Biogeosciences Discuss., 10, C4137–C4142, 2013
www.biogeosciences-discuss.net/10/C4137/2013/

Biogeosciences
Discussions
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Interactive comment on "Partial pressure of CO_2 and CO_2 emission in a monsoon-driven hydroelectric reservoir (Danjiangkou Reservoir), China" *by* S. Y. Li and Q. F. Zhang

Anonymous Referee #1

Received and published: 7 August 2013

General Comments

The manuscript reports CO2 partial pressures (calculated from pH/alka) and emissions from a subtropical hydroelectric reservoir located in China. Between Nov. 2004 and Nov. 2005 with 6 field campaigns were conducted whereas for the following years (2006-2011), one to three field campaigns per year were conducted. Only a few study on this topic have a better temporal resolution. The sampling strategy allowed the authors to quantify two among the four well-known greenhouse gas pathways to the atmosphere: diffusion at the air-water interface at the lake surface and at the river downstream of the dam; potential degassing and emissions from soils surrounding the reservoirs are missing. The diffusion from the river downstream of the dam is usually

overlooked in most of the study despite evidences of non-negligeable fluxes observed when measured (Abril et al, 2005 in Global Biogeochemical Cycles; Guérin et al., 2006; Kemenes et al., 2007,2011).

The emissions from reservoirs located in the Asian region and especially from China were overlooked so far although Asian countries (overall) are the first hydroelectricity producers in the world and have 80% of their hydropower potential still undeveloped. Therefore, better estimation of current emissions from reservoirs in this part of the world could substantially improve global estimates of emissions from reservoirs and would facilitate predictions after the construction of future dams.

Although the work presented here is an original contribution of broad interest suiting well the scope of the journal, I recommend resubmission of the manuscript since it is difficult to evaluate the quality of the work as it is currently presented.

1- The overall organization of the manuscript could be improved. There are too many illustrations (Tables and Figures) and they could be presented in a slightly different sequence (Table 2 and 3 could be in the supplementary material, Fig 6 becoming Fig 2). In the result section, a huge part of the dataset is not presented.

2- The quality of the figures could be improved

3- The manuscript is lacking details about the methods for the different analysis performed (detection limit, precision, reproducibility. . .). This is particularly critical to evaluate the quality of the CO2 dataset which is very sensitive to the precision of pH and Alkalinity.

4- The discussion is very short and is a list of assumption that could not be supported by any data. The authors could not be conclusive on the controlling factors leading to seasonal and spatial variations of CO2 emissions that they attempted to determine with correlations of pCO2 with ancillary data (Nitrogen, Oxygen, Organic carbon. . .). It might be possible to improve this section if the dynamics of Nitrogen compounds, O2,

chlorophyll and nutrients is decribed in a revised version. In the case of a very short discussion as it is, I would recommend a single “Results and discussion” section rather than a discussion separated from the result section.

5- The manuscript needs to be rewritten with an individual with good expertise in English before re-submission.

Specific Comments

P10057

-L14-15: For global emissions from reservoirs, I would rather cite Barros et al. 2011, St Louis et al., 2001 and Lima et al., 2008.

-L17: Richey et al., 2002 is not about reservoirs but natural rivers. Important missing references about greenhouse gas emissions from tropical reservoirs are Abril et al., 2005 in Global Biogeochemical cycles and Roland et al. (2010) in Aquatic Sciences

-L19-21: The Petit Saut reservoir and the article by Delmas et al. (2001) in Global Biogeochemical cycles could be added

-L26: the reference to St Louis et al. (2001) has to be added after 321 TgC yr-1

P10058-10059: In the description of data available for China, the paper by Chanudet et al. (2011 in Science of the Total Environment) reporting emissions from two dams in Lao PDR should be considered. Climate in Lao PDR and China are both driven by Monsoon and the two regions are close enough to be considered together. The two long paragraphs on these pages could be shortened.

Section 2.1

P10059 L25-26: it must be said that the dam was built at the confluence of 2 rivers and the name of these rivers should be given. Explain here that the reservoir is divided in two distinct arms (Han and Dan)

P10060 L3-6: The figure 6 should be called here (becoming Fig 1). I would keep only the panel a from the original fig 6. In addition, this would permit defining the different seasons precisely.

