Responses to Referee #2 comment on "Contrasting terrestrial carbon cycle responses to the two strongest El Niño events: 1997–98 and 2015–16 El Niños"

Dear Referee and Editor,

Thank you very much for your efforts to deal with our manuscript and provide constructive comments. We have tried our best to re-summarize the results, and modify this manuscript accordingly. The following is our point-by-point reply to the comments.

(1) But my major concern regarding this paper is the data constrains they applied. The authors need to confirm their readers that atmospheric CO2 growth rate can provide constraint on a single event, and on small regional scales. The authors have shown that VEGAS is highly correlated with atmospheric CO2 growth rate, however, this does not ensure that VEGAS can capture net CO2 flux anomalies from a single event. For example, a recent study on ERL by Fang et al. found that mechanistic models can capture ENSO response fairly well when all years are considered, however, they all have some issues when considering only El Nino or La Nina years. It is ok to use VEGAS to explore the driving mechanisms; however, some caveats are needed.

Reply: Thanks very much for your suggestions. I totally agree with you that there are biases in all of the state-of-the-art model simulations (Piao et al., 2013; Sitch et al., 2015; Wang et al., 2016). Also, the atmospheric CO2 growth rate indeed cannot provide any constraint on regional scales. So we take some recent datasets including three inversions (MACC, CAMS, and CarbonTracker) and satellite-based observations (EVI and SIF) as references for spatial simulations by VEGAS. Of course, uncertainties exist among inversion datasets because of their different prescribed priors, a priori uncertainties, inverse methods, and observational datasets selected (Peylin et al, 2013). Maybe future inversions can give us more accurate results with the increased surface and satellite-based CO2 observations. Accordingly, we have added some discussions after the concluding remarks to inform readers that model and datasets used all have biases (or uncertainties). There is still a long road to improve DGVMs in modelling community.

(2) I agree with the other reviewer that statistical significance tests for anomalies, composites etc are needed, which may help strengthen the paper (i.e., Figure 2,3,4 etc).

Reply: Thanks very much for your suggestions. We have made the statistical significance tests for composite anomalies based on the bootstrap estimation. You can see them in the modified paper.

(3) I also agree with the other reviewer that it would be good to check whether seasonal evolution of climatic drivers, GPP and Respiration matter.

Reply: Thanks very much. In this paper, we mainly focus on the contrasting responses of terrestrial carbon cycle to the two extreme El Ninos (1997/98 and 2015/16) during the whole El Nino period. Also, we covered some information of seasonal evolutions in total C flux anomaly section (seen in Figure 2-4). The spatial seasonal evolutions during the El Nino events are also a good topic. Actually, we also want to present the seasonal evolutions during the 2015/16 El Nino with temperature and precipitation regional contributions by model sensitivity experiments in another paper.

(4) My other comment is about the fire emissions. The authors mentioned that FTA anomaly is 1.95 Pg C per yr during 1997-1998, while is 0.8 Pg C per yr during 2015-2016 (that is, 1.1 Pg C per yr difference between two events). In their paper, they showed that the difference of fire emission of CO2 from GFED is 0.82 Pg C per yr between these two events, so fire emissions only can explain 70% of the difference between two ENSO events, is this correct? Is it fair to conclude that fire emission dominates the difference and thus explore why fire emission differs in the paper?

Reply: Thanks very much. But I disagree with you.

First, according to $\delta F_{TA} \cong \delta TER - \delta GPP + \delta C_{fire}$, we can get δF_{TA}=1.14 Pg C yr^{-1}, δTER=$-$1.14 Pg C yr^{-1}, δGPP=$-$1.9 Pg C yr^{-1}, and δC_{fire}=0.38 Pg C yr^{-1} between 1997/98 and 2015/16 El Ninos simulated by VEGAS, respectively. So F_{TA} difference between two events is largely determined by differences in TER and GPP. Of course, fire emissions simulated by VEGAS was underestimated in 1997/98 (Table 2).

Second, GFED fire emission datasets used here only covers the period from 1997 through 2014 (Randerson et al., 2015). So we only have the Cfire anomaly with the value of 0.82 Pg C yr^{-1} in 1997/98 without the values in 2015/16. We cannot say "the difference of fire emission of CO2 from GFED is 0.82 Pg C per yr between these two events". So It is wrong that fire emissions can explain 70% of the difference between two ENSO events. We need more up-to-date observations to quantify the difference in fire emissions between two extreme El Ninos.

Detailed comments:

(1) abstract: seems to be too long, and has two paragraphs. Better to shorten it.

Reply: Thanks for your suggestions. We have tried our best to make the abstract clear and concise.

(2) I wonder if "two strongest El Nino events" used in the title and through- out the paper is appropriate. First, two strongest events are defined only since 1980, right? So it is not in history. Second, how to define how strong an El Nino is depends on which aspects you talked about. I would probably just use two strong El Nino events or two extreme El Nino events instead to make the statement more accurate.

Reply: Thanks for your constructive suggestions. We have modified "two strongest El Nino events" into "the extreme El Nino events" throughout the paper.

(3) Explain somewhere early in the paper that positive sign of the cartbon fluxes discussed here means to the atmosphere.

