Anonymous Referee #2, Reviewer Comment #1

[Note: Our replies to the anonymous referee are given in italic font.]

This paper describes a system for relatively easily made CO2 profile measurements consisting of a balloon and an instrument to measure CO2, and a budgeting approach to derive regional-scale biosphere-atmosphere exchange fluxes. The approach is not new, but the simplicity of the measurement system might open possibilities for involving citizen science groups to contribute measurements. The paper is well-written, and fits well within the scope of AMT. However, a few issues listed below should be addressed before the paper can be recommended for publication.

We appreciate the thoughtful comments from the second anonymous referee. We are happy that the referee sees the applicability of the approach for citizen science. We agree with the comments below, and give details on how addressing these comments will improve our manuscript. In particular, the references that will expand our Introduction section are very helpful.

General comments: The introduction should be modified to include more relevant literature, I give three examples below. 1) The use of satellite borne remotely sensed of atmospheric mole fraction column for inverse modelling of fluxes is only a recent development. The introduction should therefore also refer to the standard approach of inverse estimation of surface-atmosphere exchange fluxes using the combination of transport models and atmospheric mole fractions from measurements made at an atmospheric network of observing sites, see for example Enting et al. (1995), or Gurney et al. (2002). Enting, I. G., Trudinger, C. M. & Francey, R. J. A synthesis inversion of the concentration and d13C of atmospheric CO2. Tellus B 47, 35-52 (1995). Gurney, K. R., Law, R. M., Denning, A. S., Rayner, P. J., Baker, D., Bousquet, P., Bruhwiler, L., Chen, Y. H., Ciais, P. and Fan, S.: Towards robust regional estimates of CO2 sources and sinks using atmospheric transport models, Nature, 415(6872), 626–630, 2002.

Yes, great point. There is a much longer history of making inversion estimates of surface co2 fluxes from the observational network. In our revised manuscript, we will add these references near where we discuss the satellite technique.

2) Furthermore the introduction needs to refer to other budget studies based on profile measurements, a list is shown below. Wofsy, S. C., Harriss, R. C., and Kaplan, W. A.: Carbon dioxide in the atmosphere over the Amazon basin, Journal of Geophysical : : :, 1988. Denmead, O. T., Raupach, M. R., Dunin, F. X., Cleugh, H. A., Leuning, R.: Boundary layer budgets for regional estimates of scalar fluxes, Global Change Biology, 2, 255-264, 1996. Laubach, J. and Fritsch, H.: Convective boundary layer budgets derived from aircraft data, Agricultural and Forest Meteorology, 2002. Chou, W. W.: Net fluxes of CO 2in Amazonia derived from aircraft observations, J. Geophys. Res., 107(D22), 4614, doi:10.1029/2001JD001295, 2002.)

We agree that these aircraft-based studies are great references for our paper. In particular, the theoretical background in Denmead et al. and Laubach & Fritsch will be very useful in addressing some of the points raised below. We will both add the studies in the Introduction, and specifically refer to them in addressing both reviewers' comments.

3) In terms of the measurement technique, a reference to the recent AirCore approach would be appropriate: Karion, A., Sweeney, C., Tans, P. and Newberger, T.: AirCore: An Innovative Atmospheric Sampling System, J. Atmos. Oceanic Technol., 27(11), 1839–1853, doi:10.1175/2010JTECHA1448.1, 2010.

This is a very interesting technique and would be a great way to get citizen scientists involved. In addition to making a reference for the methodology, we will add a reference in our Conclusions section that notes combining the HAB approach with the AirCore.

The materials and methods section should include a more complete description of the measurements and the approach. How was the LI-640 instrument calibrated? Was the air dried before measurement of the mole fraction? If not, what is the expected impact of changing amounts of water vapour in the atmosphere between the two flights (see e.g. the Webb correction (Webb et al., 1980) in case of eddy covariance flux measurements)? Referece: Webb, E. K. and Pearman, G. I.: Correction of flux measurements for density effects due to heat and water vapour transfer, Q.J.R. Meteorol. Soc .,1980.

We will add the following information to our Materials and Methods section. The air was not dried, and so there is a potential error introduced due to the dilution effect of water vapor. The corresponding author (Potosnak) has employed the Webb (WPL) corrections for surface fluxes (Potosnak et al., Isoprene emissions from a tundra ecosystem, Biogeosciences, 10, 1–19, 2013) and understands that this can be important for certain surface exchange situations. This relates to an improved discussion of our certainties, which is discussed further below. As noted in the text, we attempted to use the LI-840 instrument, which simultaneously measures water vapor, but we ran into instrumentation issues.