L6-10: It would be interesting for the readership to show the water flow in a figure. It could be the second panel of the Fig 6 (becoming Fig 1)

L11: Is it at maximum water level? If yes, what are the average and the minimum?

L15-17: the characteristics of the stations should be given (depth, distance from the dam, in the in the Han or Dan arm of the reservoir. . .).

L19-20: I would add bars at the dates corresponding to field campaigns on Fig 6 (becoming Fig 1) in order that the readers have a better idea of the meteorologic and hydrologic conditions during the sampling.

Section 2.2 Water sampling and analysis

This section is not well organized and lacks coherence. Some procedures are well described (use of ICP-OES) whereas others are not described at all (Alkalinity, Chloa. . .). The equipment used, the precision, detection limit, accuracy and methodology must be given for all methods.

As said in the general comments, this is particularly important for pH and Alkalinity from which are calculated the pCO2. What pH probe was used for Alkalinity determination? How was it calibrated? What certified Reference solutions were used for Alkalinity?

Section 2.3 pCO2 calculations

Was it the pH from the YSI probe used for the calculations? According to the details that should be given, what is the precision of the pCO2?

Section 2.4 Water-air interface CO2 flux calculations

This section could be significantly shortened. However a few references related to gas

transfer velocity in freshwater reservoirs could be considered in the discussion for the choice of a k600 (Frost & Upstill-Goddard, 2002 in Limnol. Oceanogr. 47, 1165–1174, Guérin et al., 2007, in Journal of Marine Systeme, Wanninkhof et al., 1985. , Science 227, 1224–1226., Vachon et al., 2010, Limnology and Oceanography 55, 1723-1732). For instance, Wang et al. 2011 used Cole and Caraco (1998), they did not determined k600 themselves thus comparing the data obtained with the same relationship give no weight to the value which was determined. For both the river below the dam and the reservoir, the use of two extreme values of k600 (or an average$\pm$SD of selected values) would allow the authors to give a range for the emissions.

Section 3 Results: a subsection about ancillary data (turbidity, anions, cations, N, P, Chloa. . .) is necessary Alka, DIC, pCO2 and FCO2 could be discussed all together since they exhibit the same seasonal and spatial variations.

P10065 L1: The authors should be consistent, using either wet and dry seasons or winter, summer. . . A clear definition of the different seasons could be given in Section 2.1.

Section Discussion

I recommend the authors to derive correlations between pCO2 and the other parameters for each season and for each station and analyse the potential changes in these relationships. They should also have a look at temperature, water residence time, water flow. . . Is photosynthesis limited by a nutrient during the year?

For the calculation of the emissions from the reservoir,

-Did the specific emission factors from Han and Dan were attributed proportionally to the surface these two arms represents?

-Did the effect of the water level variations on the surface area of the reservoir were taken into account for the calculation of the emission from the reservoir?

What is the contribution of the downstream river to the total emissions from the reser-

voir? (disregarding degassing which could not be calculated) How does it compare with Abril et al. (2005), Guérin et al (2006) and Kemenes et al. (2007)?

The authors might combine in one graph all data from reservoirs available from China or Asia. They could do the same with rivers below dams and natural rivers in a second panel.

Tables 2 and 3: could be in supplementary material

Table 5: Data from Kemenes et al. 2011 and Guerin et al. (2006) in rivers downstream of dams could be added in the table. Data from Abril et al (2005) are more precise than those from Guerin et al (2006) for the Petit Saut Reservoir.

Figure 1: The Dan and Han arms could be clearly shown on the map.

Figure 2: What are the stations taken into account? This graph artificially gives the impression of a high temporal resolution. I would do a bar graph with the 12 months of a civil year and put for each month one bar/year in order to have a better view of seasonal variations and potential inter-annual variations for month that were sampled several years during the monitoring.

Fig 3: same comment as above

Fig 4: draw a line at 380-400 ppm for the pCO2 graph The lengend on the panel showing CO2 fluxes must be FCO2

Fig 5: set the 0 of the second Y axis at the same level as for the first one.

Fig 6: see my comments on section 2.1

Fig 7: why not doing the same for the 8 years?

Fig 8: see my comments on the discussion section

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