Response to the comments of Anonymous referee #1

We would like to thank the anonymous referee for his/her comments that were very helpful to improve this manuscript. Please see below our responses in blue to the referee #1 comments.

Specific comments:

1. The authors refer several times to the diel variability in d18OF becoming more enriched in the daytime. I interpret this as less negative delta values. However, in Fig 8, I see more negative delta values for d18OF during daytime. I do not see evidence presented to support the authors' conclusions in this case. Please correct this error, or show more data to support enriched daytime d18OF.

p. 7689 line 8: "The daytime enrichment", **now reads**: "The daytime depletion".

p. 7691 line 7: "with more enriched", **now reads**: "with more depleted".

2. p. 7673, line 4: Also cite 13C studies in addition to 18O studies, e.g. Ciais, Francey, and Keeling publications.

p. 7673 line 4: "(Farquhar et al., 1993; Yakir and Wang, 1996; Cuntz et al., 2003)"

now reads: "(Keeling, 1958; Farquhar et al., 1993; Ciais et al., 1995; Yakir and Wang, 1996; Cuntz et al., 2003)"

3. In the supporting measurements section (p. 7679), please provide more details. How often did the auto chamber make measurements? Light or dark chamber? List the soil chamber model number as well.

p. 7679 line 10 "chamber (Li-8100, Li-Cor) with a 20 cm diameter. To correct the automated chamber measurements (F_{static})"

now reads:

"dark chamber (Li-8100, Li-Cor) with a 20 cm diameter. Automated chamber measurements (F_{static}) were taken every 15 minutes and averaged into half-hour intervals. To correct F_{static}".

How was precipitation collected for isotope analysis?

p. 7679 line 19: “In addition, the δ^{18}O of precipitation water ($\delta^{18}O_{pw}$) was determined on an event basis.”

Now reads:

“In addition, the δ^{18}O of precipitation water ($\delta^{18}O_{pw}$) was determined on an event basis. The precipitation water was collected using a plastic funnel (~15 cm diameter) paced on the top of a thermally insulated plastic bottle and then stored in sealed glass vials.”

No measurement of the canopy air temperature?

In this study air temperature was measured in an open pond area near the study site, as described in **p. 7680 line 3**.

4. Section 2.4, needs some work. Make it clear this goes into the IFR description immediately. I think this needs to be laid out a little differently. It’s a little sloppy as is. First you should describe the ratio of the fluxes of the isotopologues (RF = heavy/light) to derive the deltaF of the ecosystem fluxes. deltaF = eqn 1. RF becomes eqn 2a.

p. 7680 line 9: “Three methods were used to determine the isotope composition of CO_2 flux near the forest floor: IFR, mKP and WT. Griffis et al. (2004) proposed the use of the ratio between fluxes of heavier and lighter isotopologues to derive the isotope ratio of ecosystem fluxes:

$$\delta_F = \left(\frac{R_F}{R_{VPDB}} - 1 \right) \times 1000 \tag{1}$$

where δ_F is the isotope ratio of the CO_2 flux (‰). R_F is the ratio between the heavier and lighter isotopologue fluxes (F^{heavy}/F^{light}) defined as F^{13}/F^{12} for $^{13}CO_2$ and $^{12}CO_2$ fluxes, or $0.5F^{18}/F^{16}$ for $C^{18}O^{16}O$ and $C^{16}O_2$ fluxes. Using the flux-gradient method to express CO_2 fluxes:

$$F^{heavy}/F^{light} = \frac{-\left(K\overline{\rho_a}/M_a\right)d\overline{[heavy]}/dz}{-\left(K\overline{\rho_a}/M_a\right)d\overline{[light]}/dz} \tag{2a}$$

where K is the eddy diffusivity, which was assumed to be the same for the heavier and lighter isotopologues, $\overline{\rho_a}$ is the mean density of dry air, M_a is the molar mass of dry air and $d\overline{[heavy]}/dz$ and $d\overline{[light]}/dz$ are the time averaged gradients of the heavy and light isotopologues measured simultaneously at the same height. For practical applications Eq. 2a can be rewritten as:

$$F^{heavy}/F^{light} = \frac{\overline{[heavy]}_{z_2} - \overline{[heavy]}_{z_1}}{\overline{[light]}_{z_2} - \overline{[light]}_{z_1}} \qquad (2b)$$

