

***Interactive comment on “Biogeochemical contrast  
between different latitudes and the effect of  
human activity on spatio-temporal carbon cycle  
change in Asian river systems” by  
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The author thanks to the referee's valuable comments and revised the manuscript according to them in the following. These revisions also help to clarify its effect of human activity on spatio-temporal carbon cycle change in Asian river systems, which would be spot on the topic of the Special Issue and would be a useful and interesting application of the NICE-BGC model. Detailed response is described in the following; \_\_\_\_\_

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- The paper compares the fluxes of carbon (C) in the Ob, Yangtze, and Mekong Rivers, that were found to be very different due to differences in climate (latitude). This conclusion makes sense but was highly predictable since the fluxes of C from major World rivers has been known with certainty for several decades, going back to the papers of Meybeck in the 1980's, that were followed by an abundant literature. Consequently, the paper is of marginal interest and does not add anything significantly new to the recent papers from the author (who is prolific), such as his two papers published in 2017 (JGR & Ecol. Model.) and 2016 (Ecohydrology and Hydrobiology) that dealt with C fluxes from World Rivers. For instance, Figure A2 of the present ms (BGD) is exactly identical to Figure 3 of the JGR 2017 paper, and nearly identical (except one panel) to Figure 6 of the Ecohydrology and Hydrobiology 2016 paper. Figure 4 of the 2017 JGR paper is nearly identical to Figure 6 of the 2017 Ecol. Model. paper, that have been re-arranged slightly to produce Figure 6 of the present BGD ms (only a minor cosmetic re-arrangement of the figure, the actual content of the plots is identical).

Ans. The author generally agrees to the referee's comments. Yes, he used Figure A1 and A2 in Appendix in order to show more clearly the general structure of the model and the model validation in global rivers in the previous papers. So, he rewrote as the reprint from the previous papers in the figure captions after copyright permission.

- More importantly, the paper as it stands does not actually address the topic of the special issue that deals with "Human impacts on carbon fluxes in Asian river systems". However, the author has a modelling tool (NICE-BGC) that could be very useful to address this topic. First, the author could compare a reference simulation (without dams) for all of the major Asian Rivers with a simulation including water diversion by dams. This would allow to estimate how dams have already affected the C fluxes in Asian Rivers (some of which are the largest in the World, such as the Three Gorges). Second, in a similar fashion, the authors could estimate the future impact on C fluxes in Asian Rivers from the building of planned infrastructures. For instance, enormous

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dams are planned for construction in the Mekong River network that are expected to severely affect suspended matter load with potential problems in the Mekong delta (erosion). Construction of dams will not only affect TSM and C fluxes but also the delivery of nutrients to the East China Sea, with potential impacts on its productivity. Third, the author could use climate predictions for the next 100 yrs in the region and estimate how predicted changes in precipitation and temperature could affect the C and nutrient fluxes from Asian Rivers. This would allow to address the question whether building of dams or climate change would most affect the C fluxes in Asian Rivers. Addressing the above three points would be spot on the topic of the Special Issue, and would be a useful and interesting application of the NICE-BGC model.

If the author decides to modify the paper in this direction, then all of the parts of the paper regarding validation at global scale (comparison with Amazon, Yukon, etc : : :) should be removed. On the other hand, the author should focus on a careful validation of the Asian rivers, for which a large amount of data is available from literature. For instance, I would like to see how the NICE-BGC model can simulate seasonal variations of concentrations of DOC, POC, TSM, HCO3-, CO2, etc compared to seasonal changes of these quantities from observations in the different Asian Rivers (available from literature).

Ans. The author thanks very much to the referee's valuable comments. He generally agrees three points suggested by the referee are important for the topic of the Special Issue. However, the time is very limited for the revision of the paper (about one month) according to these three points. The author has already submitted another paper and now under review about the first point in global scale. The second point is also beneficial, and the author is now preparing to do this in the future work. Third point is very important, but actually it takes more time to do a climate prediction for the next 100 years. Of course, he agrees well with the importance of addressing the question whether building of dams or climate change would most affect the carbon fluxes in Asian Rivers. But it is rather a future work to carry out a comprehensive anal-

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ysis based on these three points including the effect of land use change. Instead, it is necessary to put together the previous literature as much as possible, and clarify the actual state of carbon cycle in Asian Rivers and this effect on global carbon cycle. So, he did not delete Figure 6 because he thinks this figure is important to estimate them. Otherwise, he worries that the paper would become an incoherent structure including all three points before the clarification of the actual state of carbon cycle in Asian Rivers. For the above reasons, the author removed the validation at global scale in Figure 5 according to the referee's comment. Then, he extended the simulation period for 18 years in order to validate the simulated result with the limited previous literature as much as possible. He gathered and added more literature about time-series and seasonal variations of DOC fluxes as much as possible to compare seasonal changes of these quantities in different Asian Rivers in the revised Figure 4. In particular, he compared the available literature with the simulated result in Ob River (Kohler et al., 2003; Gebhadrt et al., 2004; McClelland et al., 2016), Yangtze River (Wang et al., 2012; Shi et al., 2016; Ran et al., 2017), and Mekong River (Li and Bush, 2015; Borges et al., 2017) as much as possible though there were not so many available data except DOC yet. This validation also improved the accuracy of carbon flux in the revised Figure 5, as described in the following comment. The author also revised the manuscript to explain the above in details.

As it stands the validation of the model given in Fig. 5 is somewhat worrying. The modelled DOC flux from the Mekong is under-estimated by a factor of 10 compared to field data, while the modelled DIC flux is over-estimated by nearly a factor of 4. For Yangtze, similar large discrepancies are also observed, for instance the modelled POC flux is over-estimated by a factor of 3.

Ans. The author added newly Table 4 about some literature further to constrain carbon budget in Ob River (Gordeev and Kravchishina, 2009; Mann et al., 2016; McClelland et al., 2016; Kaiser et al., 2017), Yangtze River (Li et al., 2012; Shi et al., 2016; Guo et al., 2015; Gao et al., 2017; Ran et al., 2017), and Mekong River (Li and Bush, 2015;

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Borges et al., 2017) in the revised Figure 5, respectively. Because the author added to validate the time-series and seasonal variations as described in Figure 4, the model result improved the accuracy in Figure 5. \_\_\_\_\_

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Please also note the supplement to this comment:

<https://www.biogeosciences-discuss.net/bg-2017-447/bg-2017-447-AC1-supplement.zip>

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