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Biogeosciences Discussions

Interactive comment on "Nested atmospheric inversion for the terrestrial carbon sources and sinks in China" *by* F. Jiang et al.

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We would like to thank the anonymous referee 2 for his/her comprehensive review and detailed suggestions. These suggestions help us to present our results more clearly.

Referee 2: This paper uses a modified regional mask and TransCom L3 inversion method for estimating CO2 fluxes at regional scales. TM5 transport model is used for simulating interannually varying Response functions for the period 2000-2009. The CO2 fluxes and flux anomalies over China are given special attention, which is long overdue. However, the results are not so well presented. I have a strong feeling that the uncertainties in the inversion system is sometime overrated or underrated without sufficient rationale explanations. Thus the results and discussions have minimal impact

on our understanding of the CO2 sources and sinks over China. I cannot recommend publication of this paper. Please find below detailed issues and concerns.

Response: Thank you for your comments and suggestions. We agree that the descriptions and discussions about the uncertainties are insufficient, we have now discussed in more details in our revision.

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Line 14-15: The analyses of carbon flux variability with climate change/variability have been a matter of intense study in the past decade. I strongly urge the authors to review earlier publications.

Response: Thank you for your suggestion. The analyses of carbon flux variability with climate change/variability in this study are not our purpose of this study, but aimed at explaining the ecological rationality of the inverted carbon flux. However, we agree with the referee that it is important to make a comprehensive review for the relationship between the carbon flux and climate change. We have reviewed many more publications in our revision.

line 18: I understand there are latge number of data collected by various organizations in china. Have you look for possibilities for collaboration with the observational groups?

Response: Thanks for this comment. In fact, we have done our best to obtain more CO2 observation data in China. At the beginning of this inversion project, we established two observation stations, which locate at Xianlin, Jiangsu province and Qianyanzhou, Jiangxi province, respectively. The stations have been operational at the beginning of 2012. However, since the study period of this manuscript is 2000 to 2009, that data could not be used in this study. We also have been trying to cooperate with other research institutions, but it is very difficult in China. Through effort of nearly two years, we finally reached a cooperation agreement with Dr. Linxi Zhou group at Chinese Academy of Meteorological Sciences (CAMS) at the end of 2012, and obtained

observation data of three stations, which are from July, 2006 to Dec, 2009. We have added these data to the inversion system in the revised paper.

Line 23-25: Mistake in units? And I am sure this reference is wrong for 10 PgC/yr fossil emission. May be you should cite CDIAC. Also for the next sentence.

Response: Thanks for this comment. We have checked the values and citations in that sentence. The value of 10 PgC yr-1 is right, but the year of 2009 and the ratio of 55We have revised the sentence into ". . . In 2008, the carbon emissions from fossil fuel combustion, cement production and land use change reached 10 PgC yr-1, about 40

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Line 11-12: Please follow sign convention carefully. Are both these values sinks? Note different signs.

Response: Thank you for your suggestion. We have checked the whole manuscript, and modified the algebraic sign in our revision.

line 15-20: Cannot see the links for these climate model simulations here for this paper

Response: Thanks for this comment. Cao's work (Cao et al., 2003) is based on an ecosystem model (CEVSA model), rather than a climate model. In that paragraph, we mainly present a brief review of the studies of terrestrial ecosystem carbon sources and sinks in China based on inventories and using ecosystem models. The results based on inventories were shown first (Line 7 – Line 11; Fang et al., 2007; Wang et al., 2010), and it was followed by the results using ecosystem models (Line 11 – Line 17; Wang et al., 2007; Cao et al., 2003). Though Cao et al. (2003) do not show an explicit value of the carbon sink in China, they present a trend of the carbon sink in China as well as its potential variation induced by drying and warming climate.

line 26-27: Interesting writing style! how these two error can be compared. Are we talking apple and ornages?

Response: Thanks for this comment. We revised the sentence into ". . . Its main idea is that the CO2 concentrations at one site can be affected by the surface carbon fluxes through atmosphere transport, which could be simulated using an atmospheric transport model. If the simulated concentration deviation induced by the calculation errors of atmospheric transport are much smaller than that caused by the estimation of surface carbon fluxes, then, the errors of surface carbon fluxes could be corrected according to the biases between observations and simulations of the concentration"

line 29: Did Denning et al. do an inversion?

