See author comments to Referee #1 in **_bold italics_** below:

 General Comments

This manuscript makes use of a very impressive long-term data set of high precision CO2 and 13C-CO2 observations to investigate the patterns of 13C discrimination, their spatial coherence, and the factors controlling these large-scale patterns. The analyses push the use of these 13C-CO2 data into a new direction and are ultimately used to examine how two different stomatal conductance algorithms (used in land surface schemes) represent the observed patterns and amplitude of discrimination. The manuscript is generally well written, but there are some important details missing and assumptions that require further explanation. The introduction could be better framed by developing a set of science questions or hypotheses to be addressed – rather than fishing for… "What can atmospheric measurements teach us about processes in the biosphere?"

The introduction has been revised such that we provide enough background for the reader to understand exactly how the sparse network of carbon isotope observations in atmospheric CO₂ have been used conventionally in the past and how we propose that the richer network of data can now be used. Based on referee suggestions, we then frame our research as 1.) proposing a new mode of application for ¹³CO₂ and 2.) a hypothesis regarding stomatal conductance that we wish to test using empirical models.

Specific Comments
There needs to be a better description of the ds (source term) and big Delta – discrimination. These terms are vaguely defined (how are they related in your analysis – they are not the same thing). The big Delta (photosynthetic discrimination) values are usually defined as positive values in the literature…but not here. From the Keeling plot perspective the ds or dR value is often used as a surrogate for photosynthetic discrimination or ecosystem discrimination. This assumes that there is an isotope equilibrium between respiration and photosynthesis. I do not see how this can be the case – and it underlies much of your analysis. Please explain the rationale here and how your ds and big Delta values are related (what are the assumptions and limitations within your framework)?
The authors have ignored a rapidly growing body of literature that indicates significant short-term variation in the isotope composition of respiration. Both post photosynthetic fractionation effects and variability in the isotope composition of respiration undermine your analytical approach and data interpretation. Please justify your approach.

This has been revised in the new version. In order to alleviate confusion we have defined discrimination between the atmosphere and the ocean as ε_{ao} and we have defined discrimination between the atmosphere and the land as ε_{al} where both of these factors are negative to reflect the relative depletion of $\delta^{13}CO_2$ by the ocean and land (i.e. atmospheric CO₂ ~ -8 ‰ and C3 plants ~ -26‰). We first introduce ε_{ao} and ε_{al} in the introduction to describe how these values differ by an order of magnitude and how this difference has been used in the past to partition the global carbon budget. We then elaborate on ε_{al} in section 2.0 'Isotopic theory', where we have added the equations describing ε_{al} so that the reader can clearly see how it compares to the equations describing δ_s .

We are effectively using our calculated values of δ_s to the atmosphere to infer the isotopic signature of recently assimilated CO₂ by the biosphere (δ_l). Although there are other factors affecting δ_s, our previous analysis suggests that on seasonal timescales δ_s is driven by stomatal conductance

(Ballantyne et al., 2010). This is also substantiated by the literature and the following text citing recent literature has been added to the end of the isotopic theory section:

'Although it has been shown that the isotopic signature of respired CO_2 is dependent upon the isotopic composition of the carbohydrate consumed during decarboxylation on diurnal timescales (Tcherkez et al., 2003), on seasonal timescales the isotopic composition of respired CO_2 is thought to co-vary with the isotopic composition of recently assimilated sucrose (Scartazza et al., 2004). Moreover, respired CO_2 is dominated by recently assimilated carbon at weekly to monthly timescales (Högberg et al., 2001) and there is no apparent isotopic fractionation of this recently assimilated carbon during autotrophic respiration (Lin and Ehleringer, 1997). Our previous analysis using a global model revealed that the coherent patterns in $\delta^{13}C$ in cellulose of the biosphere and δ_s inferred from the atmosphere were driven by stomatal conductance (Ballantyne et al., 2010). Therefore our estimates of δ_s should be a suitable proxy for the isotopic signature of recently assimilated CO_2 (i.e. δ_l).'

The Ball-Woodrow-Berry model or the Leuning model predict stomatal conductance and this can be used to evaluate the variation in photosynthetic discrimination associated with changes in relative humidity or vapor pressure deficit. I do not see how this can be used to predict ds because there is no information related to the other side of this important equation – ecosystem respiration.

