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Interactive comment on "Quantifying wind and pressure effects on trace gas fluxes across the soil–atmosphere interface" *by* K. R. Redeker et al.

Anonymous Referee #2

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Review of MS bg-2014-652: Quantifying wind and pressure effects on trace gas fluxes across the soil-atmosphere interface. K.R. Redeker et al.

There are a few areas in the manuscript that I have identified as needing revision. Although there are only four areas that I have identified as needing attention, they are rather significant and require the authors to rework the manuscript somewhat extensively to convince the reader that their system is new, novel, and really measures what they state it will measure.

Areas of revision include the following:

1) Introduction: The authors make mention of static chamber measurements of trace gasses and air flow within these chambers without a complete review/reference of static

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chamber studies. There needs to be more discussion, because as this section is currently written it is incomplete. The authors are stating that quantifying trace gasses using static chamber measurements might be prone to errors that their data will help resolve so there needs to be a concrete reason why static chambers are prone to errors and why this "new" approach is an improvement. The entire manuscript hinges on the authors making a strong case for their method in the introduction.

2) Page 4, lines 91-95: The authors mention that the majority of studies only measure one trace gas at a time and that this reduces the ability to generate "broadly" applicable rules for surface-atmosphere trace gas fluxes. However, in the next sentence the authors state that they will "close this knowledge gap" by measuring two trace gases. The question arises, is measuring two trace gases really a significant increase (one trace gas vs 2 trace gases) to significantly close the knowledge gap? Possibly the authors have overstated things, if not, a thorough explanation of why two gases would be a significant improvement over other studies that only measured one trace gas is needed.

3) I'm a bit concerned about only "dark" condition data being presented. Would the results be different if light conditions were included in the results? In terms of CO2 fluxes, especially since they are bidirectional, if an ecosystem is only respiring/releasing CO2 (dark) vs. when there is CO2 release and uptake simultaneously (light) how would this influence the results of this study? A thorough discussion in the manuscript is needed to clarify this point.

4) What are the impacts of the wind tunnel(s) covering the ecosystem(s) for such a long period of time? Figure 1 gives the impression that the conditions are very artificial and not as natural as stated in earlier parts of the manuscript. There is some discussion of this, but it needs to be more in depth in order to convince the reader that the tunnels/chambers did not influence the results. This is a critical point since a proper system is needed to collect quality data. As the manuscript is written now, it leaves the reader a bit doubtful about the method that is being used. Therefore, a very de-

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tailed and concise explanation about the wind tunnels/chambers impact on the data is needed.

Interactive comment on Biogeosciences Discuss., 12, 4801, 2015.