

Anonymous Referee #1, Reviewer Comment #2

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

We appreciate the thoughtful comments from the first anonymous referee. The reviewer points out some methodological concerns about the way we interpret our data vertically, and addressing those concerns makes our manuscript much stronger. Also, the reviewer notes a number of details that we need to add to our paper. Finally, we are happy to the reviewer sees this technique as appropriate for engaging the public.

This paper presents an approach based on easily accessible 'weather' balloon launches with lightweight CO₂ sensors to determine surface fluxes of CO₂. It is appropriate for AMT, and has particular relevance to engaging the public in making CO₂ flux measurements in appropriate regions. However, it needs some significant revisions before publication. Most notably, the computational approach that has been used to compute fluxes for each 100 m layer of atmosphere is not appropriate. The profiles should be integrated first and then the flux is computed from the difference. While it may work to compute the differences first and then integrate this can lead to spurious values due to how vertical mixing plays out between the two flights. The layer by layer values are not interpretable. Additional clarification is needed on details of the measurement methods.

We appreciate the standard mixed layer approach is slightly different from the approach we take in this manuscript. We also address this in our responses below. We also understand that our presentation in Figures 1 and 4 could have been more clear. In particular, giving the units as $\mu\text{mol}/\text{m}^2/\text{sec}$ and the x-axis as NEE was not helpful. But we still think there is value in presenting the differences in 100m bins. Looking at Equation 1, the summation is linear, so mathematically there is no difference between differencing first or summing (integrating) first. We like the vertically resolved presentation, given that one target for our technique is citizen scientists. The vertically resolved differences do elucidate boundary layer processes such as the well mixed layer and changes in boundary layer height.

The reviewer correctly points out that some of the differences will be due to vertical mixing and not surface exchange. This is correct, and again our use of the x-axis label "NEE" was not correct. Only the sum of the differences is NEE, under our assumption of no horizontal advection. We will also change the units. Below is more detail, copied from our reply to the second anonymous reviewer:

"In response to comments from the other anonymous referee [referee #1], 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 (C_2 and C_1) are first multiplied by the molar density (ρ) and divided by the time (ΔT) to give $\mu\text{mol co}_2/\text{m}^3/\text{sec}$. 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 $\mu\text{mol co}_2/\text{m}^3/\text{sec}$, 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."

Specific points to address are itemized here.

Page: 4 Line 105, here and throughout it would be more appropriate to refer to mixing ratio instead of concentration. The instrument is measuring CO₂ mixing ratio in micromole CO₂ per mole-of-air

Yes, this is a good point—it is more correct to say mixing ratio than concentration for our measurement. We will make this change throughout the manuscript. (See also our reply to the next comment.)

Page: 5 Starting at line 151 You need to be specific about what the Licor measures. By specifying the unit as ppmv you imply that it is mole of CO₂ per mole of air. Is it dry air or ambient air with H₂O included? However, the instrument signal is actually dependent on density - number of CO₂ molecules inside the cell and has to be converted to mole fraction by pressure correction internal to the analyzer. Is it pressure corrected? You need to explicitly discuss these details.

We will more fully discuss in the Materials and Methods section the following details. Also, see our response to the second anonymous referee about not drying the air. Yes, the instrument is an IRGA, so the initial measurement is absorption of CO₂ and is $\mu\text{mol CO}_2/\text{m}^3$. The ideal gas law is used to find the molar density of air ($\text{mol air}/\text{m}^3$) and convert to ppmv. This is done using the temperature of the cell (controlled to 50 deg C) and the measured pressure near the cell. The instrument does this calculation, and we will include that information in the text. As we discussed in our response to the second anonymous reviewer, we did not dry the air. This does introduce a source of error that we will assess in our revised manuscript.

Page: 6 line 180; In the discussion about temperature I assume you are referring to the potential temperature, which would be the temperature if a parcel of air were brought to the pressure of reference height (surface). That's probably not far off, but you need to include some discussion of how much the actual temperature does deviate from the what you get from assuming constant potential temperature. The previous discussion about presence of mixed layer and noting that the mixed layer height increases over the day implies a deviation from constant potential temperature with height.

We will explain this step more fully in our revised manuscript, since it relates to the citizen scientist nature of our work. Temperature is necessary to compute the density of air (ρ_{air} in Equation 1). Ideally, we would have measured in situ air temperature using a fine-wire thermocouple, but due to the simple nature of our equipment, this was not done. As a proxy, we use a form of the integrated hydrostatic equation that assumes temperature (not potential temperature) does not vary with height. We then plot log pressure versus altitude, and the slope (with some conversion from the scale height) is the temperature. The fits obtained by approximating that temperature is constant are very good. While we could use a constant potential temperature, we feel the exercise of obtaining the average temperature with this methodology will help citizen scientists understand the integrated hydrostatic equation.

Line 190 It would be better to show the steps for equation 1 starting from the mass balance that relates the changes in the integral of CO₂ density to the sum of in and outgoing terms; flux at bottom, entrainment at top, difference in advection in and out. Then state that for cases where entrainment is small ($dC/dz \approx 0$ at top of volume) and advection is small (dC/dx is small) you can get the surface flux

from the change in CO₂ mass inside your volume. The equation should be applied to the integral NOT to individual layers because there is vertical mixing

As mentioned above, we understand that our presentation of the vertical differences could be more clear. Yes, some of the layer differences will represent vertical mixing, but we would like to see that influence. More broadly, our mass-balance approach does extend past the top of the mixed layer, so it differs from some previous approaches (for example, the helpful references given by the second anonymous referee, in addition to what we cited in our text). We do need to assume that entrainment at the top (dc/dz) is small, and we will explicitly add that into our text. Since our top is in the free troposphere, that is a very reasonable assumption. Yes, advection is an issue. As discussed in our reply to the second anonymous referee, we will expand our discussion of advection and assess the potential errors introduced by the assumption of no horizontal advection.