where $\overline{[heavy]}$ and $\overline{[light]}$ are the half-hour mean mixing ratios of isotopologues, measured at two heights (z_1 = 0.45 and z_2 = 1.45 m) near the forest floor. Large uncertainties in the estimates provided by the IFR method were observed when the gradient of $^{13}CO_2$ mixing ratio was smaller than 0.035 µmol mol^{-1} m $^{-1}$, which, considering the typical proportion of $^{13}CO_2$ in the air, corresponds to a gradient of CO_2 mixing ratio of approximately 5 µmol mol^{-1} m^{-1}. The same threshold was also observed by Griffis et al. (2005a). Hence, only half-hours in which the difference of CO_2 mixing ratio between two measurement heights was larger than 5 µmol mol^{-1} were used to calculate δ_F.

now reads:

"Three methods were used to determine the isotope composition of CO_2 flux near the forest floor: IFR, mKP and WT analysis. Griffis et al. (2004) proposed the gradient diffusion theory to calculate the ratio between fluxes of heavier and lighter isotopologues (F^{heavy}/F^{light}), as follows:

$$F^{heavy}/F^{light} = \frac{-\left(K\overline{\rho_a}/M_a\right)d\overline{[heavy]}/dz}{-\left(K\overline{\rho_a}/M_a\right)d\overline{[light]}/dz} \qquad (1a)$$

where K is the eddy diffusivity, which was assumed to be the same for the heavy and light isotopologues, $\overline{\rho_a}$ is the mean density of dry air, M_a is the molar mass of dry air and $d\overline{[heavy]}/dz$ and $d\overline{[light]}/dz$ are the time averaged vertical gradients of the heavy and light isotopologues measured simultaneously at the same height. For practical applications Eq. 1a can be rewritten as:

$$F^{heavy} / F^{light} = \frac{\overline{[heavy]}_{z_2} - \overline{[heavy]}_{z_1}}{\overline{[light]}_{z_2} - \overline{[light]}_{z_1}} \tag{1b}$$

where $\overline{[heavy]}$ and $\overline{[light]}$ are the half-hour mean mixing ratios of isotopologues, measured at two heights (z_1 = 0.45 and z_2 = 1.45 m) near the forest floor. Large uncertainties in the estimates provided by the IFR method were observed when the gradient of $^{13}CO_2$ mixing ratio was smaller than 0.035 µmol mol^{-1} m $^{-1}$, which, considering the typical proportion of $^{13}CO_2$ in the air, corresponds to a gradient of CO_2 mixing ratio of approximately 5 µmol mol^{-1} m^{-1}. The same threshold was also observed by Griffis et al. (2005a). Hence, only half-hours in which the difference of CO_2 mixing ratio between two measurement heights was larger than 5 µmol mol^{-1} were used to calculate F^{heavy}/F^{light}, which was expressed in delta notation, as follows:

$$\delta_F = \left(\frac{R_F}{R_{VPDB}} - 1 \right) \times 1000 \tag{2}$$

where δ_F is the isotope ratio of the CO_2 flux (‰), R_F is defined as F^{13}/F^{12} for $^{13}CO_2$ and $^{12}CO_2$ fluxes, or $0.5F^{18}/F^{16}$ for $C^{18}O^{16}O$ and $C^{16}O_2$ fluxes, and R_{VPDB} is the molar ratio ($^{13}C/^{12}C$ or $^{18}O/^{16}O$) of Viena Peedee Belemnite."

On line 21 'averaged vertical gradients'.

p. 7681 line 21: "are the time averaged gradients" **now reads:** "are the time averaged vertical gradients"

The WT method description seems sloppy also. If Eqn 4 sums over j layers, why is it labeled as Fj? Do you mean the cumulative flux for each species instead of for each layer (line 16)? I don't really understand how this method is applied and would appreciate more details either here or in the appendix.

