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Interactive comment on “Influence of cross-shelf water transport on nutrients and phytoplankton in the East China Sea: a model study” by L. Zhao and X. Guo

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This is an important paper evaluating the source of nutrients and associated productivity in the East China Sea and should generate wide interest. There is, however, a major flaw that needs to be fixed:

The problem is with the initial nutrient concentration in the Yellow Sea. This is obviously critical as without a reliable starting point the model would go nowhere. The authors used a very high NO_3 concentration of about $5\mu\text{M/L}$ citing a Chinese atlas and the World Ocean Atlas 2005. The latter gave a value lower than $4\mu\text{M/L}$ while the former gave a value below $2\mu\text{M/L}$. It is therefore not clear why the authors chose an initial value

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higher than both. Further, the World Ocean Atlas did not have any station in the central Yellow Sea so the values were extrapolated. The conclusion is that the Chinese atlas is probably more reliable. As such, the model got off with a poor starting point so the authors need to rerun the model with more realistic NO₃ concentration to start with. Otherwise the authors need to explain where the high NO₃ originated.

My other comments are minor: 1. The authors mentioned in the Introduction that the East China Sea receives nutrients from the South China Sea through the Taiwan Strait. It is actually the upwelling of the South China Sea water on the western part of the Kuroshio that supports the major portion of nutrients to the ECS(explained in my 2008 JO paper:Distribution of nutrients in the East China Sea and the South China Sea connection). 2. Under 3.1 the authors stated "For a direct comparison with the observations..." yet there is no comparison given. 3. Why nutrient fluxes reach a minimum in March , not in June when minimum volume flux occurs need to be explained.

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