Page: 7 line207: You need to explain why it is necessary to generate a composite C₂ seasonal cycle of the eddy flux data rather than use the data as is for each year.

We will add text to indicate that using the composite over time is useful, since that looking at the variability in one place (the flux site) can be used as a proxy for looking at the variability in the spatial dimension.

line 235: Usually the layer above the mixed layer is referred to as free troposphere.

We will be sure to consistently use free troposphere, not free atmosphere.

Page: 8 Line 239 Clarify what you mean by the pressure values. Are you adjusting the partial pressure of CO₂ or the atmospheric pressure by 1%? I haven't seen any place where CO₂ partial pressure is actually used, so to mention it here causes confusion. Secondly, it would be more convincing to have some explanation of why the measurements shifted by 4 ppm.

We believe that there was an error with the Licor LI-840's internal pressure sensor. Since the instrument measures absolute co₂ concentration as noted above, the internal pressure sensor is used to compute the mixing ratio. So, changing the pressure changes the mixing ratio. The pressure change of 1% equates to a roughly 4ppm mixing ratio change. We don't have a good explanation for this, except that free troposphere co₂ mixing ratios match after this adjustment.

line 253; Revisit the discussion about anomalously low NEE for 7/23/15 after recomputing using the difference of the integral and not doing the differences layer by layer. Then if you still think this point is anomalous, show the profile. Even if there is enhanced CO₂ in some layer the calculation still gives an estimate of surface flux, but it won't be NEE if the source of CO₂ is some combustion or anthropogenic source. It would still be NEE if the high CO₂ was from an unplanted field. Are there any large herds of livestock around? If that profile is judged to be anomalous it shouldn't be included at all, just say that there were more flights, but profiles with local influence were rejected.

Since the differencing and summation (integration) are both linear, the order itself should not change the result. We will check this upon revising our manuscript, to ensure there are no differences due to computation. We did use different vertical bin sizes, and found this had only a small effect on the summed NEE. We understand that NEE strictly refers to ecosystem exchange, and we'll remove this point from the figure that makes the comparison to the Bondville data.

From our casual observations during our flights, there are very few unplanted fields during the summer and the vast majority of the land area is in a corn-soy rotation. There are some livestock operations, but we are not aware of any large feed lots or other larger-scale livestock concentrations.

line 263 With an unexplained instrument malfunction it might be more prudent to discard that data.

We realize it is less than ideal to include these data, but we think discussing them is illustrative since it highlights the utility of using free-troposphere concentrations as a check. Also, we are not using these questionable data in an annual summation, and this is an initial presentation of the methodology.

line 265: the NDVI trend is for a 16-day average while the NEE estimates are a snapshot for part of a single day. There are many reasons for CO₂ flux on a particular day to vary from the smoothed area average. Different flux footprints and hence mix of crop type or presence of bare fields on different days or clouds are just two causes that come to mind. Trying to interpret variations between the two years based on just a few observations in each year is too speculative and ought to be left out of the discussion.

We will reduce our mention of these results in the discussion and highlight their speculative nature.

Page: 9 line 277; averaging the whole annual NEE into 100 days of 12 hours ignores too much of the sharp seasonal pattern for crops. Comparing to the instantaneous flux values from Bondville tower is fine, but not averaging the annual NEE.

We were attempting to use a back of the envelope calculation to bring in an independent source of data, but we agree this leaves out a lot of detail and we will remove this comparison.

line 281; if you are going to include this data point but still call it anomalous you have to show its profile so the readers can make their own judgment of what is wrong with it. It isn't OK to talk about it and just claim that it has local source contamination.

Yes, we will include this profile as a separate figure.

Page: 13 This figure may not really be necessary. I don't think it adds very much to understanding the method. But if you keep it, I think it is not appropriate to compute the flux separately for each layer. As noted in comment about equation 1, there is vertical mixing so the differences between flight 1 and 2 at any specific layer are not simply related to the fluxes during the intervening time. The profiles must be integrated first and then take the difference to get the flux. I'm also not convinced that the ideal profile in lowest km would be constant. There is strong enhancement in early morning from the residual of respiration into the nocturnal layer and some change in concentration with height because mixing is not instantaneous so concentrations at higher position lag behind the concentration at the surface.

We would like to keep this figure, because one of our targets for this methodology is citizen scientists. They will not have a good a priori understanding of boundary layer dynamics. We want to show an 'idealized' pattern that demonstrates two important boundary layer processes: a change in the mixed layer concentration and an increase in the boundary layer height. We completely agree there are other potential processes, including the decay of the residual layer and changes in concentration with height in

the mixed layer. We will change the text of the figure caption to indicate that this idealized pattern only represents these two processes, and is not an 'expected' profile.

For the order of differencing and summation, see our comments above.

Page: 16 Figure 4 should not show flux per layer. Integrate the profile and take difference

Again, see our comments above on this point.