The cumulative flux for each *j* layer (F_j) was calculated according to Warland and Thurtell (2000) and Raupach (1989). The cumulative flux calculation is generally used to estimate the net flux over the canopy, which was not used in this study. We have changed the wording and Eq. 4 to clarify these points:

p. 7682 line 11:

"where *i* and *j* are the concentration (*C*) and source (*S*) layer indices, respectively, Δz_j is the thickness of the source layer *j* with respect to the vertical coordinate (*z*) and **M** is the dispersion matrix,"

now reads:

"where *i* and *j* are the concentration (*C*) and source (*S*) layer indices, respectively, *m* is the number of source layers, Δz_j is the thickness of the source layer *j* with respect to the vertical coordinate (*z*) and **M** is the dispersion matrix,"

p. 7682 line 16:

"The cumulative flux (F_j) for each source layer is given by:

$$F_j = \sum_{j=1}^{m} S_j \Delta z_j \tag{4}$$

where *m* is the number of source layers, which was set to 4 in this study. The bottom source layer thickness (Δz_1 = 1.4 m) was set to match the two lowest concentration intake heights. Estimated $^{12}C^{16}O_2$, $^{13}CO_2$ and $C^{18}O^{16}O$ fluxes for the ground level source layer were converted to delta notation using Eq. (1)."

now reads:

"The flux (F_j) for each source layer is given by:

$$F_j = S_j \Delta z_j \tag{4}$$

Estimated $^{12}C^{16}O_2$, $^{13}CO_2$ and $C^{18}O^{16}O$ fluxes for the ground level source layer (S_1) were converted to delta notation using Eq. (2) and compared with the IFR estimates. To do that, Δz_1 was set to 1.4 m to match the two lowest concentration intake heights.

p.7691 line 16:

“(Eq. 3). The dispersion matrix”

now reads

“(Eq. 3). A full description of this analysis and additional details in the derivation of **M** can be found in Warland and Thurtell (2000). The dispersion matrix”

5. The last paragraph in sec 3.3 doesn’t seem to apply to the WT comparison. It’s mostly a discussion of theory regarding the IFR method.

This paragraph was included because the use of the IFR inside canopy has raised concerns due to well known limitations of the gradient diffusion theory inside plant canopies. We believe that we need to demonstrate to the readers that the IFR performance was not affected by the length scale of turbulence in this study, as concentration gradients were not measured in the canopy, but in the trunk space, close to the ground, where the turbulence length scale is not expected to affect IFR performance. We have chosen to discuss that in sec 3.3 because in this section we also discuss turbulent transport.

It seems the IFR comparison with WT is somewhat tricky because IFR works better in stable (low u_star) conditions and WT works better when u_star is high. For that reason, this seems like a limited comparison and maybe not the best way to verify the IFR method.

We agree with the referee that there are some the limitations with the comparisons between WT analysis and IFR, but we decided to present the WT analysis results because this methodology could be further evaluated and compared with the IFR method in future studies.

6. Sec 3.4, third paragraph. d18OR wasn't 'observed' in the study, it was modeled. d18OF was observed. This argument seems contradictory as d18Oa increases during the daytime and d18OF decreases during the daytime. Please explain.

p. 7688 line 19

"Wingate et al. (2010) also observed more depleted $\delta^{18}O_R$ values in the daytime in a Maritime pine stand which were synchronized with variations in $\delta^{18}O_a$ and F_R. In this study, diel cycles of $\delta^{18}O_a$ (Fig. 5) were also observe near the forest floor. Seibt et al. (2006) suggested that the ambient air invasion into the soil was responsible for the $\delta^{18}O_R$ diel cycle observed in a temperate *Picea sitchensis* plantation, and that this cycle could be amplified by acceleration of CO_2 hydration associated with soil CAE activity. This also seems to be a plausible explanation for the diel $\delta^{18}O_R$ trends observed in this study."

now reads:

"Wingate et al. (2010) also reported more depleted $\delta^{18}O_R$ values in the daytime in a Maritime pine stand which were synchronized with variations in $\delta^{18}O_a$ and F_R. In their study $\delta^{18}O_a$ was more enriched during the daytime, similarly to $\delta^{18}O_a$ daily trend observed in the present study (Fig. 5). On the other hand, Seibt et al. (2006) observed an opposite trend in $\delta^{18}O_a$ close to the soil surface, with values around 3‰ during the night and -2‰ in the daytime. They observed that daily variations in $\delta^{18}O_R$ were coupled with $\delta^{18}O_a$, and that the $\delta^{18}O_R$ diel cycle could be amplified by acceleration of CO_2 hydration associated with soil CAE activity. This explanation does not apply to our study as changes in $\delta^{18}O_a$ and $\delta^{18}O_R$ occurred in opposite directions. One hypothesis to explain our daily variations in $\delta^{18}O_R$ is that diel variations in $\delta^{18}O_{sw}$ could lead to changes in $\delta^{18}O_R$, but a more frequent soil water sampling scheme would be required to test this hypothesis.