Response: That's a mistake. We have deleted this citation in the revised paper.

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line 1: Should give some credits to the CSIRO scientists. Apparently you are using the code written by Enting,Rayner,Law group. (eventhough I find reference to their work later on)

Response: Thank you for your suggestion. We also realize that the review of the inversion works in this manuscript is insufficient. We have strengthened it in our revision.

line 13, title and elsewhere: This cannot be called a nested method, here or in rest of the paper. Please discuss otherwise making your point clear.

Response: Thanks for this comment. Compared with the large regions generally used in TransCom, i.e., 11 land areas and 11 marine areas over the global surface, our method divides those areas of concern (e.g., North America of Deng and Chen, 2011; China of this study) into a number of small regions, and left the rest remaining larges regions. With this method, much more detailed carbon fluxes of the areas of concern could be inverted compared to the global large regions scheme. So, we named it as a nested inversion for the fact that many small regions are nested in large regions although the inversion for large and small regions is done simulataneously.

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Texts around line 15 : What is the justification of this fine region division? In the abstract you mention that there are very few or no data from China

Response: Thanks for this comment. Though there are very few observation data in China, there are a number of observation sites around China, such as in Japan, Korea, Taiwan and Hong Kong. These sites are downwind of China, hence, the observed CO2 data at these sites contain information on the carbon fluxes of different areas in China. And as responded in the second comment, we have obtained three sites of CO2 data in China from Dr. Linxi Zhou' group at CAMS, the three sites are located in Northeast China, Eastern China, and North China, respectively. We have added these data in our revision. After adding these data, it is more feasible for this fine region division in China.

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line 22-23: Please cite original source for fosil fuel emisions or give details how are the distributions made or the global totals scaled?

Response: Thank you for your suggestion. We will cite the original source for fossil fuel emissions used in this study as follow: "...These two types of fluxes are (i) the Miller Carbon Tracker fossil fuel emission field, which is constructed based on CDIAC 2007 (Boden et al., 2010) and EDGAR 4 databases (Olivier and Berdowski, 2001). Global, regional and national fossil-fuel CO2 emissions until 2007 were compiled in CDIAC 2007. The emissions in 2008 and 2009 are extrapolated from the 2007 CDIAC statistics using energy consumption statistics from the BP Statistical Review of World Energy 2010. More detailed descriptions could be found in the document of Carbon Tracker (http://carbontracker.noaa.gov)."

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Please coment about the performance of PISCES fluxes compared to Takahashi ocean. Since you are giving very small a prior uncertainties you should be careful

choose your prior fluxes.

Response: Thank you for your suggestion. We agree that we should be careful with the prior ocean flux and its uncertainty, because we use an ocean flux uncertainty of 0.67PgC yr-1 according to Baker et al. (2006), who used the Takahashi ocean fluxes. The OPA-PISCES-T model have been evaluated to have good performances on ocean carbon fluxes in the equatorial Pacific (Feely et al., 2006), North Pacific (McKinley et al., 2006) and Southern Ocean (Le Quéré et al., 2007). We have evaluated the annual global ocean fluxes calculated using OPA-PISCES-T model of -2.3 PgC yr-1, which is close to the result of Takahashi et al. (2009) (-2.0 PgC yr-1). Using the same ocean fluxes and uncertainties, Deng and Chen (2011) have successfully inverted the global land and ocean fluxes.

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line 1-3: change '...considered as 1 site...' to '...considered as independent site...' How did you account for the differences in calibration scales for the measurements from different institutes? I presume WDCGG archive data as supplied by the observational groups, without any correction.

Response: Thanks for this comment. The CO2 observations of HKO archived from WDCGG are daily data. Before being used in the inversion system, the data have been carefully checked and then smoothed and interpolated to weekly data using the curve fitting routine ccgcrv, which is the same as GLOBALVIEW. The ccgcrv code was downloaded from the website of ftp://ftp.cmdl.noaa.gov. We will add these descriptions in our revision.

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line 1: Did you use all hour data from the continuous sites or day/night only data are selected?

