Response to referee comments (in red):

Anonymous Referee #1
Received and published: 29 March 2016

This paper is competently written, and I don't find obvious errors in method, analysis, or results. My main complaint has to do with context and integration of results into previous results.
The authors spend some time reviewing previous, sometimes contradictory studies of the boreal and arctic regions
- conflicting browning/greening NDVI studies
- the 'carbon bomb' vs. the authors' results that don't show a large carbon efflux from permafrost regions
- high northern latitudes have decreasing sink, or even becoming a net source vs. the present study that disagrees with this result

After multiple readings of the paper, I'm not sure how far this work goes towards resolving any of these questions, but I think potential is there to do so. The basic result, that there is increasing CO2 uptake in the boreal region (not in the arctic) while the amplitude of arctic CO2 cycles has increased, seems reasonably well established by the results of their study. What I don't really get is a sense of how these results fit into the literature to confirm or deny other hypotheses as a means to clarify our understanding of this admittedly complex region.

In the introduction the authors say that "The net carbon balance of increased plant growth and increase soil respiration is unclear, but has important consequences for predicting carbon-climate feedbacks." By the end of the paper, I don't get the feeling that the authors make a definitive statement addressing this one way or the other. I believe this study has merit, and that any flaws are not fatal. A more rigorous organization of previous literature and the place of this study within our understanding would be helpful. Also, it seems that perhaps the authors are being too passive and 'nice' here, and are just presenting their results without directly confirming or refuting the work of others. Be bold! In the conclusion, state who among your predecessors you agree with, who you disagree with, and say why. You take the risk of perhaps ruffling a few feathers, but you will ensure response, and that's a very effective way to move science forward. (I'm reminded of a current disagreement between a group that hypothesizes that the Amazon experiences greenup during drought, and the group that believes this isn't the case. The issue has not been resolved, but there have been some very interesting studies that have come out of the dispute.)

We appreciate the reviewer's encouragement to take a stronger position on how our findings relate to the existing body of literature. We find no trends towards the carbon release that is often predicted for this region. Our limited temporal study is however unable to weigh in on whether that carbon release will ever occur in the future. We simply can say it hasn't happened yet. We can say that as a whole, the boreal region is maintaining carbon uptake strength in spite of the often discusses drought effects in Alaska and Canada. It could be that opposite trends in Eurasia are offsetting drought effects in North America. We do not feel comfortable using the inversion fluxes to attribute flux trends longitudinally between NA and EU. There aren't enough CO2 observation stations to constraint this well. We have expanded the conclusions section to draw the reader's attention to areas of conflict with previous literature. Including: "*Furthermore, our atmospheric inversions results show no evidence of an overall trend towards increasing $CO_2$ releases in either the boreal or Arctic zone over the 1985-2012 period. This is an important check for process-based biospheric models which have been challenged to predict the timing of an incipient 'carbon bomb' from the high northern latitudes (Treat and Frolking, 2013). At the moment, the increase in biomass productivity has appeared to be outpacing $CO_2$ losses from warming northern carbon-rich soils. Time will tell whether this trend continues, or whether it will reverse, due to nutrient or water limitations, etc., and become a net carbon source in a few decades as predicted by popular opinion among the community of experts (Abbott et al., 2016)*."

Some specific comments:
- Author is not listed in reference in the 4th paragraph of the introduction.

Fixed.

- The Jena inversion uses LPJ land flux and Mikaloff Fletcher/Takahashi ocean flux. What does the RIGC inversion use?

Added these details to the model description.

How are these surface fluxes similar/different, what might that mean for inversion results? Could these differences be the source of the RIGC peak CO2 uptake being double that of Jena (section 3.1.1)?

These priors are used as a starting point and allowed to change based on the inversion residual minimization. They could contribute to the differences in the inversion results. They also use different atmospheric transport models and entirely different model configurations. It is hard to identify one cause of the differences. The fact that they share many similar trends gives us some confidence that those trends are robust. Inversion models are known to vary widely in their magnitude of the fluxes. For that reason, interannual variability is the focus of our study (Baker et al., 2006).

