment on "Impact of the Kuroshio intrusion on the nutrient inventory in the upper northern South China Sea: insights from an isopycnal mixing model" *by* C. Du et al.

Anonymous Referee #2

Received and published: 13 June 2013

In this discussion paper the authors examined the impact of the Kuroshio intrusion on the nutrient inventory in the central northern South China Sea (NSCS). To quantify the extent of the Kuroshio intrusion, an isopycnal mixing model was adopted to derive the proportional contribution of water masses from the SCS proper and the Kuroshio. This manuscript provides a preliminary analysis for understanding the spatiotemporal variations of nutrient in the upper layer (upper 100m) of the NSCS. The result is interesting and the analysis is scientifically valuable. However, I think this manuscript will be more convincing and can be published in BG if the authors can properly clarify/address the following questions.

1. In section 4.1, the authors made a comparison between the diffusive flux along and

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

across isopycnal surfaces to support their statement of "isopycnal mixing was indeed prevailing over diapycnal diffusion in controlling the physical transport of nutrients in the upper central NSCS". However, it is the total amount instead of flux that determines which one (isopycnal or diapycnal process) is dominated, that is, you should take their respective area (surface area for diapycnal mixing and cross sectional area for isopycnal mixing) into account. Moreover, vertical and horizontal advection has not been discussed, which could be far greater than isopycnal/diapycnal processes.

2. In section 4.4, p6955, lines 19-28, the authors calculated the new production for different seasons in the study area and compared it with previous studies. I noticed that the new production in winter is 7.4 ± 2.7 mmolCm-2 d-1 in this study, but still substantially lower than that reported by Chen (2005) ($\sim$21.7 mmolCm-2 d-1), this is considerable relative to the nutrient inventory (200-290 mmol m-2 for N+N) and cannot be neglected. I suggest the authors to do a quantitative estimation on the nutrient budget to find out which one (vertical/horizontal mixing, advection, or biological production, etc.) is the major control factor and to evaluate their relative contributions.

Minor comments:

1. In section 3.3 or Fig. 4., I suggest use N+N to replace the SRP to keep consistency, since the authors stated that "we used N+N as an example throughout this paper unless otherwise indicated" (p6944, lines 22-23). Otherwise, justification for use SRP in this figure should be provided.

2. P6953, line 28, "N2-fixation is a net sink for the N+N inventory", should be "a net source".

3. Fig. 3., the legends, labels and titles are too small.

Interactive comment on Biogeosciences Discuss., 10, 6939, 2013.