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Interactive comment on "Spatial variations in the Kuroshio nutrient transport from the East China Sea to south of Japan" *by* X. Y. Guo et al.

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This manuscript uses a comprehensive data set to assess the spatial and temporal variability of water and nutrient transports by the Kuroshio Current, from the East China Sea to south of Japan. The authors have done a commendable effort to put together and systematically analyze a very extensive data set. It consists of five sections with nutrient and hydrographic measurements taken during at least the last decade. The manuscript is a natural extension of Guo et al. (2012) where only the East China Sea sections (PN and TK) were analyzed; from the authors' references I see this is the first time this extensive data set has been examined not only for nutrient but also for water mass transports. This reason alone would deserve, in my opinion, publication of the manuscript. The authors show the Kuroshio Current transports O(1000 kmol $s^{-1)}$)

of nitrate, which is comparable to the Gulf Stream transport (Pelegrí and Csanady, 1991; Pelegrí et al., 1996, 2006; Williams et al., 2006, 2011), although about two-thirds are associated to relatively narrow recirculations. The manuscript also analyzes the nutrient-transport errors caused by gaps in nutrient data and discusses the contributions of the Kuroshio Current through the East China Sea, the Ryukyu Current and the recirculation of a substantial part of the Kuroshio Current south of Japan.

I would like to congratulate the authors for their interesting study. Overall, I find the manuscript is close to meeting the high standards for publication in Biogeosciences. However, I believe it would greatly benefit from a careful revision including (1) the temporal variability of the water and nutrient transports and (2) a thorough discussion of the water and nutrient transports and balances. Additionally, there are several issues that need to be addressed and clarified. My concerns and suggestions are described next, categorized as major issues, additional considerations and minor points. I encourage the authors to take these comments and suggestions into account for their revised manuscript.

Major issues

1) Data set and time series

Figure 1 shows the data distribution, separated as Conductivity-Temperature-Depth (CTD) and nitrate data. The nitrate data goes further back in time than the CTD data, to the late 1960's in section 137E. The authors do not explain why they restrain the CTD data only to the first decade of the 21st century. Is it because there are no earlier data? However, there are CTD data for sections PN and TK from the late 1980's (Guo et al., 2012). Or do the authors avoid earlier times when a large meander in the Kuroshio Current south of Japan was observed (Kawabe, 1995)? Or is it simply because they prefer to have one single decade with data available from all sections? There is no problem to restrict the analysis to this last decade but the authors should

explain why they do so and clarify what is the real extension of the available data. Further, the authors should be careful when referring to hydrographic data: Bottle data is indeed hydrographic data, with temperature and salinity values, although with much reduced vertical resolution than CTD data (perhaps this is what the authors meant to say but I'm surprised there are many instances of sections with nitrate data but without hydrographic data).

The data set is so unique that I regret not seeing a time series of transports through each section. In my opinion, this would be a significant additional contribution in their future Biogeosciences paper. I would encourage the authors to include one figure where they show the time series of water and nitrate transports for the last decade through all five sections, it should be sufficient if they show the positive and net volume/nitrate transports. Further, I would incite the authors to put together these 10 years of data to calculate monthly-mean values for these four variables, such a plot would be very nice to see; if there are no enough data to have monthly values then I would recommend them to use a two or three months averaging box.

2) In Section 4.2 the authors discuss the water and nitrate contributions from the Kuroshio and Ryukyu Currents and the open-ocean recirculations. They emphasize the contributions from the different branches at different density layers as well as the importance of the recirculation in the open ocean south of Japan (OK, ASUKA and 137E). This is fine but, in my opinion, their discussion falls somewhat short. In Table 1 the authors provide information which is never discussed. I don't mean to go into the details but they should make an effort to extract general behaviors. For example, why showing the (layer and total) area-averaged and transport-averaged nitrate concentrations if you don't analyze them? What do we learn from these numbers, from their along-stream variation, from the differences between the positive and negative values?

The points addressed in Section 4.2 are probably the most important ones in the manuscript and the authors should make an exhaustive and convincing discussion,

including plots and additional figures if necessary. For example, it would be very helpful to show some schematic diagrams, based on their results, illustrating the main streamlines (with numbers for water and nitrate transports across the sections) in layers 1, 3 and 5. This should help identify the contribution of each branch to the net downstream flux and the contributions of the open-ocean recirculations to the positive transports. The authors should also try to extract some relevant conclusions from the nitrate transport unbalances and from the differences between the area-averaged and transport-averaged nitrate concentrations, for example regarding nitrate remineralization during the offshore recirculation south of Japan.