Technical corrections:

title and throughout: I think the authors should be consistent in how they denote the isotopologues throughout the manuscript. For example, if 16O and 12C are implied, they should appear as: CO2, C18OO, and 13CO2.

Please replace $C^{18}O^{16}O$ by $C^{18}OO$ in:

p. 7671 – title
p.7672 line 2
p.7673 lines: 5, 17 and 24
p. 7674 lines: 1, 12 and 15
p. 7675 lines: 1 and 17
p. 7677 line 1
p. 7678 line 22
p. 7680 line 16
p. 7682 lines 4 and 20
p. 7683 line 16
p. 7685 line 1
p. 7693 line 19
p. 7694 line 19
p. 7703 – Table 2
p. 7704 – fig 1 caption
p. 7711 – fig 8 caption

p. 7672 line 13:

“were calculated with estimates provided by a modified”

now reads:

“were calculated by IFR and compared with estimates provided by a modified”

line 22:

“δ^{18}O-CO$_2$ flux”

now reads

“$\delta^{18}O_F$”

line 26: I see lower values of d18OF during the daytime in Fig 8.

Correction made above.

line 27:

“carbon anhydrase”

now reads:

“carbonic anhydrase”

line 27:

“very variable”

now reads:

“quite variable”

p. 7673 line 17-19: I don't like how this sentence is worded. The 'differences' in soil and leaf 18O fluxes are not 'caused' by the isotopic equilibration between CO2 and H2O. They are caused by the evaporative enrichment of leaf water as you describe in the next sentence. Please reword. line 22: Welp et al is not an appropriate citation here. Perhaps something more classic like Craig and Gordon?

p. 7673 line 16:

"On the other hand, the use of $C^{18}O^{16}O$ as a tracer relies on differences in ^{18}O composition ($\delta^{18}O$) of CO_2 fluxes originating from soil and plant canopies, caused by the isotopic equilibration between CO_2 and liquid water in soils and leaf tissues (Gillon and Yakir, 2001)."

now reads:

"On the other hand, the use of $C^{18}O^{16}O$ as a tracer relies on differences in ^{18}O composition ($\delta^{18}O$) of CO_2 fluxes originating from soil and plant canopies (Gillon and Yakir, 2001). The $\delta^{18}O$ of canopy and soil CO_2 fluxes is coupled with $\delta^{18}O$ of soil and leaf water."

p. 7673 line 22-23: Actually it would be using three isotopologues including 12C16O2. Perhaps say 'using carbon and oxygen isotope tracers'.

p. 7673 line 22:

"Ogée et al. (2004) proposed the use of the two isotopologues for partitioning the net CO_2 ecosystem exchange into plant and soil components."

now reads

"Ogée et al. (2004) proposed the use of carbon and oxygen isotope tracers for partitioning the net CO_2 ecosystem exchange into plant and soil components."

p. 7674 line 3: Also add varying environmental conditions to the list.

p. 7674 line 3: "variables such as efficiency of the CO_2 hydration, kinetic fractionation, isotopic composition of water in the soil and foliage"

now reads: “variables such as changes in environmental conditions, efficiency of the CO_2 hydration, kinetic fractionation, isotopic composition of water in the soil and foliage”

line 12: Cite chamber studies, e.g. Powers and McDowell et al 2010

p. 7674 line 12:

“(Law et al., 1999; Davidson et al., 2002)”

now reads:

“(Law et al., 1999; Davidson et al., 2002, Powers et al. 2010)”

p. 7699 line 4:

Pataki, D.E., Ehleringer, J.R., Flanagan, L.B., Yakir, D., Bowling, D.R., Still, C.J., Buchmann, N., Kaplan, J.O.,Berry, J.A.: The application and interpretation of Keeling plots in terrestrial carbon cycle research, Global Biogeochemical Cycles, 17, doi:10.1029/2001GB001850, 2003.

Qiu, G., Warland, J.S.: Inferring profiles of energy fluxes within a soybean canopy using Lagrangian analysis, Agric. For. Meteorol., 139, 119-137, 2006.

now reads:

Pataki, D.E., Ehleringer, J.R., Flanagan, L.B., Yakir, D., Bowling, D.R., Still, C.J., Buchmann, N., Kaplan, J.O.,Berry, J.A.: The application and interpretation of Keeling plots in terrestrial carbon cycle research, Global Biogeochemical Cycles, 17, doi:10.1029/2001GB001850, 2003.

