

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 Tadanobu Nakayama

Anonymous Referee #2

Received and published: 5 January 2018

1.. The objectives of this manuscript are to evaluate 2 hypotheses for the Ob, Yangtze, and Mekong Rivers: (i) Latitudinal difference is an important factor to constrain a variability of horizontal and vertical carbon fluxes, and (ii) Seasonal and diurnal variations are important components to evaluate the impact of regional hot spots on global carbon cycle change. The toolset is to apply the NICE-BGC model, developed by the author.

2. I have two overall levels of response/review. Level #1. As a pure modeling exercise. Level #2. As contributing knowledge to the biogeochemistry of river carbon cycles in these 3 rivers, and how this knowledge might be extrapolated; i.e, addressing the

C1

hypotheses.

3. Level #1. As a modeling exercise. The NICE-BGC model is very complex, addressing pretty much the entire land-surface dynamics of ecosystem function, and water and carbon dynamics. This brings up multiple complexities in data acquisition and processing (that I am painfully aware of), accurately expressing the processes being addressed, and then the sheer computational logistics. Very challenging – I applaud the author for even taking this on.

Several points: (1) The devil is in the details, especially with regard to how specific processes are represented in the model. Simply that there is an algorithm with the name eg “CO₂ evasion” doesn’t tell us anything – what is it? This becomes important at multiple levels – eg how is sediment storage or outgassing represented with enough resolution to distinguish between day and night fluxes?

(2) An important issue is to make sure all of the scales and time steps line up for the processes represented. Yes, he did dial through the several different resolutions for Hydro, which was interesting. But more broadly there is often disconnect between scale of forcings, landscape, and time step of process. A 10 x 10 scale is ~100km a side, which implies a travel time for water quite in excess of diurnal processes.

(3) Calibration/validation is, indeed, a significant challenge. A thoughtful evaluation of the problem would be useful. I certainly appreciate that data can be sparse, especially to represent all of the processes included in the model. But there is much more available than is used here, which can lead to erroneous conclusions (some, below)

4. Level #2. Biogeochemistry

I find the conclusions on the biogeochemistry to be either not at all surprising, and often disagree.

(1) The first step is to units straight. Fluxes are often referred to gC m⁻² yr⁻¹. Per m⁻² of what? The total area of the basin (eg Stung Treng, for Mekong) or per local tributary,

C2

or per ??? Without knowing this is impossible to evaluate. (2) The next step is for the reader to have some confidence in computed results. A thorough cal/val process would be useful here, but the manuscript doesn't do that. So do presented results make at least intuitive sense? Table 3 says that GW is ~89% of total flow of the Mekong. I would be shocked if that is true (I've co-authored papers on Mekong hydrology) - it has to be wrong. Results for the Amazon had discharge had perhaps a low-water average, not annual, while TOC fluxes were far too low. Mekong water temperatures are far too cold (why do T)? Etc etc. (3) I don't understand why trying to do diurnal fluxes, Yes, there can be differences in pCO₂ concentrations in still waters, much less so in rivers, usually mitigated by lower wind speeds, but this isn't relevant at more regional scales, and don't understand how the model can represent it. I have no idea how to get/explain diurnal variation in sediments (outside of tides, but that is a different issue). If real, I would be most interested in seeing that documented. (4) Why do the 6 other rivers? Not part of the original argument. (5) Etc

5. Evaluation: This manuscript, while potentially very interesting, is another example of the difficulties in taking complex more global models, and using them to interpret finer scale dynamics. Such models certainly have their place, but caution must be taken in their application.

(1) The manuscript doesn't address at all the stated objective of human activity (2) Yes, there are differences between the 3 main rivers over time and space, but the manuscript doesn't attribute cause/effect with any confidence. (3) The toolset used, NICE-BGC, wasn't applied with enough specificity and confidence, relative to the questions asked. Topics were all over the place, without enough detail to be convincing. It is not clear that the author really understands the details of the biogeochemistry being addressed. To be fair, a very wide range of issues were included, but a better strategy might be to parse those out into several more focused manuscripts.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-447>, 2017.