Here we are assuming that variability in δ_s is driven by respiration of recently assimilated carbon by the biosphere (see comment above and revised text). Our previous research (Ballantyne et al., 2010) indicates that isotopic discrimination during assimilation is the primary factor driving these seasonal cycles in δ_s, suggesting that our assumption of using δ_s as a proxy of respired CO_2 (i.e. δ_r) is valid. Furthermore there are numerous studies suggesting that the $\delta^{13}C$ values of respired CO_2 inferred from Keeling plots at the forest stand scale reflect the isotopic discrimination values one would expect from the Farquhar model, suggesting that both heterotrophic and autotrophic respiration produce little net fractionation (Bowling et al., 2009;Scartazza et al., 2004;Pataki et al., 2003). We are simply extending this rationale to the global scale by looking at atmospheric observations of $\delta^{13}CO_2$.

There has been significant debate in the scientific literature regarding the use of RH or vpd in the BWB model and Leuning model. The key references from this debate should be noted since this manuscript indirectly begins to resurrect this old problem.

This is a good point- maybe this debate should be resurrected. We have included the following paragraph addressing the current debate:

'There is considerable debate within the ecophysiology literature as to whether stomates respond primarily to VPD or RH. Although there is empirical evidence at the forest stand scale that stomatal conductance in some instances responds more to VPD (Bowling et al., 2002) and in other instances responds more to RH (Wang et al., 2009), a consensus has yet to emerge as to what is the primary metric of atmospheric vapor to which plants are responding. Part of this lack of consensus may be due to the fact that VPD and RH are not independent variables and thus strong empirical relationships may emerge between stomatal conductance and both of these variables. Recent efforts have turned towards combining the empirical stomatal conductance models evaluated here with optimization models to gain greater insight into stomatal sensitivity to both CO_2 and H_2O (Medlyn et al., 2011;Katul et al., 2010). In contrast, biosphere models seem to be

converging on VPD as the physical mechanism driving stomatal conductance (Medvigy et al., 2009;Cramer et al., 2001) and coupled global carbon-climate models seem to be converging on RH as the physical mechanism driving stomatal conductance (Friedlingstein et al., 2006), indicating a disconnect in how stomatal conductance is formulated at different spatial scales. Here we have analyzed and presented a global dataset of atmospheric observations that may provide new insight as to how plants respond to the soil-atmosphere water continuum.'

There is room in the discussion to consider that atmospheric water vapor and RH have been increasing significantly over time. How could trends in regional atmospheric water vapor impact your analyses and future patterns of photosynthetic discrimination?

This is a good point and is the direction in which we are currently taking this research. The following paragraph regarding changes in RH and VPD has been added to the discussion:

'The degree to which surface RH and VPD change in response to atmospheric warming remains uncertain. Based on the Clausius-Clapeyron relationship a 1°C increase in temperature should increase the atmosphere's capacity to hold water by ~7%. This relationship seems to hold true at the global scale, where a significant increase in specific humidity (kg H_2O vapor/kg dry air) has been attributed to anthropogenic warming over the latter half of the 20th century (Willett et al., 2007). Although it is clear that as the atmosphere warms it contains more water vapor, the response of RH is much less clear. At the global scale there does not appear to be significant trends in RH (Willett et al., 2007) and in fact spatially and temporally invariant RH seems to be an emergent property of global climate models (Held and Soden, 2000). If in fact, specific humidity is increasing (i.e. the amount of water vapor in the atmosphere) and RH (i.e. the ratio of the amount of water vapor in the atmosphere to the amount of water that the atmosphere could potentially hold) then VPD must be increasing at the global scale. However, more recent data suggests that surface RH may in fact be declining over land, possibly due to limited ocean moisture as the Earth's ocean surface warms slower than the land surface (O'Gorman and Muller, 2010). However, there is considerable regional variability in changes in RH with very limited data from tropical regions (Simmons et al., 2010). Therefore, determining whether stomatal conductance responds toVPD that is increasing globally, or to RH, that may be decreasing regionally, is critical to predicting future carbon assimilation by the biosphere and thus realistic future climate scenarios. '

Can you provide some explanation regarding the source footprint of your analyses based on the tower sampling?