Powers, H.H., Hunt, J.E., Hanson, D.T.,Mcdowell, N.G.: A dynamic soil chamber system coupled with a tunable diode laser for online measurements of $\delta^{13}C$, $\delta^{18}O$, and efflux rate of soil-respired CO_2, Rapid communications in mass spectrometry, 24, 243-253, 2010.

Qiu, G., Warland, J.S.: Inferring profiles of energy fluxes within a soybean canopy using Lagrangian analysis, Agric. For. Meteorol., 139, 119-137, 2006.

p. 7675 line 13: Consider changing ‘inside’ to ‘below’.

p. 7675 line 13: “inside”

now reads: “below”

p. 7676 line 13: ‘most predominant’ is redundant.

p. 7676 line 13: ‘most predominant’

now reads: “predominant”

line 16: comma after 2009 line 21:

p. 7676 line 16: “July 2009”

now reads: “July 2009,”

line 21 delete ‘the occurrence of’

p. 7676 line 21: “indicated the occurrence of loamy”

now reads: “indicate loamy”

line 22: ‘average soil carbon’

p. 7676 line 22: “The average carbon content”

now reads: “The averaged carbon content”

p. 7677 line 4: ‘tunable diode laser’

p. 7677 line 4: “tunable diode trace gas analyzer”

now reads: “tunable diode laser trace gas analyzer”

p. 7677 line 17: list the size and ID of the stainless tubing.

p. 7677 line 17: “stainless tube”

now reads: “stainless tube (0.43 cm I.D.)”

p. 7678 line 9: CMDL is now ESRL/GMD.

p. 7678 line 9: “(CMDL)”

now reads: “(ESRL/GMD)”

Please correct. line 12-14: Equal signs missing?

p. 7678 line 12:

“The calibration values for primary standard A were: CO_2 mixing ratio 360.62 μmol mol^{-1}, $\delta^{13}C$ -8.475‰ (± 0.003‰) and $\delta^{18}O$ -1.037‰ (± 0.006‰). Primary standard B consisted of CO_2 mixing ratio 545.77 μmol mol^{-1}, $\delta^{13}C$ -16.202‰ (± 0.002‰) and $\delta^{18}O$ -11.861‰ (± 0.007‰).”

now reads:

“The calibration values for primary standard A were: CO_2 mixing ratio = 360.62 μmol mol^{-1}, $\delta^{13}C$ = -8.475‰ (± 0.003‰) and $\delta^{18}O$ = -1.037‰ (± 0.006‰). Primary standard B consisted of CO_2 mixing ratio = 545.77 μmol mol^{-1}, $\delta^{13}C$ = -16.202‰ (± 0.002‰) and $\delta^{18}O$ = -11.861‰ (± 0.007‰).”

p. 7680 line 10: Make clear this goes into the IFR description immediately.

Corrected above.

p. 7681 line 11: 'The isotopic composition of the soil CO2 flux (d18OF and d13CF). . .'

p. 7681 line 11: "The isotope composition of CO_2"

now reads:"The isotope composition of CO_2 flux"

line 23: consider changing 'amplitude' to 'range' or 'span'.

p. 7681 line 23: "amplitude"

now reads: "range"

p. 7683 line 1: d18OF

p.7683 line 1: "Modeling $\delta^{18}O$ of net soil CO_2 flux"

now reads: "Modeling $\delta^{18}O_F$"

p.7683 line 12: 'in the soil via abiotic soil invasion. . .'

p.7683 line 12:

"atoms with liquid water in the soil"

now reads:

"atoms with liquid water in the soil via abiotic soil invasion"

line 19: CAE not defined.

