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***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.**

**H. G. Sun et al.**

jhan@mail.iggcas.ac.cn

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We thank you very much for detailed and constructive comments. Following the suggestions of the first paragraph of the comments, we revised the abstract and added the hypothesis concerning the old carbon effect on riverine POC, and deleted the statement about the DIC-POC transformation in river to be an important CO<sub>2</sub> sink. In below, we response your specific comments in a one-to-one style.

(1) Mook & Tan 1991 is not in the reference list. Reply: Sorry for carelessly omitting the

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reference. It has been added in the revised version. We have also made a thorough check for other references in the manuscript.

(2) 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 and the comparison of min and max range will be more adapted. Reply: Following your suggestion, we revised this section as below: Our measurements show that the DIC concentration of the Xijiang at the sampling site XJ-1, close to the Gaoyao Hydrometric Station, ranges from 1.35 to 2.07mmol/L with a mean value of 1.72mmol/L, consistent with the historical values, and it is higher than in most large rivers of the world (Degens et al., 1991), characteristic of the drainages developed on typical carbonate regions.

(3) The authors cited a paper by Sun 2007 arguing that POC is positively correlated with TSS. This relation must be précised 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. In this last case, such relation must be discussed and demonstrated. Reply: TSS and POC cited here are both expressed as g/l. In the revised manuscript we have clarified this confusion in the text and in Table 2.

(4)Figure 1: bottom map is not easy to read, some station names must be shifted. Reply: We have modified the map for easy reading.

(5) Figure 4: use open and solid symbols instead of color. Reply: We have made this modification as you suggested.

(6) Not easy to read. I suggest to define the limit from one river to another by a solid or dotted line. Reply: Following your comments, we redesigned this table. Data for different rivers are separated by a dotted line. Data for different seasons are separated by a space.

(7) What do you mean by 'shoal', and how do you know that phytoplankton is abundant on this place? Reply: On the upstream to this sampling site, there is a remarkable wide

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segment of the riverbed, thus the riverine water flowing there consequently becomes slow and shallow. Indeed, we have seen abundant submerged aquatic plant and green algae growing there. We have rewritten the relevant sentences in the revised version.

(8) You cannot expect seasonal variation of POC on the soils at these sampling depths. Indeed, the samples are too 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. Reply: Following your suggestions, we have revised this section for more conciseness and delete some interpretations. We agree with this comment that these data of soil samples can be treated as a whole but cannot be directly compared. However, the data we presented here is used mainly for the assignment of the soil POC  $\delta^{13}\text{C}$  value that should be more related to soil erosion process, because the  $\delta^{13}\text{C}$  value of soil POC component in the riverine POC will be used for subsequent calculations. For this purpose, a simple depth-integration of the soil POC could be suitable. In addition, here we have no special interest in seasonal variations of soil POC content and its isotopic signature, but just summarize the data obtained.

(9) The hypothesis that the mid  $^{13}\text{C}$  value of DIC results from equal proportion of carbonate and soil  $\text{CO}_2$  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. Reply: This hypothesis has been widely used in previously studies (Mook and Tan, 1991; Das et al., 2005; Li et al., 2010). In addition, if a region is of high partial pressure of soil  $\text{CO}_2$ , this hypothesis can also be mathematically deduced based on the definition of  $\delta^{13}\text{C}$  value and the process of carbonate weathering. It should be noted that in the absence of vegetation, atmospheric  $\text{CO}_2$  may contribute to DIC by

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rock weathering, but this is not the case for the Xijiang basin. To address this concern sufficiently, some statements and references have been added in the revised version.

(10) I do not understand the sentence. On p12 authors said that soil CO<sub>2</sub> contribute equally to carbonate weathering to the DIC signature, whether they argue here that DIC coming from soil is minor? This needs explanation. Reply: Soil CO<sub>2</sub> can contribute to riverine DIC by two ways. One is that soil CO<sub>2</sub> is involved in rock weathering and fixed as alkalinity in water; the other is that soil CO<sub>2</sub> can be dissolved by surface and rain water and then enter into the river. However, the former is the dominated process, and the later is insignificant (Dreybrodt, 1988; Aucour et al., 1999). Here we say that, as you questioned, “DIC coming from soil is minor” is referred to the later process. To avoid confusion, we revised this statement with some explanation.

(11) I do not understand the entire paragraph. Reply: We have revised and largely shortened this paragraph, in which only the significance of the in-river processes, particularly the in-river photosynthesis is addressed.

(12) 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. Reply: This statement has been modified as shown below: In previous discussion, the significance of the riverine primary production in the Xijiang river system has been inferred from seasonal and spatial variations in isotopic signature of DIC. It can be further examined from C/N ratios and carbon isotopes of POC.

(13) I am not sure to understand why authors compare the POC flux calculated from DIC to the CO<sub>2</sub> consumed by silicate weathering? because both are CO<sub>2</sub> sinks? If yes, 3% is not a high proportion and I do not agree with the last sentence of the chapter: 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

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sink is rather insignificant (I suggest also to delete this hypothesis from the abstract). Reply: I agree with this comment. The hypothesis has been deleted from the abstract and other relevant parts. The comparison of the POC flux transformed from DIC with the CO<sub>2</sub> consumed by silicate weathering is also deleted. Following your suggestion, the hypothesis concerning the old carbon reservoir effect on riverine POC is presented in the abstract.

In addition to the revisions mentioned above, we also polished the English throughout the manuscript. The following references have been added in the revised version.

References: Aucour, A. M., Sheppard, S. M. F., Guyomar, O., and Wattelet, J.: Use of <sup>13</sup>C to trace origin and cycling of inorganic carbon in the Rhône river system, *Chem. Geol.*, 159, 87-105, 1999. Das, A., Krishnaswami, S., and Bhattacharya S. K.: Carbon isotope ratio of dissolved inorganic carbon (DIC) in rivers draining the Deccan Traps, India: Sources of DIC and their magnitudes, *Earth Planet. Sci. Lett.*, 236, 419-429, 2005. Dreybrodt, W.: *Processes in karst systems*. Springer, Heidelberg, 1988. Li, S. L., Liu, C., Li, J., Lang, Y. C., Ding, H., and Li, L.: Geochemistry of dissolved inorganic carbon and carbonate weathering in a small typical karstic catchment of Southwest China: Isotopic and chemical constraints, *Chem. Geol.*, 277, 301-309, 2010. Mook, W. G., and Tan, F. C.: Stable carbon isotopes in rivers and estuaries, in: *Biogeochemistry of major world rivers*, edited by Degens, E. T., Kempe, S., and Richey, J. E., SCOPE report, 42, Wiley, Chichester, New York, 1991.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/8/C4635/2011/bgd-8-C4635-2011-supplement.pdf>

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

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