- Section 3.3: the authors claim that the flux amplitude increase, shown in figures 3cd, is larger in the arctic than in the boreal regions. This is clearly true in the RIGC product, especially with regard to SON efflux. However, I'm not sure I agree that this is true for Jena. To my (subjective) eye, the summer uptake and fall/winter efflux amplitude increase is larger for both Jena products in the boreal region than in the arctic.

It's roughly the same absolute increase in the flux amplitude, but that's on top of very different mean seasonal cycles. It's the percentage increase that larger in the Arctic region.

- I'm a bit confused about the results shown in sections 3.5 and 3.6, Figure 11. Figure 3 clearly displays a strong amplification of July CO2 uptake, and Figure 8 shows a clear upward trend in JJA temperatures over the period of study. But Figure 11 (and references to studies in the text) correlate cooler summertime temperatures with increased uptake. What am I missing here? These seem contradictory. Is the moisture component the more important than the temperature?

The difference is that the records were detrended before the correlation analysis in Fig 11. There may be different drivers of long-term trends and short-term interannual variability.

*"In this analysis, all data sets were de-trended using a stiff spline to remove long-term trends, thus emphasizing processes controlling interannual variability (IAV)."*

- Section 3.5: Russell and Wallace (2004) and Schaefer et al. (2002) looked at carbon flux in relation to modes of climate variability such as the annular modes. Hurrell et al. (2001) discussed trends in the NAO itself. Would studies such as these help provide context here, or are they unrelated?

The annual modes are related in that they correspond to temperature and precipitation anomalies, but I don't think it's necessary to include them in the discussion. The analysis of temperature controls on NEE and NDVI is fundamental regardless of whether the temperature anomaly is caused by an annual mode or not. We did add a statement related to the Russell and Wallace findings…

*"The RIGC inversion shows significant correlation between warm winters and increased $CO_2$ uptake the following growing season (negative correlation), consistent with Russell and Wallace* (2004)*, but this relation did not appear in the Jena correlation."*

- Is the last paragraph of section 3.6 necessary?

It seems important for completeness, and it's interesting that the same patterns don't hold in the northern region.

- Figure S1: RIGC BA+BNA fossil fuel (ORNL/EDGAR) is about half the Jena anthropogenic flux for the same region (also EDGAR, but apparently different version.
Intuitively, I would expect that Jena uptake would have to be larger than RIGC to resolve observed CO2 concentration with these anthropogenic fluxes. Why isn't this the case?

*"The RIGC and Jena inversions use different fossil-fuel emissions datasets to isolate the net land surface fluxes related to biology. Comparing fossil-fuel emissions for the EU and BA+BNA Arctic and Boreal zones used in each inversion (SI Fig. 1) shows that while the mean emissions were lower in the RIGC inversion,*

*the IAV and trends in absolute fluxes were similar in each inversion. Differences in the fossil emissions are therefore unlikely to contribute significantly to trends in the biological land fluxes of the BA+BNA Arctic and Boreal zones."*

Remember that the long-term means have been removed in this analysis because of offsets like this among models.

- Patra et al. (2008) and Parazoo et al. (2008) discuss model resolution in relation to simulations of CO2. I wonder if advection of the effect of large surface CO2 flux into boreal/arctic regions is a partial (or dominant?) cause of the increasing amplitude of high-latitude CO2 concentrations? Or is Graven et al. (2013) the last word? What role might model resolution play? Are these issues not germane to this manuscript?

There is a body of literature suggesting that many transport models under estimate the vertical mixing, which would directly affect these inversion predictions, in particular when the measurement sites are located close to the intense source regions, e.g., the land biosphere or industrial centers. However, the fact that 2 different transport models produce similar $CO_2$ flux trends is reassuring. This consistency between the two inversions is obtained because the measurement sites used in both inversions are remotely located and designed to sampling marine air. Patra et al. (2008) have shown that the so called "site representation error" is high for the coastal or continental sites.

