

## ***Interactive comment on “Impact of mire reclamation on export potential and characteristics of dissolved carbons in the Sanjiang Plain, Northeast China” by Y. D. Guo et al.***

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Received and published: 5 August 2012

Thanks very much for the comments from the Anonymous Referee ! I have learned a lot from the comments. The statements below are my reply to the comments, and I wish to obtain more advice from the referee and other researchers who have looked through my paper!

Comments 1: I have some reservations with the paper in that some of the data appear to have been previously published in Song et al. (2011) referenced below. I have

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suggested in the report below that this paper should be referenced in the manuscript. The papers appear to share common sampling sites in 2009, and given the similar approach taken in both papers, I am not sure that this manuscript merits full publication in Biogeosciences.

Response: Firstly, thanks for the comments! Here, I must formally declare that never the data in Song et al. (2011) were same to or used in my paper. I did the sampling work beginning at 2008 when is one year earlier then Song' work in 2009. So there is no necessary and possibility that my paper cites the data from Song. But in fact, there does be a common sampling site shared in both the papers, for the site of “degraded mire” is a very typical spatial point in the Sanjiang Plain. Secondly, I think my work is not a repeat work of Song, for there are two different sampling designs with different research ideals. My work emphasizes the succession and homoousia in all the ecological features of the three landscapes before mire reclamation so as to compare the alteration in dynamics of dissolved carbon under different strength of impact from reclamation. So I choose the three sample sites on behalf of three historically spatially connected landscapes in one river basin. It is a vital condition to choose the spatial-related landscapes for quantifying the difference from different reclamation history. Hence, my work aims to explain the temporal alteration in export concentrations of the dissolved carbon by the method of spatial comparison. There is some difference with the work of Song which emphasizes spatial difference of dissolved carbons in large spatial scale in the whole Sanjiang Plain. Perhaps this point was not clearly expressed in my paper for my poor English expression. I will add detailed explanation about my ideal to the reversion. Thirdly, Sanjiang Plain is large plain which possesses different wetland community and carbon pools, but a similar high disturbance strength of agricultural activities, in different river basins. Hence, that what kind of natural wetland was choosed as an original background to describe the characteristics of dissolved carbons before land reclamation is very important to quantify the effect of long-term influence of reclamation. The natural wetlands in the four rivers choosed as the background reference in Song's work were the wetlands in the river courses accepting water from

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the whole basins in which almost 85% of area is farmland. In my work, I selected the natural mires without any interference from agricultural activities since it is purposely protected for scientific research. So, the dissolved carbons from the different natural wetlands in the two papers are obviously distinct not only in the values but also in its representativeness. This discrepancy perhaps comes from the difference in the two work's design ideals as indicated above. Finally, the three years' sampling campaign with dense sampling activities in my work would effectively reduce the error from seasonal and inter-annual variability and provide meaningful quantified conclusions. Moreover, the application of the method of DOC fractionation and EEMs gives a clear insight into the alteration in the inner components of DOC in different sites. Of course, my paper contains some defects as referee pointed, I will do my best to make it better under the following instructions of the comments from the referee.

Comments 2: However, my main reservation with the paper is the degree to which the results reflect the hydrology of the sample sites. In this respect, it is unfortunate that no data are presented on flux rates, and insufficient information is provided on the hydrology of individual sample sites (i.e. characteristics of the three categories of drainage ditches).

Response: Thanks for the comments! It is really a very good advice to give the flux rates from the different sites. But as indicated above, the main idea of my work is to compare the difference among the sites using concentration and components of DOC as the main parameter. Alteration of runoff features in different landscapes under the same precipitation regime is an auxiliary explanation demonstrating to what degree the surface hydrological processes was altered by long-term reclamation. The coupling dynamics between water level and precipitation in different sites showed in my paper is a result of the concern. But it is not enough as the referee indicated. It is really a better method to quantify the flux rates of dissolved carbon and runoff from different landscapes. It gives me much inspiration. I plan to conduct the measurements and carry out a well-directed specific research on the flux rates in the following work. In fact,