Additional considerations

3) The authors choose to place the reference level for their geostrophic calculations at 2000 m (or the sea floor if shallower) and use an inverse technique to calculate the velocities at this reference level. This does not mean they have obtained a unique true geostrophic solution, as there will be a different solution for each choice of reference level. For example, if the authors had chosen the reference level as 1000 m (or the sea floor when shallower) the solution would have probably been substantially different. I suspect this may be a reason for the relatively large calculated transports; the results of Ichikawa et al. (2004) and Howe et al. (2009) suggest that a shallower reference level, about 1000 m, could perhaps be more representative for the Kuroshio Current.

There are methodologies to select the reference level. Machín and Pelegrí (2006) applied the inverse method with a varying reference level and selected the one that minimized the mass transport unbalances. This approach provides a robust justification for choosing the reference level, but certainly implies substantial additional work. Maybe the authors would like to try this or perhaps leave it for future works; if the authors do not attempt this then they would need to provide an explanation, perhaps a relevant reference, on why the 2000 m level is a sensible selection. One possibility may be Jayne et al. (2009). Are there other references that would sustain this selection?

Finally, it would be nice if the authors show the velocities at the reference level. I would expect these to be very small if the reference level has been properly chosen.

4) In pages 6741 and 6742 the authors mention that there are several instances where there are no nitrate data simultaneous with CTD data. They explain that, in such cases, they replace the simultaneous nitrate data by the corresponding temporally-averaged nitrate values (a function of spatial position). The authors explain what is the transport term neglected in this approximation (page 6742) and estimate the size of this error (page 6749). It turns out that the error is one order of magnitude smaller than the actual value. However, there is still another situation where there is no nutrient and velocity data available for the same time period (page 6749). The authors follow the same procedure but now computing the average nitrate concentrations from a totally different time period. In this case the error is substantially larger, possibly a factor 1/3 the actual value.

I would like to suggest the authors to examine a relatively simple alternative, as described in Pelegrí et al. (2006). Use all available data in order to obtain a temperature-nitrate relationship for each section. This relationship will probably be quite tight (low dispersion around a single curve) and, for those cases when there is only hydrographic data available, it will allow you to infer nitrate concentrations from temperature data. In this way for each CTD cast you will have an empirical simultaneous nitrate cast, and you may then use this cast to calculate the nitrate transports. You may check on the validity of this approximation in a way similar as you have done in the manuscript; I trust that it will lead to a reduction of the nitrate transport errors, as compared with your procedure, for those cases when no simultaneous nutrient data is available.

Minor points

5) The authors need to have their manuscript carefully revised by a native English speaker. There are numerous orthographic errors that need to be corrected and many

sentences that could be simplified in order to facilitate reading. Further, in many places there are words that are not properly chosen and may lead to misunderstandings. For example, in the Abstract (page 6738, line 6), the authors say “4 sections along the Kuroshio path” when they probably mean “four sections across the Kuroshio path.”

6) The authors state that mass conservation is assumed within each of eight isopycnal layers (Section 2, page 6740) but this is not really true. The inverse model cannot satisfy mass conservation for each layer, it simply looks for the best possible solution that approximately meets this requirement, as becomes clear from the numbers in Table 1.

7) Along the text there are references to geographic locations that are not identified in Figure 1. The authors should identify Ryukyu Current, Tokara Strait, Okinawa Island and any other geographic feature mentioned in the text.

8) Page 6747, line 21: I trust that “unit width” again refers to 25 km, please clarify.

9) Page 6749 (lines 18-22) and Table 1: The authors need to define the positive and negative directions.

10) Page 6752: it is true that Williams et al. (2011) reported a very significant increase in the Gulf Stream nutrient transport within a relatively short distance (35.5° to 36.5°N), therefore attributable to enhanced local recirculation, but many other characteristics (such as the along-stream changes in nutrient concentration and nutrient transport within different isopycnal layers) were earlier discussed by Pelegrí and Csanady (1991), Pelegrí et al. (1996, 2006) and Williams et al. (2006).

11) Figures 1 and 5: please clarify where the origin for the water and nitrate transports is located. I understand it is between each pair of stations but it needs to be specified. I assume the scale for water and nitrate transport represents the distance between the stations (dots) and the water and nitrate transport lines (red and black lines); if so, it needs to be stated. Further, the line drawn for nitrate concentration (blue line) is

confusing, I would recommend removing it.

12) Figures 3 and 4: Sections OK, ASUKA and 137E are incomplete, the offshore end has been removed. This is fine but you need to say it. Similarly, explain section 137E is incomplete in Figure 6.

13) Figures 3 and 4: panels labeled (a) through (e) should be properly identified, either in each panel or in the figure's caption.

14) Caption for Figure 6: It says "Fig. 3e" twice but in both instances I believe it should say "Fig. 4e".

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