Of the 18 sites included in our analysis only 5 of these sites were tall towers. This has been mentioned in the text and these sites have been identified in table 1. Although a rigorous footprint analysis of all these sites has not been performed, the footprint of the tall tower measurements is obviously much greater (~10^4 km^2) than the surface measurements (~ 1 km^2). This along with the appropriate citation (Helliker et al., 2004) has been added to the '3.1 Site Selection' section.

In the methodology please describe the frequency of the flask measurement at the various towers.
Tall tower sites were sampled daily and surface sites were sampled weekly. These details have also been added to the '3.1 Site Selection' section.

Why are the analyses restricted to the dependence on atmospheric humidity and no other environmental drivers. Do other environmental variables also yield similar weak correlations or do RH and VPD stand out as key drivers? I worry about a spurious correlation.

For this analysis we really wanted to use the atmospheric observations to test stomatal conductance models that are used for capturing the exchange of carbon and water between the atmosphere and biosphere. Thus we focused on two physical variables that have been identified as drivers of stomatal conductance- RH and VPD. Previous analyses have focused on empirical relationships between respired CO_2 and other environmental variables, such as precipitation amount and temperature (Pataki et al., 2003). Although many of the sites do show weak correlations between RH and δ_s, the correlations between VPD are very strong and very significant for more sites. Hopefully our results will inform the next generation of fully coupled carbon-climate earth system models that include different formulations of stomatal conductance (Friedlingstein et al., 2006).

Details

Abstract line 10 – "the Leuning model"

This has been fixed

Page 4606 last line – delete response

This paragraph has been revised according to Referee's first General Comment.

Equation 4. I would explain what the background value is and how it is obtained when describing this equation. Not later.

The section 'Analytical Approach' where this is described has been moved up so that it follows the section on 'Isotopic Theory'.

Page 4609 line 1. Delete "values" repetitive

This has been deleted.

Page 4609 line 20. Do you mean R or RH here?

This has been changed.

Page 4609 line 20. This should be Ball-Woodrow-Berry (BWB)

This has been changed throughout the MS.

Be consistent with the sign of big Delta values (the intro and discussion sections seem to use a different convention)

This has been changed to ε values to which we define as negative to illustrate depleted (e.g. more negative values) in the biosphere.

Page 4611 line 1-20. Is this section necessary? The method has been used by Miller and others and Lai and others.

This section has been moved to follow the section on Isotopic Theory as suggested by the referees previous comment.

Page 4611 1-25. But is there any reason why this would not be the observed pattern? This seems obvious.

Although one would expect the free troposphere (>3000 MASL) to show a much more attenuated seasonal cycle than the surface, it is not entirely intuitive what happens mathematically when you take the residuals of these two curves and solve for the δ_s term.

Page 4612 line 5 "Baltic Sea"

Corrected

Page 4612 line 20. The meaning here is not clear. Please revise.

This has been revised to read:

'These results indicate a non-linear response of stomatal conductance to atmospheric water vapor, especially at the sites that were more responsive to VPD.'

Page 4615 line 5-10. Can you select a different background value to see if the stair-step pattern persists?

Unfortunately, there are insufficient observations from the free troposphere at high latitudes, such as Barrow, AK to specify a different background reference curve. We are currently looking at different background reference curves for sites outside of N. America, but that is an analysis for a separate paper.

Page 4619 line 10-15. This discussion is a bit awkward as written and should be revised for clarity

This paragraph has been revised. Essentially, we are evaluating the expected relationships between δ_s and RH and VPD, as laid out in the preceding paragraph. This paragraph has been clarified.

Page 4618 – last line. This is an incomplete sentence.

This is now a complete sentence

Page 4620. Check spelling on stomatal.

'stomatal' corrected

Page 4621. I think this discussion should be removed. These models have been tested at the leaf and ecosystem scale for a broad range of ecosystems. Some of the problems/disparities observed here could easily be problems related to the isotope analyses or the implementation of these stomatal models for grid cells where the land use is not properly prescribed.

This is precisely why this paragraph should remain in the discussion. Both of these stomatal conductance models have been verified with reasonable success at the leaf and ecosystem scale, but they should be applied with caution to the global scale. The specific problems mentioned by the referee have been added to this paragraph. Furthermore, this paragraph leads directly into the following paragraph concerning observed and modeled changes in RH vs. VPD at the global and regional scales.

Overall recommendation: Major revision

References

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