- Figure 11: There are significant correlations out to two years for RIGC and 4 years for Jena that are not discussed in the text. What might these long time-lag correlations mean?

Added this paragraph: *"Our analysis found significant correlations out to 2 to 4 years prior, suggesting that temperature anomalies could have an impact on NEE after several years delay. While there have been studies suggesting that multi-year lags between climate and $CO_2$ fluxes are important (e.g. Bond-Lamberty et al., 2012), these correlations were not consistent between the inversion models, preventing us for speculating as to the cause. "*

References
Hurrell, J.W., Y. Kushnir, M. Visbeck 2001: The north atlantic oscillation. Science, Vol. 291, No. 5504 (Jan 26 2001), p603-605.
Parazoo, N.C., A.S. Denning, S.R. Kawa et al., 2008: Mechanisms for synoptic variations of atmospheric CO2 in North America, South America, and Europ. Atmos. Chem. Phys., 8, 7239-7254.
Patra, P.K., R.M. Law, W. Peters, et al., 2008: TransCom model simulation of hourly atmospheric CO2: Analysis of synoptic-scale variation for the period 2002-2003. Glob. Biogeochem. Cy., 22, GB4013, doi:10.1029/2007/GB003081.
Russel, J. and J.M. Wallace, 2004: Annual carbon dioxide drawdown and the northern annular mode. Glob. Biogeochem. Cy., 18, GB1012, doi:10.1029/2003/GB002044.
Schaefer, K. A.S. Denning, N. Suits et al., 2002: Effect of climate on interannual variability of terrestrial CO2 fluxes. Glob. Biogeochem. Cy., 16(4), 1102, doi:10.1029/2002GB001928.

---

Response references:
Baker, D. F., Law, R. M., Gurney, K. R., Rayner, P., Peylin, P., Denning, A. S., Bousquet, P., Bruhwiler, L., Chen, Y.-H., Ciais, P., Fung, I. Y., Heimann, M., John, J., Maki, T., Maksyutov, S., Masarie, K., Prather, M., Pak, B., Taguchi, S. and Zhu, Z.: TransCom 3 inversion intercomparison: Impact of transport model errors on the interannual variability of regional $CO_2$ fluxes, 1988–2003, Global Biogeochemical Cycles, 20(1), GB1002, doi:10.1029/2004GB002439, 2006.

Bond-Lamberty, B., Bunn, A. G. and Thomson, A. M.: Multi-year lags between forest browning and soil respiration at high northern latitudes, edited by G. Bohrer, PLoS ONE, 7(11), e50441, doi:10.1371/journal.pone.0050441.t002, 2012.

Patra, P. K., Law, R. M., Peters, W., Rodenbeck, C., Takigawa, M., Aulagnier, C., Baker, I., Bergmann, D. J., Bousquet, P., Brandt, J., Bruhwiler, L., Cameron-Smith, P. J., Christensen, J. H., Delage, F., Denning, A. S., Fan, S., Geels, C., Houweling, S., Imasu, R., Karstens, U., Kawa, S. R., Kleist, J., Krol, M. C., Lin, S. J., Lokupitiya, R., Maki, T., Maksyutov, S., Niwa, Y., Onishi, R., Parazoo, N., Pieterse, G., Rivier, L., Satoh, M., Serrar, S., Taguchi, S., Vautard, R., Vermeulen, A. T. and Zhu, Z.: TransCom model simulations of hourly atmospheric

$CO_2$: Analysis of synoptic-scale variations for the period 2002-2003, Global Biogeochemical Cycles, 22(4), n/a–n/a, doi:10.1029/2007GB003081, 2008.

Russell, J. L. and Wallace, J. M.: Annual carbon dioxide drawdown and the Northern Annular Mode, Global Biogeochemical Cycles, 18(1), n/a–n/a, doi:10.1029/2003GB002044, 2004.