Reply: Thanks for your suggestions. We have added this information in the second paragraph in Introduction as follows "Directly, land-atmosphere C flux (F_{TA}, <u>positive sign is into the atmosphere</u>) is mainly attributable to the imbalance between the gross primary productivity (GPP) and terrestrial ecosystem respiration (TER)…"

(4) Introduction: There are actually more observation-based studies that argue temperature is more important driver. While many of the paper cited here in Line 78

are mostly model-based results, and models have be shown to over- estimate the role of precipitation (see, Piao et al., 2013 and Fang et al. 2017)

Reply: Thanks very much for your suggestions. We have added some paper such as Clark et al., 2003, Doughty et al., 2008 in Introduction to illustrate the observation-based evidence for temperature dominance.

(5) Introduction: line 86, here “sensitivity analysis” is not the right word and is misleading for this paper (wang et al., 2013), I think this number is the slope based on regression analysis.

Reply: Thanks very much. We have modified “sensitivity analysis” into “regression analysis” according to your suggestions.

(6) Results: Line 184-185: it is true that models can capture the general re- sponse to ENSO with a moderate correlation coefficient. However, a recent ERL study shows they have problem in capturing response to El Nino vs Response to La Nina.

Reply: DGVM models can well capture the response to ENSO with significant correlation coefficients (In this paper and Figure 5 in Wang et al., 2016) in long time series on interannual time scales. We also agree that there are biases in certain El Nino or La Nina event, about which we have added some discussions. We also added Fang et al. (2017) study result in the discussion to inform that state-of-the-art DGVMs may still have some problem in capturing response to El Nino vs Response to La Nina. In this paper, we also used three inversion results as references for VEGAS simulations. The spatial anomaly of F_{TA} in VEGAS in 2015/16 is consistent with that in CarbonTracker. This consistency gives us some confidence in model simulation results.

(7) Results: line 196-197, why use the mean of CAMs and MACC?

Reply: These two inversion datasets (CAMS and MACC, Chevallier, 2013) have similar results on the interannual time scales (Figure 1). So we take the mean of them as one reference dataset in the study.

(8) Figure 2c and 3d, why there appears to be two strong peaks for the inversion?

Reply: It's a good question. Comparing Figure 2c and 3d, we can know the two peaks mainly come from the tropical anomalies. We here present evolution of the spatial anomalies in CAMS and MACC during 1997/98 (Figure R.1). We can clearly see that strong positive anomalies occurred over the Indonesia, South Asia, Africa, part of Amazon, and Southern South America in tropics during the two peak periods (Aug-Oct 1997 and Mar-May 1998). In contrast, strong negative anomalies occurred over Southern Africa and Southern South America during the low period (Nov 1997 to Feb 1998).

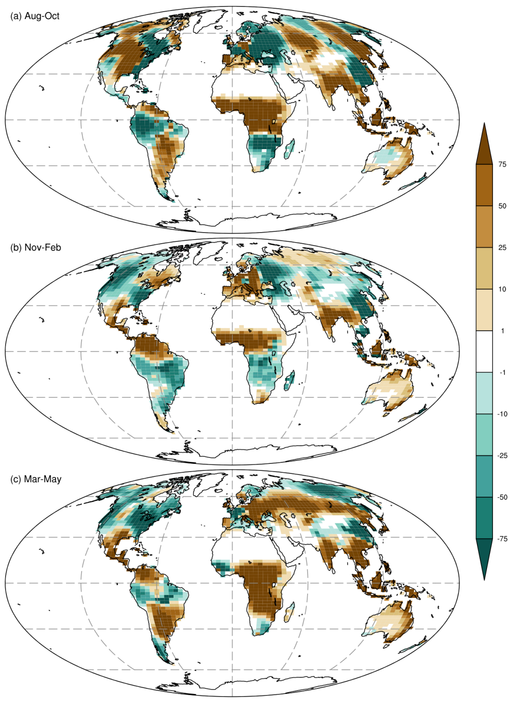

Figure R.1. F_{TA} evolutions in CAMS and MACC during 1997/98 El Nino.

Reference:

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during 1984-2000, P. Natl. Acad. Sci. USA, 100, 5852-5857, 2003.

(3) Doughty, C. E., and Goulden, M. L.: Are tropical forests near a high temperature threshold?, J. Geophys. Res., 113, G00B07, 2008.

(4) Fang, Y., Michalak, A. M., Schwalm, C. R., Huntzinger, D. N., Berry, J. A., Ciais, P., Piao, S. L., Poulter, B., Fisher, J. B., Cook, R. B., Hayes, D., Huang, M. Y., Ito, A., Jain, A., Lei, H. M., Lu, C. Q., Mao, J. F., Parazoo, N. C., Peng, S. S., Ricciuto, D. M., Shi, X. Y., Tao, B., Tian, H. Q., Wang, W. L., Wei, Y. X., and Yang, J.: Global land carbon sink response to temperature and precipitation varies with ENSO phase, Environ. Res. Lett., 12, 064007, 2017.

(5) Peylin, P., Law, R. M., Gurney, K. R., Chevallier, F., Jacobson, A. R., Maki, T., Niwa, Y., Patra, P. K., Peters, W., Rayner, P. J., Rödenbeck, C., van der Laan-Luijkx, I. T., and Zhang, X.: Global atmospheric carbon budget: results from an ensemble of atmospheric CO_2 inversions, Biogeosciences, 10, 6699-6720, 2013.

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P., Piao, S. L., Le Quéré, C., Smith, B., Zhu, Z., and Myneni, R.: Recent trends and drivers of regional sources and sinks of carbon dioxide, Biogeosciences, 12, 653-679, 2015.

(9) Wang, J., Zeng, N., and Wang, M.: Interannual variability of the atmospheric CO_2 growth rate: roles of precipitation and temperature, Biogeosciences, 13, 2339-2352, 2016.