Please find our responses to comments from Referee # 2 in ***bold italics***

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Biogeosciences
Discussions

Interactive comment on "Novel applications of carbon isotopes in atmospheric CO_2: what can atmospheric measurements teach us about processes in the biosphere?" by A. P. Ballantyne et al.

Anonymous Referee #2

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This manuscripts extends an earlier study (Ballantyne et al., GBC 2010) of (almost) the same authors. The earlier study is, however, not discussed. It is mentioned exclusively to reference that NWR is a good background for the northern hemisphere.
So I have not understood the contribution of the current manuscript. The authors have to work out clearly the novelty of this work compared to the earlier study.

This work expands on our previous paper in Global Biogeochemical Cycles that focused primarily on one site in N. America. In this previous study we focused on isotopic observations in tree-ring cellulose as well as atmospheric CO_2, in addition to a modeling experiment to determine what factors were important in driving apparent seasonal isotopic discrimination. The current study builds upon the previous study by focusing strictly on the atmospheric observations from a global dataset. The distinction between where the other study left off and this study begins has been made more clearly in the introduction.

The revised manuscript benefits greatly from this thorough and thoughtful review. In particular, the referee's suggestion to use assimilation weighted values in our analysis has made our analysis more credible and realistic. Most reviewers approach a paper with the attitude of- 'what is wrong with this paper'; however, it is clear that this reviewer approached our paper with the attitude of- 'how can this paper be even better'. Although the criticisms presented by referee #2 required considerable re-analysis and re-consideration of the data, we believe that the revised paper is greatly improved.

Additionally there are a number of points that should be clarified:
1. It is not discussed that the source signature is a mixture of assimilation and respiration. This is astonishing given the finger-wagging section in the Miller & Tans (2003) paper which is repeatedly referred to. It is also essential because the disequilibrium will change over season as well, not only humidity.

This is a good point and one that we have taken into account by re-analyzing the data and revising the text. The mixture of respiration and assimilation signals presents the biggest problem during shoulder seasons, such as spring and fall, when respiratory losses may equal or exceed assimilatory gains by the biosphere. We addressed this by using modeled assimilation values to identify the extent of the growing season when assimilation exceeded respiration. For most sites this reduced the number of months included in our analysis by eliminating winter months. In fact, for Barrow, AK we only ended up using 4 summer months in our analysis. Based on this truncation of the annual cycle (Fig. 1) we updated Figure 3 to only include months during the growth season and re-calculated all the statistics in Table 1. This re-analysis of the data focuses in more on the growth season and eliminates winter months when fluxes are dominated by heterotrophic respiration and also many fall and spring months when fluxes may represent an admixture of assimilation and respiration. The result is a more focused analysis that highlights biosphere-atmosphere exchange during the growth season when respiration is dominated by autotrophic respiration that carries with it the isotopic signature of recently assimilated CO_2.

2. I wondered about the humidity calculations. It should be assimilation weighted in order to do the regression. It should also be in the footprint of the station. What is the footprint of Point Barrow? Especially of the filtered time series for maritime background air? Where and when did you take the humidity in the model?

Thank you for pointing out this oversight from our previous analysis. We are now only using assimilation-weighted mean monthly values of relative humidity, vapor pressure deficit, and temperature calculated at the leaf surface by the SiB model (Baker et al., 2010). Each assimilation-weighted value is taken from a single grid cell from the model encompassing the site from which observations were made. Each grid cell (1x1 degree) exceeds the spatial extent for the footprint for most sites included in the observation network. Point Barrow is an unusual site given its high latitude and proximity to the Arctic Ocean. We suspect that Point Barrow gives more of a circum-arctic perspective, especially during the 4-month growing season that we ultimately included in our analysis. However, despite only using mean values from these four months of the growing season, the correlation between $\delta^{13}C$ source values and VPD as well as RH are extremely high (see revised statistics in Table 1) and the RMSE with model estimates of discrimination are quite low, suggesting that $\delta^{13}C$ source estimates are in fact providing useful information about isotopic discrimination by the biosphere, at least during these 4 months. Although this revised analysis does not really affect the over-all conclusions of our investigation, the methods and results have been updated to include the details of the analysis as well as the implications for the results.

Section '3.3 Model Evaluation' has been revised accordingly:

'To test models designed to simulate the isotopic fractionation occurring during stomatal conductance, we used the simple biosphere model SiB biosphere model (Sellers et al., 1996). The model was driven by National Centers for Environmental Prediction Reanalysis Data (Kanamitsu et al., 2002) interpolated to the model timestep for the years 1983–2006. Maps of plant functional types were derived from remote sensing products (DeFries and Townshend, 1994). Mean monthly values of assimilation-weighted leaf surface temperatures (T) and relative humidity (RH) were calculated for each grid cell (1° x 1°) encompassing a network site using the most recent version SiB3 (Baker et al., 2010). For our regression analysis we only included months when net exchange between the atmosphere and biosphere was negative, resulting in an annual cycle that was truncated to the growing season. This was done to isolate the isotopic signal attributable to carbon that had recently been assimilated by the biosphere (Miller et al., 2003). Leaf T and RH were then used to calculate saturation vapor pressure and ultimately vapor pressure deficit (VPD). The assimilation-weighted values of RH and VPD were then used as the primary variables driving 2 commonly used stomatal conductance models- the Ball-Berry Model (Ball, 1988):'