CAE was defined in p. 7674 line 8

p. 7684 line 23: Max looks like 9:00 to me, not 7:00.

p. 7684 line 23: "at 07:00 h EST"

now reads: "at 08:30 h EST"

line 24: refill time 10-11am

p. 7708 Fig. 5 caption: "09:00 to 10:00 (EST)

now reads: "10:00 to 11:00 (EST)"

p. 7685 line 1: Entrainment of air enriched in 13C and 18O.

p. 7685 line 1: "the enrichment of the air in $^{13}CO_2$ and $C^{18}O^{16}O$"

now reads: "entrainment of air enriched in ^{13}C and ^{18}O"

line 2: CO2 depleted in the heavy isotopes

p. 7685 line 2: "buildup of relatively more depleted CO_2 originated"

now reads: "buildup of CO_2 depleted in the heavy isotopes originated"

p. 7686 line 10: Fig 6 does not show this. What fCA was used in these calculations?

p. 7686 line 8: "In this study the ratio between modeled F_{inv} and measured F_R, was calculated for half-hour periods in which $\delta^{18}O_F$ was obtained using the mKP approach (Fig. 6). The average F_{inv}/F_R for these periods was 0.13 and the maximum value for this ratio was 0.19."

now reads: "In this study the ratio between modeled F_{inv} and measured F_R, was calculated for half-hour periods shown in Fig. 6, in which $\delta^{18}O_F$ was compared with the mKP approach. The average F_{inv}/F_R for these periods was 0.13 and the maximum value for this ratio was 0.19, for $f_{CA} = 1$."

p. 7687 lines 1-5: Don't you mean WT instead of mKP here?

p. 7687 lines 4: "as observed by for the comparisons between mKP and IFR methods."

now reads:

"as observed previously for the comparisons between mKP and IFR methods."

p. 7689 line 8: Again, I see a decrease rather than an increase on those days. Day 220 is not shown in full. Otherwise, yes, higher f_{CA} is needed to improve the model agreement. A general comment here is that the possibility of diel cycle soil water d18O is not discussed as a possible cause of the variability in the d18OF. Changes in fCA may not be the only possible explanation.

please see comments 1 and 6.

p. 7691 line 7-8: Again, I just don't see it…

corrected above

line 10: '. . .soil water d18O sampling scheme. . .'

p.7691 line 10: "soil sampling scheme"

now reads:

"soil water sampling scheme"

p.7692 line 7: '. . .used to quantify sigma_w and TL. . .'

p. 7692 line 7: "used to σ_w and T_L"

now reads: "used to estimate σ_w and T_L"

p. 7693 line 14: please define 'r'.

defined in p. 7683 line 10

p. 7694 line 11: '. . . converted into the VPDB scale using eq B1.'

p. 7694 line 11: "converted into the VPDB scale"

now reads: "converted into the VPDB scale to be used in Eq. (B1)"

Table 1: In footnotes, these are mean values of multiple core samples right? Is there a standard deviation or error on the texture?

Table 1 – Textural composition and organic carbon content of the soil at a deciduous forest in Borden, ON, Canada.

Depth (cm)	**Texture (%)***			**C (%)+**
	Sand	**Silt**	**Clay**	
0-10	87.1 (±3.6)	8.2 (±2.6)	4.6 (±1.1)	2.40 (±0.03)
10-15	90.5 (±0.6)	6.5 (±0.8)	3.0 (±0.6)	0.66 (±0.03)
15-30	95.1 (±1.0)	3.3 (±1.3)	1.6 (±0.4)	0.63 (±0.02)
30-45	95.3 (±1.4)	3.3 (±1.3)	1.3 (±0.6)	0.53 (±0.02)
45-60	96.8 (±1.3)	2.4 (±1.1)	0.8 (±0.4)	0.52 (±0.02)

*soil texture analysis was obtained from three soil cores. Numbers in brackets are ± 1 standard deviation

+soil carbon content from nine soil cores.

Fig 2: I find it odd that the symbols are larger than the error bars. Consider using smaller symbols, or state that the error bars are smaller than the symbols.

