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Atmospheric Measurement Techniques Discussions

Interactive comment on "The NASA Carbon Airborne Flux Experiment (CARAFE): Instrumentation and Methodology" *by* Glenn M. Wolfe et al.

Anonymous Referee #2

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Summary/General comments: Wolfe et al. describe the CARAFE aircraft, payload, and measurement methodology including flight data from campaigns in 2016 and 2017. Much of the manuscript focuses on the airborne eddy covariance method, how it is applied, and uncertainty analysis. The manuscript is well written and well placed in AMT. The authors have done a commendable job attempting to investigate the many challenges and sources of uncertainty in performing airborne eddy covariance. I do have some reservations and questions that need to be addressed. Once appropriate changes are made I would recommend publication.

Presentation/conceptual concern: The manuscript presents the CARAFE payload and

eddy covariance technique as a useful new tool for improving our understanding of carbon gas exchange. This tone underlies much of the manuscript, but the authors fall short of actually justifying, and this should be rectified. There is a cursory review of other airborne approaches that misses many techniques (such as the mass balance method), and the relative strengths/weaknesses are not really clearly highlighted. This isn't a problem if the manuscript focused on the CARAFE payload, but this would need to be addressed to assert the added value of airborne EC for CO2 & CH4. Even more so, the authors don't actually link observed eddy covariance to surface fluxes and provide added science valueâĂŤit is made clear it is not known how to best link to horizontal spatial flux scales on the surface. More so, the error analysis suggests flux errors when considering the surface that can easily exceed 100%. I finished the manuscript wondering whether this approach was a wise usage of the aircraft and payload. Making high accuracy GHG airborne measurements from aircraft can be used with mass balance and different inversion systems to quantify fluxes with errors of $\sim$20%. With such an approach, larger areas can be covered with the aircraft as repeat legs are not needed and there are far less stringent requirements on level flight and surface characteristics, enabling the usage of far more data. Further, problems like the 2016 wind measurement error reported render all those flights of no scientific value because of the stringent requirements for EC. I commend the authors for their efforts and rigorous analysis, but at this point they cannot assert that airborne EC for CO2 & CH4 as presented in the manuscript provides added science value over more conventional accurate airborne sampling. I would actually think given the gaps in linking to horizontal surface domains, the tight restrictions on where the approach is useful, the limits imposed on flight area coverage, and the high fractional uncertainty, it is worth questioning if for surface carbon exchange this technique will add to addressing current science questions or whether accurate flight measurements for usage in inversions and mass balance approaches would be more scientifically fruitful. My suggestion is that the authors make changes in the abstract, introduction, and conclusion to more accurately capture this reality. The emphasis should be on the presentation of the

CARAFE payload. The extensive discussion of EC and uncertainty should remain, but a clear discussion of the limitations and that added science value is yet to be shown should be made clear.

Detailed comments:

Page 1 Line 16: not accurate as stated – exchange between surface and atmosphere only drives atmospheric abundance of some gases – not all atmospheric composition.

Page 1 Line 17: should modify to "potentially helping". Also, what are you defining as regional? Need spatial scale. Traditional airborne measurements can cover similar scales so would need to be specific and distinguish.

Page 1 Line 25-26: It does not follow from the paper that this system will further our understanding of ecosystem exchange – this has not been established.

Page 2 Line 3: the Dlugokencky reference is a very incomplete citation for such a broad statement.

Page 2 Line 4: The above described global approaches can also be defined as top-down and bottom-up. Need more specificity referencing spatiotemporal scales.

Page 2 Line 4-14: This is a very cursory coverage of other approaches that does not address many airborne approaches (mass balance, point source circling, eulerian/lagrangian inversion) that have been well established to evaluate fluxes at 10-100 km scales. NB those approaches are more flexible than EC and can deal with point sources that can be important for CO2 (power plants) and CH4 (lots of point sources). Addressing point sources is important for Carbon gases, and EC is ill-equipped for this. This point needs to be addressed.

Page 2, line 15: EC does not directly quantify surface-atmosphere exchange – it quantifies exchange between two atmospheric levels. An important distinction, as surface exchagne is inferred, which large errors induced due to flux divergence.

Page 2 line 24-26: As stated above, other airborne approaches are more flexible and have similar spatial capabilities.

Page 3 lines 1-2: This strong statement needs citation support.

Page 3 lines 28-31: This is illustrative of the very limited spatial domain that can be covered.

Section 2.2: I need to see more validation of winds. We should see the results from box patterns and other maneuvers done to test/validate winds, and thus be able to determine accuracy.

Section 2.2: The problem with the 2016 data is buried here. Based on this large, systematic problem, the authors decide not to use 2016 data. The authors should follow through and only show 2017 data (there is 2016 data still in many places). Further, this point should be made up front in the manuscript – small mistakes led to wind problems that rendered a whole deployment not useful for EC. This is illustrative of a major weakness to the CARAFE approach.

Page 5 line 4: Not sure where this comes from. I'd like to see more on this.

Section 2.4: I need to see more on the in-flight performance of the GHG analyzers. What is the accuracy in flight? Can the authors show the LGR analyzers show no vertical dependency compared with the Picarro?

Line 11: pressure fluctuations may impact accuracy however.

Page 6 line 5 & Figure 2: I am unclear on this linear transforming one instrument to the other. More clarity is needed. I also am concerned that this may not be appropriate for CO2 and CH4. When I look at figure 2 I get greatly concerned as the variation from flight to flight is actually very significant for gases that we care about fractions of a ppm (ppb for CH4). I also worry about inflight variations. We need more information on the validation of the GHG obs.

Section 2.5 (and figure 3): This section is somewhat of an aside. There is not other usage or discussion of this system.

Page 7 line8-9: This +-20 m requirement is very tight (as needed)- and will restrict the ability to use this data. Also, it should be made clear this is above ground and not asl. This makes it even harder to meet this requirement over terrain with any variability exceeding 20m. Further the 5 degree restriction is tough, but might it need to be tighter?

Page 7 Line 22: The problem is undersampling of the PBL depth can lead to systematic biases, and this is a major problem.

Page 11 Line 19: I'm confused, I had thought the authors earlier asserted 10Hz wasn't necessary, but here is seems this is a important error term.

Page 14 line 18: This is disappointing. If all these flight hours are being used there should be more planning for multiple altitude legs for this type of validation.

Page 15 lines 7-8: And these large uncertainties are a major problem for the approach. With relative uncertainties that push to 100% the utility of the technique is degraded.

Page 16 line 15-16: This is fair, but it means this manuscript has not established the utility of this approach.

Conclusions: I take issue with much of how the conclusion is written (see major comment above). This would be better served to summarize the aircraft system and payload, and then highlight the challenges in the EC approach and the resulting expected uncertainties.

Page 17 Lines 29-30: It has not been established that this approach should be a part of a standard toolbox.

Page 18 Lines 7-8: This isn't a new vector – as stated by authors the approach is old, and has been applied to Carbon before. The utility has never really been established,

particularly given the limitations, and that is why it has hardly been adopted.

Page 18 Line 17: This does not clearly follow.

Table 1: Methane and CO2 should be shown in ppb and ppm respectively (not fractional uncertainty).

Figure 1: The authors should indicate which flight legs are actually of use for EC on this plot – showing all the flight legs is misleading. 2016 data was deemed not useable, so should not be shown.

Figure 2: These are worryingly large to me. Also, this should show 2017 data as the authors don't use the 2016 flights.

Figure 7: This plot is sobering, and the log scale relative error brings into question the utility for CO2 and CH4.

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