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Biogeosciences Discussions

Interactive comment on "Feasibility for detection of ecosystem response to disturbance by atmospheric carbon dioxide" *by* Bjorn-Gustaf J. Brooks et al.

Anonymous Referee #2

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The authors present a study attempting to describe what sort of extreme-event-related carbon flux anomalies might be detectable by three Western Mountain measurements sites in the US. It is notably not an inversion study, but rather based on forward simulations wherein artificial flux anomalies of varying (spatial) size are defined, and then the sensitivity of the measurement stations to the resultant concentration anomalies is assessed, and compared to a defined model-data-mismatch for each station. As Peter Rayner has clearly pointed out, this does not really test whether such an anomaly could be detected by inverse modelling methods, but it does tell us something. The conclusions are rather grim, suggesting that only relatively large anomalies can be detected, and only if there is a measurement site more or less directly in the affected region.

The fact that this was not tested in an inversion framework likely makes the conclusion more pessimistic than necessary, and is a major limitation of the study.

I have some serious concerns related to the fundamental design of the experiment, and the fact that the anomalies tested are excessively artificial in nature. They are artificial not only in their spatial homogeneity, but also in their temporal consistency throughout the year. Referring to L17-20 on page 4: By choosing the difference between the minimum and the maximum, isn't this rather twice the expected interannual variability (i.e. twice the observed anomaly relative to the mean)? It clearly doesn't represent expected interannual variability when compared to the other fluxes in Figure 1, especially in the winter. (From Shiga et al. (2014) we know that it is easier to detect flux signal in the winter due to not only the smaller variability in the background fluxes - which you are neglecting, see following comment - but also due to the changes in dynamics and a shallower mixing height.) In any case, this does not seem like an anomaly that one could expect to see from the land biosphere.

But a bigger problem in my mind is the neglect of other flux signals, as described on L6-10 of page 9. Ignoring the confounding effects due to the variability of other fluxes is a critical shortcoming of the current study. Any attempt to perform an inversion to determine fluxes from concentration anomalies is dominated by such confounding effects. What is more, it is rather atypical that such a nicely behaved biogenic flux anomaly occurs, wherein the surrounding ecosystem is completely unaffected. Even in the "prototypical" case (which, while more realistic in its heterogeneity in time and space, was near undetectable in this framework), only the anomaly was considered. While it is clear that this spatially restricted anomaly was the only region that passed the statisical tests described by Zscheischler et al. (2013), that does not mean that the spatially and temporally surrounding fluxes were exactly identical to those of the previous year. Here you have really made the problem far too easy. As the footprints have already been calculated for the sites, an easy way to test the effect of the background fluxes might be to convolve the footprints with the CarbonTracker fluxes for the different years shown in Figure 1. Yes, the transport would be inconsistent with the flux patterns from a process point of view, but at least it would provide a more reasonable idea of the kind of "noise" one might expect to see, and compare it to the artificial anomalies.

At the same time, I have some concerns about your definition of the MDM, which is one value for each site for all days/months. The definition of MDM as a single value (not time-dependent) is certainly oversimplified, especially as it's not clear up until now (P6) how the measurements were sampled. There is much description of three-hourly fluxes and hourly concentrations, but this doesn't take into account the difficultly of accurately representing the mixing height at different times of the day. Often only afternoon (well-mixed) values are used in inversion studies because of the difficulty of representing the mixing height accurately at different times of the day. For high altitude sites such as these, different data selection might be appropriate. I scanned the Shiga et al. (2014) paper to see if it was further explained there, but didn't find the information easily. How is this treated in this study? Please include an explanation. There is a real danger with a synthetic data study that the capabilities of a network are overstated because the transport is fundamentally perfect. This can be addressed with the estimated MDM, but one value for a single site fails to take into account the systematic difference between, for example, nocturnal and diurnal representation errors. You state yourself that the signal is easier to detect in certain months than in others, related to different transport patterns. Does it not follow that MDM is likely not identical for all months? (As an aside, I'm surprised that the relatively coarse resolution of $1^\circ \times 1^\circ$ was able to capture the complex flows around mountainous terrain as well as is claimed here, but as it is taken from published literature I cannot object too strongly.)

Another, smaller concern relates to the fact that the measurement error is included in the MDM definition used in Shiga et al. (2014), which includes a sort of double counting, as the error is again addressed separately in this study. I doubt that it would seriously change the results here, but it is a clear flaw in the design of the current study.

Furthermore, no attempt is made to test how general this result is: Does it apply only

for these specific three sites? Is it a function of the complex flows related to the terrain? Or might this result be representative for other measurement networks as well? Some level of discussion setting this study in a larger context is lacking.

Unless these concerns can be at least partially addressed, I do not think this study is appropriate for publication in Biogeosciences.

Finally, a small question about my understanding of Figure 2: Does this mean that the ecosystem is a net sink at 8pm, 9pm, and 10pm local time in mid-July, and only at 11pm (33 hours before 8am local time) does respiration overcome GPP in the diurnal cycle? This seems exceedingly late. And is this with the +0.2 $\mu mol C m^{-2} s^{-1}$ anomaly, or is this the standard CarbonTracker flux? Perhaps this could be explained further.

Minor/typographical comments:

P4, L23 L30: 2008 year → year 2008

P5, L5: represents → represent

P6, L10: biggest found prototypical drought → biggest prototypical drought found

P9, L1: Differed by a factor of what? Two?

P9, L3: *the* intensity

P9, L20-21: A bit clunky in style, perhaps better phrased as an indirect question: We produced a data set of excess CO_2 values for each experimental case to determine what magnitude a carbon cycle anomaly must have in order to be detected by a Mountain West CO_2 observing station.

P9, L24-26: Are you really comparing these anomalies to the total, integrated US sequestration capacity? If so, the number seems awfully high. Is it really so that all the forests in the entire US can only sequester 14 TgC/month?

P9, L33-35: The thread of this sentence got a bit tangled along the way.

Sentence bridging P9-10: Review the commas here. (In general I found that throughout the text I had to add the occasional comma mentally to appropriately parse the clauses. This could be improved.)

P10, L10: Start sentence with "The".

P10, L24: *the* constant intensity

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