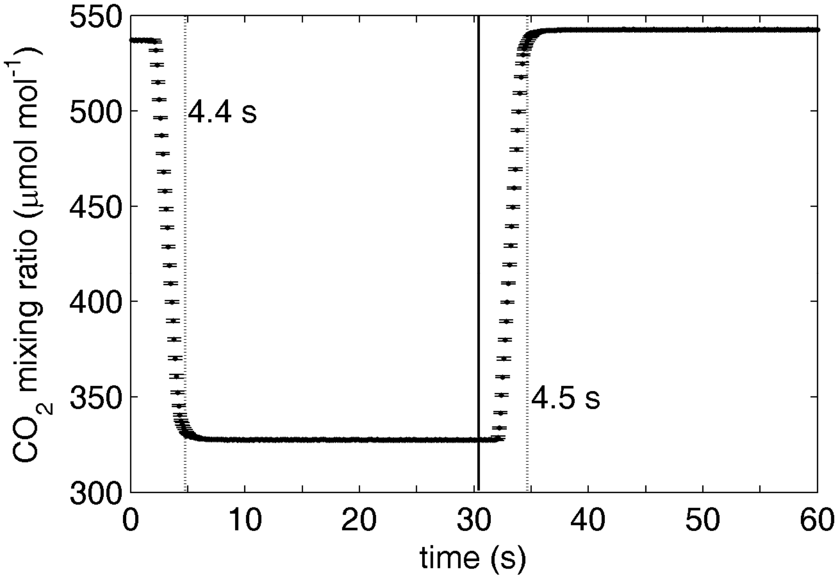

Fig 4: No gaps in FR due to data quality screening?

F_R was screened for quality control and periods excluded were due to equipment failure.

Fig 5: This figure would benefit from longer/major tick marks at the labeled hours so it's easier to determine the times in the middle and upper panels. A few minor ticks would be good in the upper panel CO2 axis.

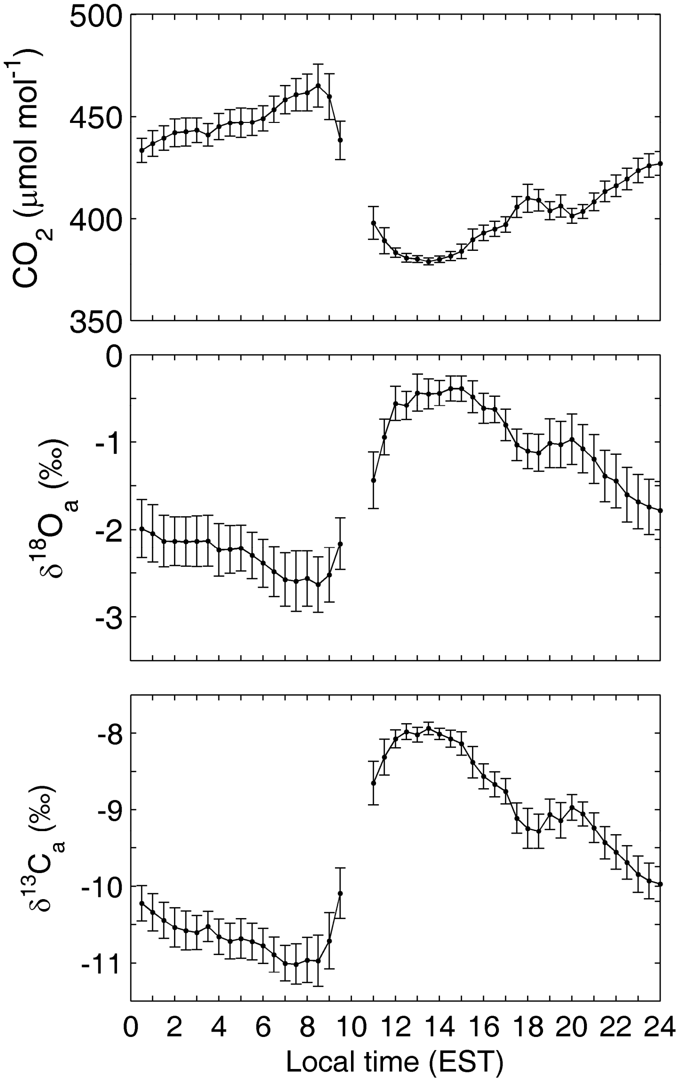

Fig 6 & 7: Pick consistent axis labels for the different d18OF products, e.g. IFR d18OF (permil), WT d18OF (permil) and mKP d18OF (permil). You might consider using both abbreviations and full descriptions (e.g. modified Keeling plot) in the figure captions.

Fig 6: There is no need to have the scale of the x- and y- axes of d13C match those of d18O. You can zoom in some more. Label y-axis as 'IFR'?