Response: Thanks for this comment. The simulated hourly data selection for the

continuous sites of GLOBALEVIEW-CO2 datasets is based on the information of Table 1 (Summary of strategies used to compute daily values from quasi-continuous data.) in Data Comparability section of "GLOBALVIEW-CO2, 2010: Documentation", while that for HKO sites, the hourly data of all-day were used.

line 9-12: You can find papers focussing on carbon balance of south/southeast asia using inverse modelling. And why 'may be', can you not check the postriori uncertainties from your inversion for the south, southeast and all other regions? A check on the uncertainty reduction is a must before interpreting the flux results in any case.

Response: Thanks for this suggestion. We will check the uncertainty reduction and add much more discussion on the uncertainties in the revised paper. We will also read the inversion papers focusing on the carbon balance of south/southeast Asia.

line 14-15: Why are the uncertainty for this inversion 1.48 is so different from that for Deng and Chen (0.49)? Apparently your prior uncertainties are very similar!

Response: We have double checked our inversion code, and found that we made a mistake on calculating the posterior uncertainties. The uncertainty for the inverted global land sinks is 0.65 PgC yr-1 in this study, which is a little large than the one (0.49 PgC yr-1) inverted in Deng and Chen (2011). The reason for this difference is that we used fewer CO2 observations compared to Deng and Chen (2011). We will revise this value in our revision.

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line 5 and thereabout: You should consider looking through earlier papers and references therein for IAVs in CO2 fluxes for different parts of the globe in relation with climate variations. In addition you might be aware that Gurney et al. used TransCom L3 regional basis functions, which do not include IAV in meteorology as well as fossil fuel emissions explicitely.

Response: Thanks for this comment. We will review more publications related to the

IAVs in CO2 fluxes for different regions of the globe and their relationship with climate in our revision.

line 15 onwards: Please add quantitative explanations here. If the inversion setups are different, you need to specifically state how are they different, e.g., a priori fluxes, uncertainties, etc. in quantitative manner. Too many use of 'may be' is not good.

Response: Thanks for this comment. We have revised the paragraph as follows: "The increasing trend in the terrestrial sink in this study agrees well with Deng and Chen (2011) and Le Quéré et al. (2009). During 2002 to 2005, our results are in the range of Le Quéré et al. (2009) and Deng and Chen (2011), i.e. stronger than Le Quéré et al. (2009), but weaker than Deng and Chen (2011), while after 2006, the land sinks are lower than Le Quéré et al. (2009) and Deng and Chen (2011). The main reason for the differences between this study and Le Quéré et al. (2009) is that the emission from biomass burning used in this study is larger (2.25 Pg C yr-1) than the emission (1.46 Pg C yr-1) caused by land-use change in Le Quéré et al. (2009), while the differences after 2006 is mainly attributed to the fact that there are large residual values (> 1.0 PgC yr-1) in Le Quéré et al. (2009), since the net budgets between this study and Le Quéré et al. (2009) are very close (4.35 vs 4.40 PgC yr-1). The differences between this study and Deng and Chen (2011) are caused by the partition of carbon sinks between land and ocean, since the carbon sinks of ocean (-2.56 PgC yr-1) inverted in this study is larger than that (-1.95 Pg C yr-1) obtained by Deng and Chen (2011), which are mainly caused by the selection in observations, as the selection of CO2 data in this study is much stricter than Deng' work, and much more CO2 data were used in Deng and Chen (2011), especially over continental region. "

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para 1: Again please check the signs before the numbers. Given the uncertainties, are these numbers different? Is this consistent with your conclusions at the end of the abstract. The discussions here leave me with a feeling that your inversion is working

perfectly.

Response: Thank you for your suggestion. We will check the signs, and add discussion on uncertainties.

para 2: So given the sparse measurement network and short lifetime of BVOCs, we will not be able to track the BVOC oxidation. Then it is perfectly fine to budget BVOCs as CO2, in my opinion. However, you can prepare a table by splitting the CO2 sources and sinks budgets over China by accounting all the flux components and non-CO2 species oxidation.

Response: Thank you for your suggestion. We will add a table in the revised paper according to your suggestion.

