

Interactive comment on “Increasing summer net CO₂ uptake in high northern ecosystems inferred from atmospheric inversions and remote sensing” by L. R. Welp et al.

Anonymous Referee #1

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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

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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.)

Some specific comments:

- Author is not listed in reference in the 4th paragraph of the introduction.
- The Jena inversion uses LPJ land flux and Mikaloff Fletcher/Takahashi ocean flux.

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What does the RIGC inversion use? 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)?

- 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.

- 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?

- 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?

- Is the last paragraph of section 3.6 necessary?

- 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?

- 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

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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?

- 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?

References

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