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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.

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Response to referees’ reports on Redeker et al. ’Quantifying wind and pressure effects on trace gas fluxes across the soil–atmosphere interface’.

Below we respond to each referee in turn.

Referee #1. We were disappointed with the comments made by this referee. Some comments are ill-natured and some border on the petty and niggling. We feel that the many of the comments lack foundation and we provide a robust response below.

1. Line 3 on C1707: "few measurements at four sites". Response: This comment seems to imply that our study is, in some way, inconsequential. Multiple measurements under a range of wind conditions (n = 119 in total, as shown in Tables 2 and 3) were

taken across the four separate sites and the sites were chosen to represent a range of land covers and soil types. While we agree that more sites and more wind states could be looked at, we believe that our results cover a sufficiently wide range of environments and wind conditions to allow us to make general conclusions about the importance of wind as a factor in affecting soil-atmosphere trace gas exchanges.

2. Line 7 on C1707: "which we have known for a long time". Response: This comment implies that there is a large literature quantifying the effects of wind on soil-atmosphere gas exchanges. However, this implicit assertion is disingenuous because there simply IS NOT A large and SYSTEMATIC LITERATURE on the effects of wind on soil-atmosphere fluxes. What does exist is a literature on chamber artefacts, focusing primarily on the role of circulatory fans (or their absence) on measurements of soil-atmosphere flux. We briefly review the chamber literature on line 16 on page 4803 and line 14 on page 4804 of the paper. However, the extant fan/chamber artefact literature cannot be used to extrapolate to the effects of natural winds on soil-atmosphere exchange, because of the conditions under which these methodological studies have been conducted. It is also important to note that our paper is not just about the relationship between soil-atmosphere gas fluxes and wind speed, but about how spatial variations in wind speed affect gas fluxes. The apparatus we used enabled us to manipulate wind speeds not only in the immediate “footprint” of the inner, toroidal wind tunnel, but also in the region surrounding the toroid, enabling us to make inferences about the role of lateral flows in soil gas in the surrounding environment. To our knowledge, ours is the first study that explores how realistic spatial variations in wind speed, across a representative range of common soil types, affects gas fluxes

3. Line 12 on C1707: Boundary layer. “This “new” concept has been known to the earth system science community for decades.” Response: This is a remarkable comment because it indicates that the referee has failed to grasp some of the core concepts we are exploring in this study, and which we explicitly outline in the manuscript. Of course, we know that boundary layers develop in plant canopies and above the soil surface;

eddy covariance theory is predicated on this idea. However, what we are proposing here is that the boundary layer concept can be extended to incorporate porous media (i.e. soil), and gas exchange across the soil-atmosphere interface. This is in fact a new concept because existing soil flux chamber methodology assumes that diffusion is the dominant mode of transport across the soil-atmosphere interface (Livingston and Hutchinson, 1995), and many current approaches for measuring soil-atmosphere flux explicitly seek to minimize or prevent non-diffusive transport processes from operating (Pumpanen et al., 2004). The clue is in our original text: we say a boundary layer develops "at the near surface IN soils" NOT "above the soil"; we believe we are very clear here.

4. Line 3 on C1708: Massman's model already very comprehensive and no link provided to the model. Response: Massman's model is based on pressure fluctuations in (primarily) snow pack and, to our knowledge, has not been applied as a predictive tool for individual wind states across each of the various ecosystems that we explore in this paper. Neither has it published any predictions about the influence of local variations in wind state on temporal and site-specific fluxes. THIS IS NOT TO SAY THAT IT IS NOT A USEFUL MODEL, but it does not lend itself to easy application. Where our paper is significantly different from Massman's papers is that it is primarily an empirical study that did not consider at any time pressure fluctuations (we measured pressure differential between two points, not how the pressure differential between those points varied over time), a primary component of much of Massman's recent work on the topic. We show, contrary to Massman, that pressure fluctuations (indeed pressure overall) is not nearly as useful a predictor as the combined effect of local winds. Having explained why we feel that we provide a useful comparator/companion piece of research to the Massman work we do acknowledge that, through an oversight, we have not referenced Massman, 2006 and/or Massman and Frank, 2006. This will be added to the discussion here and to the references.

5. Line 12 on C1708: Wind versus pressure. Response: We appreciate that wind and

pressure are not entirely separate entities. However, pressure gradients in and around a soil profile are not easily measured (and are affected by other spatiotemporally variable environmental parameters as well, such as temperature, local pore water content, soil porosity, etc. . .) and pressure controls on the direction of the dependency between the two can vary at local scales. For example, pressures within the soil may not reflect the pressure changes above them, as described in Xu et al., 2006 and considered and discussed within the paper at length in section 4.1 on the pages 4816 and 4817. The entire purpose of our experimental apparatus was that it allowed us to investigate wind and pressure differential separately, by increasing the wind of the interior AND exterior of the toroid we could maintain a similar relative pressure between the interior and exterior of the wind tunnel.