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first para: How good are these uncertainty estimates (too conservative, I feel), e.g., the national/regional CO2 flux uncertainties for USA or Europe are of the order of 0.3-0.4 PgC/yr at best. Do these uncertainties include interannual variations?

Response: As you pointed out, the uncertainties in Tian et al. (2011) and Piao et al. (2009)'s works are very small, that may be attributed to the methods they used (modelling studies).

para 2: These numbers are unbelieveable given the data network used for this inversion. I do not think we know such details for USA or Europe with that kind of dense network of surface, tower and aircraft observational network.

Response: The carbon sinks of forest, grass, and crop lands in China in this study are estimated based on the region division. As the region partition scheme in China is mainly based on land cover types, i.e., forest, crop, grass, and desert. We agree with referee that it is very difficult to estimate the carbon fluxes of each land cover types using inversion method, especially for China. We also pointed out that the estimations have large uncertaities at the end of that paragraph.

last para: Again I would recommend you to read earlier papers on the role of biomass burning and climate anomalies for land-atmosphere carbon fluxes. China certainly behaves different than the Southeast Asia, for example, in Response to ENSO cycle; what about the affect of NAO or AO cycles on climate over China?

Response: Thanks for this suggestion. We will investigate the effects of ENSO, NAO and AO cycles on climate over China, and show more detailed explanations to the variations of carbon sinks in our revision.

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The results of correlation shown in Figure 5 aren't conclusive.

Response: Thanks for this comment. We agree that the correlation between carbon sinks and drought areas shown in Figure is not significant. One reason is that the drought is only one of the factors which drive the variation of carbon sinks in China, and another reason is that the drought area is unable to reflect the whole drought condition in China. We have revised this Figure using drought index in our revision.

Figure 6 should be replotted showing same regional mask for both temperature and CO2 flux.

Response: Thank you for your suggestion. We will redraw this Figure in the revised paper.

Reference:

Le Quéré, C., RÂÍodenbeck, C., Buitenhuis, E. T., Conway, T. J., Langenfelds, R. and co-authors. 2007. Saturation of the Southern ocean CO2 sink due to recent climate change. Science 316, 1735–1738.

Feely, R. A., Takahashi, T.,Wanninkhof, R., McPhaden,M. J., Cosca, C. E. and co-authors. 2006. Decadal variability of the air–sea co2 fluxes in the equatorial pacific ocean. J. Geophys. Res. 111, C08S90. Deep Sea Res. 49, 2443–2469.

McKinley, G. A., Takahashi, T., Buitenhuis, E., Chai, F., Christian, J. and co-authors. 2006. North Pacific carbon cycle Response to climate variability on seasonal to decadal timescales. J. Geophys. Res. 111, doi:10.1029/2005JC003173.

Takahashi, T., S. C. Sutherland, R. Wanninkhof, C. Sweeney, R. A. Feely, D. W. Chipman, B. Hales, G. Friederich, F. Chavez, A. Watson, D. C. E. Bakker, U. Schuster, N. Metzl, H. Yoshikawa-Inoue, M. Ishii, T. Midorikawa, Y. Nojiri, C. Sabine, J. Olafsson, Th. S. Arnarson, B. Tilbrook, T. Johannessen, A. Olsen, Richard Bellerby, A. Körtzinger, T. Steinhoff, M. Hoppema, H. J. W. de Baar, C. S. Wong, Bruno Delille and N. R. Bates (2009). Climatological mean and decadal changes in surface ocean pCO2, and net sea-air CO2 flux over the global oceans. Deep-Sea Res. II, 56, 554-577

Olivier, J.G.J. and J.J.M. Berdowski (2001). Global emissions sources and sinks. In: Berdowski, J., Guicherit, R. and B.J. Heij (eds.) The Climate System, pp. 33-78. A.A. Balkema Publishers/Swets Zeitlinger Publishers, Lisse, The Netherlands. ISBN 90 5809 255 0.

Boden, T. A., Marland, G., and Andres, R. J.: Global, regional, and national fossil-fuel CO2 emissions, Carbon Dioxide Information Analysis Center, Oak Ridge National Laboratory, US Department of Energy, Oak Ridge, TN, doi:10.3334/CDIAC/00001V2010, 2010.

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