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I am doing the basic work of calculating the area of each catchment for the different landscapes. I found there are much actual difficulties to obtain the exact catchment area of the degrade mire and paddy field in my paper. In the study area, the land elevation gradient was as small as 3/1000, especially the place far from the main river courses where the boundary of catchment exists. More confusingly, great amount of small roads and ditches resulting from the long-term agricultural activities make the boundary more obscure. I have wanted to interpret the boundary of degraded mire and paddy fields using image of Landsat/TM in 2010. But I failed after weeks of work on computer and in fields. A strange phenomenon I found is that some ditches in very flat area are communicating with two catchments synchronously, such as some area in the middle of Bielalong and Nongjiang river. So I want to use roads and banks of ditches between two basins as the boundaries on the basis of natural elevation points. It will be a hard time consuming work. But what puzzling me is the rationality to revise the natural boundary of the catchment. Here I wish to obtain some advice or affirmation from the referee, and I would be very appreciative! As to the insufficient information on the hydrology of individual sample sites, I will provide more detailed description for all the sites in the revision.

Comments 3: In places, also, the analysis is very descriptive, I am not sure how useful isolated EEMs are, for example, sampled in July (but year not given), and more data are required here to give confidence in the results.

Response: Thanks for the advice! I will add some information of HIX (humification index) from a series of EEMs to illustrate the humification features from different sites. I think the isolated EEMs are useful to reveal the difference in the components of DOC from each site. It is a direct proof of the difference of DOC source, degradability and interference strength from agricultural activity.

Comments:4 Abstract: the last sentence suggests that 'change in the hydrological regime of the mire landscape by sustained agriculture...'are responsible for the changes described. However, the hydrological regime is not described in the paper

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(water levels are presented in Fig. 3, but not flux rates), nor are details of agricultural practices.

Response: Thanks for the comments! Details about the content of hydrological regime of the landscapes are discussed in Comment 2. The description of hydrological regime is indeed a deficiency in my work. I will strengthen the survey about the hydrological regime in further research.

Comments:5 Mire: throughout the paper the authors refer to the sites as 'mire' wetlands. In Europe, mires generally refer to any peat accumulating wetland – but to avoid confusion the authors should define this term.

Response: Thanks for the advice! I will give a more detailed description on the characteristics of mire in my study area. I think the mire in Sanjing Plain has the common basic features to that in Europe.

Comments:6 Introduction: Much of the literature here is European and North American in focus. It would be useful if more information could be provided on the results of work completed recent in NE China. In particular, some of the sampling sites seem to be shared with Song et al., (2011) which is not referenced in the submitted manuscript, and it would be good to see some acknowledgement of this paper to give confidence that the papers are distinct and the same data are not used in both papers.

Response: Thanks for the comment. I will reference the paper from Song et al. (2011) in my reversed manuscript, and do a distinct comparison between the two papers.

Comments:7 Site Descriptions: latitude and longitude would be useful in section 2.1, together with elevation, and a description of the hydrological regime.

Response: Thanks for the advice! I will do an extensive supplement in the site description, and the exact position, elevation and hydrological condition.

Comments:8 In Section 2.2, more information is needed on individual sample sites (as noted above), and the distinction between the different categories of drainage ditch

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(particularly the 'capillary' ditch; C-DD). Details of sample storage (and the time between sampling and analysis) are needed in Section 2.3, together with an assessment of error (particularly given the use of 2 decimal places). Section 2.5 is not needed. It is also not clear, how water levels were measured (Fig. 2).

Response: Thanks for the advice! I will supply detailed information about the individual sample sites, and the detail processes of water level measurement will also be added.

Comments:9 Results& Discussion: this is very descriptive, and a more quantitative analysis, together with more comparison with published data elsewhere, would be more useful.

Response: Thanks for the advice! I will divide the part of Result from Discussion in the reversed manuscript, so that a more quantitative analysis and comparison with published data will be carried out.

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Interactive comment on Biogeosciences Discuss., 9, 5347, 2012.

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