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Biogeosciences Discussions

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Interactive Comment

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 #1

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The paper by Sun and coauthors present isotopic and chemical data obtained on POC and DIC from the Xijiang river during a one year sampling. The dataset is relevant and analytical methods are correct. The main objective of the paper is the determination of the origin of carbon from DIC and POC using a mass balance approach on carbon isotopes. Calculations are well explained and results are very interesting. They demonstrate for example that an important contribution of POC is issued from primary production, probably up to 50%. Then, a first order estimation indicates that transformation of carbon issued from carbonate weathering (DIC) into POC is also important

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Interactive Discussion

Discussion Paper

and could constitute up to 20% of POC in dry season. More particularly, I found that their hypothesis concerning Âń old carbon reservoir effectÂż on riverine POC is cleverly presented . According the authors, it could be largely due to the incorporation of old carbon from DIC into POC. Such hypothesis deserves to be presented in the abstract. In contrast, I do not fully agree with their argumentation on the fact that such process could constitute an important sink of CO2 in river systems. This requires better explanation or calculation (see last comments below). I found this paper very interesting and in perfect agreement with the editorial line of Biogeochemistry. I recommend its publication after minor revision (see below).

P4 line 11 : Mook & Tan 1991 is not in the reference list.

P8 line 11 : the mean DIC cited in the text is not reported in the table or in the text. However, a precise mean is difficult to define from such data (including various stations and various events) and the comparison of min and max range will be more adapted.

P9line 14 : the authors cited a paper by Sun 2007 arguing that POC is positively correlated with TSS. This relation (that I cannot look for because I can't access to paper published by Chinese Science Bulletin) must be precised here. Indeed, if POC vs TSS are both expressed as g/l, it is quite obvious. If it corresponds to POC (%) vs TSS (g/l), then its rather unusual (generally, increase of TSS are due to resuspension or soil leaching and contains lower organic matter). In this last case, such relation must be discussed and demonstrated.

Figure 1 : bottom map is not easy to read, some station names must be shifted.

Figure 4 : use open and solid symbols instead of color

Table 1 : Not easy to read. I suggest to define the limit from one river to another by a solid or dotted line.

P10 line 6 : what do you mean by Âń shoal Âż, and I do you know that phytoplankton is abundant on this place?

P11 line 2 : you cannot expect seasonal variation of POC on the soils at these sampling depths. Indeed, the samples are to deep to be affected by seasonal processes, unless the soils are ploughed. Furthermore, the slight variation described here can also be due to the sampling scheme. The best sampling strategy to compare soils is to collect the same horizons and not the similar depths. The change from one horizon to another between two soils (at similar depth) can largely explain the variations observed here. These data can be treated as a whole but cannot be directly compared. This comment also concerns line 6 to 11, where the interpretation of the results is probably wrong. I suggest to delete them.

P12 line 9-11. The hypothesis that the mid 13C value of DIC results from equal proportion of carbonate and soil CO2 is very very important for most of the calculations made on this paper. Regarding this, it is not sufficiently argued and references must be absolutely added.

P14 line 17-19 : I do not understand the sentence. On p12 authors said that soil CO2 contribute equally to carbonate weathering to the DIC signature, whether they argue here that DIC coming from soil is minor ? This needs explanation.

P15 lines 9-14 : I do not understand the entire paragraph.

P15 line 21 : the significant contribution of organic carbon from river plant to POC is not really discussed before this line. It is presented in part 3.2 results and on the basis of C/N ratio, not isotopic data.

P 21 line 19 : I am not sure to understand why authors compare the POC flux calculated from DIC to the CO2 consumed by silicate weathering ? Because both are CO2 sinks ? If yes, 3% is not a high proportion and I do not agree with the last sentence of the chapter (p22 line 5): this POC sink does not appear to be such important. Furthermore, the organic matter degradation must be taken into account and part of this POC will be degraded (around 50% at least) once deposited before being buried. Thus, the final proportion of this sink is rather insignificant (I suggest also to delete this hypothesis from the abstract).

The English needs revision.

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