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Biogeosciences Discussions

Interactive comment on "Transformation of dissolved inorganic carbon (DIC) into particulate organic carbon (POC) in the lower Xijiang River, SE China: an isotopic approach" *by* H. G. Sun et al.

Anonymous Referee #2

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Comments on "Transformation of dissolved inorganic carbon (DIC) into particulate organic carbon (POC) in the lower Xijiang River, SE China: an isotopic approach" by H. G. Sun et al.

General comments:

In short, this is an original and informative manuscript and can be considered to be published in the journal of Biogeosciences after a major revision, because there are still some problematic description and reasoning statements in its current version.

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They include:

First, the authors have ignored metabolism of organic matter by decomposers such as bacteria and fungi in the river water, sometimes confused with the decomposers to producers.

Second, the authors have ignored some details of isotopic fractionation from CO2 to HCO3-.

Specific comments:

Lines 3 and 4 of the page 9473, the author indicated that there is oxidation of organic matter in rivers, and in the line 7 of the same page the author also indicated “through interim processes such as microbial utilization”. Indeed, they are all very important biogeochemical processes in the river water, and mainly bacteria and fungi involved in the decomposition of aquatic organic matter. However, in the later analysis (such as in the line14 of page 9748, and the part of 4.1.2 of the manuscript), the author does not seem to consider the decomposition of bacteria and fungi in the water.

In the lines from 12 to 14 of page 4973, narrative content and referred literature is not matched. Those references do not focus on the Xijiang River.

In the line 16 of page 4975, in the presence of pollution, the particles in the clay will absorb the NH4+ ion, if you do not eliminate out this part of inorganic N, which will result in the POM C/N ratio decreased.

In the lines from 7 to 9 of page 9744: the author indicated that “DIC shows a decline trend from upstream to downstream, consistent with the fact that carbonate is mainly exposed in the upper reaches, indicating the dominate impact of carbonate weathering on the riverine DIC concentration of the region” which statements are conflicted with the increasing biomass of algae downstream.

In lines 1 to 3 of page 9480, the author indicated that “producing equal contributions of both carbonate and soil CO2 to the DIC, resulting in a mid δ13C value of the DIC

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between the two sources", and in the lines 6 and 7 of the same page, "DIC from silicate rock weathering will have the same δ13C value as soil CO2". The reviewer suggested that there is an isotopic fractionation from soil CO2 to aquatic HCO3-, which is about $\sim$9‰ (Clark and Fritz, 1997). The author should have not ignored of this. The lines from 14 to 23 of the same page, isotopic fractionation from soil CO2 to aquatic HCO3- was not considered completely, i.e., the ionization process from aquatic CO2 into HCO3- (the fractionation factor is $\sim$9‰

In the lines 13 and 14 of page 9483, the author indicated that "This implies an all-year excess of production over respiration in the Xijiang river system, but close to zero during the flood even", which is conflicted with the large pCO2 in the river.

In the lines 21 and 22 of page 9483, the author indicated that "bacteria can grow by directly using bicarbonate". The bacteria are the decomposers, and can not directly use HCO3- to produce organic matter.

Reference

Clark I D, Fritz P. Environmental Isotopes in Hydrogeology. New York: Lewis Publishers, 1997

Interactive comment on Biogeosciences Discuss., 8, 9471, 2011.