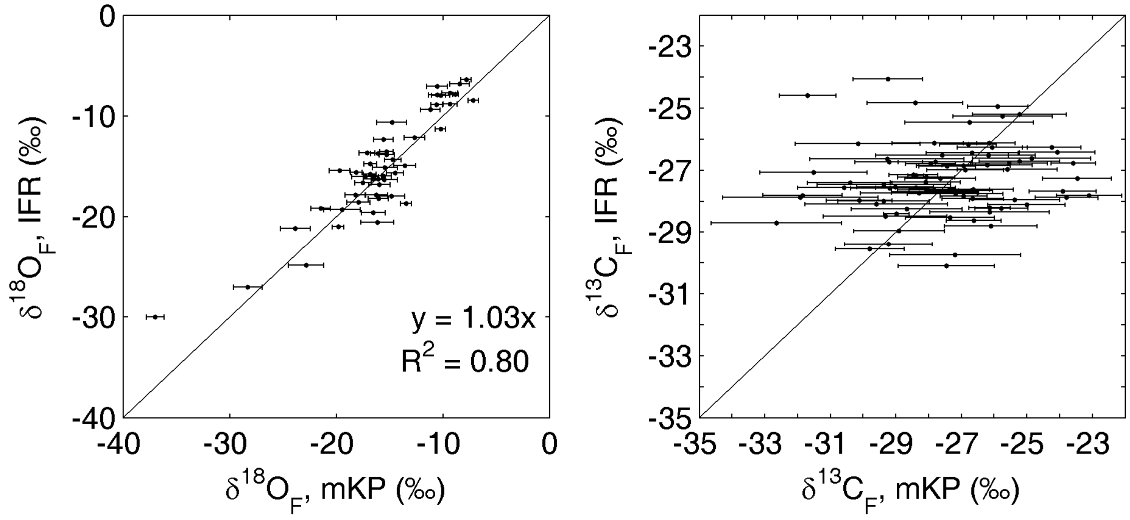

Figure 1 – Relationship between half-hourly isotopic ratios of the CO_2 flux near the forest floor ($\delta^{18}O_F$, $\delta^{13}C_F$) estimated using Keeling plot (mKP, ± standard error of intercept) and isotope flux ratio method (IFR) in the understory of a deciduous forest at Borden, ON, Canada.

Fig 7: y-axis label should be the same as in Fig 6.

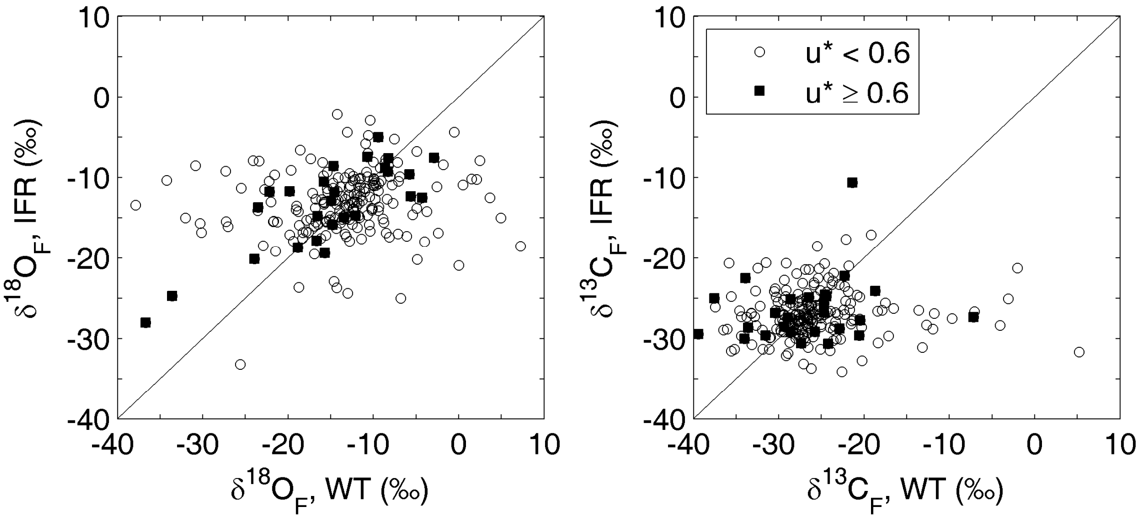

Figure 2 – Relationship between half-hourly isotopic ratios of the CO_2 flux near the forest floor ($\delta^{18}O_F$ and $\delta^{13}C_F$) estimated using a Lagrangian dispersion analysis (WT) and isotope flux ratio method (IFR) in the understory of a deciduous forest at Borden, ON, Canada. u* is the friction velocity (m s^{-1}).