The instruments were calibrated with a zero and a two-point calibration (that is, two non-zero co2 concentrations). After the instrument was zeroed, the higher calibration standard was used to set the span, and then the calibration was checked with lower calibration standard. Standards were obtained from the Chicago AirGas facility. We will add this information to the manuscript.

An assessment of the uncertainty of the approach should be given. This includes the uncertainty of the CO_2 profile measurements, such that error bars can be shown both in the CO_2 profiles and in the NEE contribution profiles (Fig. 4). It also includes a discussion of the uncertainties introduced by the assumptions made in the budgeting approach (neglecting advection).

Yes, we agree an error analysis is necessary. We will add standard error bars calculated from the multiple co2 concentration measurements from each 100m bin. We will also add error bars for the NEE contribution profiles, assuming that the measurement errors (instrument uncertainty + real variability) are uncorrelated. We have done some previous analysis, and this source of error is relatively small. We will formally compute this error for each flight.

Using guidance from some of the literature sources above, we will make some estimates of the error due to advection. While this will be more qualitative, it will give some guidance. We will also discuss further our future intent to do multiple launches along an upwind/downwind transect, which will allow an experimental investigation of the advection issue.

As mentioned above, another source of error that will be investigated is the dilution effect of water vapor. Using the data from the 14 Aug 2014 flight that has a clear increase in the boundary layer height, we will investigate how the surface flux of water vapor (evapotranspiration) would dilute the co2 concentrations measured during the second flight. Using the observed boundary layer (average between the two flights) and literature values for evaporation, we will calculate the impact on the molar density of moist air, and assess how much error is introduced by our assumption that the density is constant. (In reference to Equation 1, this is the assumption that rho_i is constant between the two flights).

Specific comments Figure 1: The units on the x-axis of the right panel seem wrong. The shown contributions to NEE from the different height intervals should have units of flux per height interval, or something like micro-moles/m2/s/km, such that when vertically integrated the units are those of NEE.

In response to comments from the other anonymous referee, we will make some changes to this figure, including the units. Looking at Equation 1, what is plotted in the figures is derived from that equation, but there are some steps that we did not make explicit. Because the summation is linear, the concentrations from the two flights (C2 and C1) are first multiplied by the molar density (rho) and divided by the time (Delta T) to give umol co2/m3/sec. This is the average over the 100m interval. For the plot, we then multiplied each of these terms by 100 m, which is the vertical integration interval. This was done BEFORE the summation, which we neglected to mention explicitly. Because the summation is linear, this is valid but confusing. The advantage is that the plotted values can be directly summed to get their contribution to surface NEE.

In the revised figure, we will instead plot umol co2/m3/sec, and not multiply by the vertical integration interval of 100 m. The figure will be unchanged in shape, but the values will be a factor of 100 smaller. Also, we will change the NEE in the x-axis label. While the summed values are NEE, individual 100m intervals could contribute to either surface exchange or vertical mixing. We acknowledged this possibility in the discussion of the 13 Aug 2015 flight, but changing the units and the x-axis label will aid in interpretation. The new x-axis label will be "Molar difference."

Line 204: The link to ameriflux does not work

We apologize. After many years of stability, these links have changed. To avoid future confusion, we will simply remove the link and give the site ID instead (US-Bo1).

Line 224-225: this could also (and more likely) be the residual layer, i.e. the previous days mixed layer, combined with a change in advection. I guess that is what is mentioned in the next sentence (lines 225-227)

Yes, we will change the order of the sentences to give this possibility first, since we agree it's more likely. We will also explicitly mention the residual layer.

Line 281: it should be assessed if at least the wind direction or changes in wind direction between the first and second flight of 23 July 2015 are pointing to a contamination. This should also be done for other flights to exclude potentially contaminated profiles, since otherwise only the data that look strange are checked for this.

We will use NOAA's HYSPLIT model to check for changes in wind direction between the two flights.

Line 319: In terms of future applications, the authors might want to include the use of these profile data in regional scale inverse modelling, which would allow for taking advective contributions into account.

This is a great idea, and we will mention this application.