3. I am really appreciating the method to determine delta_s quasi-continuously. And one extension of the current study compared to the earlier one is the greater number of stations. So it should be warranted that Niwot Ridge is still a good background for Ulaan Uul, for example. This is not discussed. NWR might be o.k. on the same latitude if you consider delta_s to be a monthly mean. But the flying carpet on the NOAA website shows a nice north-south gradient and gradual shifts in the seasonal cycle maxima and minima with latitude. I would also add caution to the statement that the Miller & Tans (2003) method is an alternative to the Keeling plot. It is an extension. A very good one, though, but it adds a varying background, not more. The rearrangement of the equations does not add anything extra, I think.

The observation made by the reviewer that NWR may not be a good background reference site for the entire N. Hemisphere is an astute observation and a valid point. The choice of background reference curves is not trivial. In fact, our previous analysis (Ballantyne et al., 2010) comparing model simulations with observations indicated that using a reference curve from the 'free-troposphere' above 3000 masl was the optimal reference curve for inferring isotopic source signature of fluxes from the

biosphere to the atmosphere. Unfortunately, such high elevation 'free troposphere' reference sites are not available for comparison with all the surface sites on all continents. The alternative would be to select the appropriate marine boundary layer reference curve for each site based on latitude. Although this would address the latitudinal issue identified by Referee #2 it would introduce other artifacts, such as isotopic signatures from air-sea gas exchange. We acknowledge that there are assumptions in using NWR as the reference curve for all sites in the Northern Hemisphere; however, using regional reference curves from surface sites would also require assumptions.

Text has been revised to state that Miller & Tans (2003) is an 'extension' of the Keeling Plot approach.

4. I think that the intuition in section 5.3 might be misleading. It might come from the implied time scales. Already in the introduction is the funny sentence that stomates "may respond to changes in atmospheric water vapor within weeks". They respond within in minutes, which is the basis of Ball-Berry and Leuning :-)
But on another time scale, humidity goes down over summer leading to soil water stress and reduced stomatal conductance. So you seem to think rather in this time scale. A summer signal is indeed building up in weeks to some month, exactly as in your delta_s.

This has been revised in the introduction to read:

'Using the $\delta^{13}CO_2$ composition of recently respired CO_2, researchers have been able to infer stomatal response to atmospheric water vapor, but that this isotopic signal may take weeks to be transmitted as respired CO_2 (Bowling et al., 2002). '

The discussion has been revised significantly to address stomatal response across a range of spatial and temporal scales.

Plants try to keep their Ci/Ca ratio rather constant if they have enough light and are not water-limited. So it probably goes down with soil water status rather than directly with atmospheric humidity.
From an ecophysiological perspective this is correct, stomatal conductance is really responding to the difference soil water availability and atmospheric water vapor, which we can think of as the gradient driving potential transpiration. Unfortunately, this potential transpiration gradient is not a common meteorological variable and thus we rely on other physical 'proxies' of this gradient. Although our analysis does not capture this actual gradient, it does evaluate two commonly used proxies of this gradient- relative humidity and vapor pressure deficit. Our analysis ultimately suggests that vapor pressure deficit is a more effective proxy of this potential transpiration gradient at the global scale.

A few minor things are:

a) Use a per mil sign in the plots.

All plots have been updated with ‰ sign.

b) It is d13C source in the figures but delta_s in throughout the rest of the text.

We acknowledge this inconsistency. However, it is always frustrating to read a paper with acronyms or symbols in figures that are unclear. After all most readers, first read the abstract and then look at the figures to decide whether they want to read the entire paper. If the figures are not absolutely clear and comprehensible chances are they will not take the time to read the paper.

c) The CO2 compensation point should have star, i.e. gamma* Without the * it does not include leaf/dark respiration.

This has been updated in the revised text.

d) Please be precise that you talk only about NOAA/INSTAAR data. In the introduction are a few paragraphs that make think that Scripps and CSIRO had never existed.

Good point, this has been explicitly stated in the revised introduction to read:

'Although we only include sites from the NOAA/ESRL global flask network (http://www.esrl.noaa.gov/gmd/ccgg/) in our analysis, these flask samples could be combined with other regional sampling networks or even eddy flux measurements where $\delta^{13}CO_2$ measurements are being made.'

e) What is the line in Fig. 4?

The line represents the probability density function of correlation coefficients, whereas the boxes represent a binned histogram of the correlation coefficients. This is more clearly explained in the revised caption of Figure 4. This figure has also been updated based upon the revised analysis.

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