6. Line 18 on C1708: A more general model would include isotopic signatures, biological production and consumption reactions. Response: We intentionally did not generate a set of specific mathematical equations for our "conceptual model" (Lines 21-23, page 4802- "We propose a CONCEPTUAL MODEL of the soil profile that has a "mixed layer", with fluxes controlled by wind speed, wind duration, porosity, water table, and gas production and consumption."). This model, by its very nature, is generalized and with further empirical measurements isotopic effects can be incorporated. At this time we report only that the mixed layer develops within the context of several important environmental parameters (as supported by our data and text, specifically figure 5, and described in lines 4-17 on page 4818) and that any mathematical approach that does not take this process into account is unlikely to accurately reflect surface fluxes of trace gases. The reviewer may have missed our description of the model on page 4818 where we specifically state that consumption and production processes within the soil profile would affect the depth and duration of the developed "mixed layer".

7. Line 22 on C1708: Different types of diffusion. Response: The referee makes a rather semantic point here. As soon as a modifier such as 'Knudsen' is used we think it DOES suggest a different version or type of the process. Our use of the term

"ordinary" comes from Rolston and Moldrup (2012) and we are happy to make that use more explicit in a revision of the paper. We believe the referee is incorrect in defining diffusion as necessarily requiring an energy gradient. A number of different processes can be described by the diffusion equation, as we are sure the referee knows, but that does not mean that they are the same physically. Heat conduction in a solid requires a thermal gradient and is accurately described by the diffusion equation but involves only energy transfer. On the other hand, the isothermal mixing of two gases can also be described by the diffusion equation but is a different process involving the net movement of gas molecules.

8. Line 5 on C1709: "there is more than half a century of literature establishing that diffusion is the dominant form of transport in soils – this sentence is misleading". Response: We disagree that what we are saying is misleading. What we have written is: "Even if we assume that diffusive fluxes are an important form of gaseous movement in soils, such fluxes are highly sensitive to gradients in local soil gas concentrations." Nowhere do we imply that diffusion is not an important process; rather, what we are asserting is that other transport mechanisms (such as the effect of bulk flow processes, like wind movement) need to be more fully accounted for in soil-atmosphere flux measurements.

A large number of chamber studies have been undertaken where it is assumed that diffusion is the dominant form of gas transport in soils, but there are few studies that have investigated the effects of wind on gas transport. Several studies exist on the effects of pressure pumping, upward-driven advection due to the development of pressure gradients in soil, or plant-facilitated gas transport (Chanton, 2005, Tokida et al., 2005, Tokida et al., 2007, De Visscher et al., 2004), but none – as far as we are aware – have systematically tested for the effects of wind movement at the soil-atmosphere interface. By referring to the last 50 years the referee implies there is an extensive literature that we are ignoring or have misunderstood; we invite the referee to provide evidence of that extensive literature.

In fact, contrary to what the referee has asserted here (i.e. that diffusion is the dominant mode of gas transport in soil), we would argue that the soil gas transport and soil-atmosphere gas flux literature in fact highlights the importance of advective processes and how little we understand their role in soil-atmosphere exchange. For example, earlier in their review, the referee lauds the work of Massman (2006), but it is notable that Bowling and Massman (2011) (Journal of Geophysical Research Biogeosciences doi:10.1029/2011JG001722), conclude their paper on the importance of advection in gas exchange in snowpacks with the following [CAPS added]: "Pressure pumping was a persistent feature of transport in this mountain forest seasonal snowpack, causing short term enhancement up to 40% higher than molecular diffusion, but 11% over a winter. IT IS HIGHLY LIKELY THAT PRESSURE PUMPING INFLUENCES SOIL GAS TRANSPORT IN GENERAL, AND FUTURE RESEARCH SHOULD INVESTIGATE THE IMPORTANCE OF THIS PROCESS IN SOILS." Likewise, work in wetland systems (including flooded agricultural systems like rice) (Chanton, 2005, Glaser et al., 2004, Tokida et al., 2005, Tokida et al., 2007), tropical rainforests (Teh et al., 2005) and landfills (Czepiel et al., 2003, De Visscher et al., 2004) have highlighted situations where advection may be a dominant or co-dominant transport process in soil. Clearly, the importance of advection is on the research agenda, and, while it might be a secondary or short-lived process in some soils (depending on wind climate and soil type) it could be as important or more important than diffusion in others. Therefore, we feel our study is timely and provides a means of establishing the importance of advection. It is notable too that the short-term enhancement in soil-atmosphere gas exchanges that we observed is broadly consistent with the 40% estimated by Bowling and Massman (2011); our data show enhancement in the range of 0-250% for moderate wind speeds (from zero to 3.5 m/s) directly over the soil surface.

We are happy to include the Bowling and Massman paper results (and reference) in our discussion within the paper.

9. Lines 7-11 on C1709: CO_2 as a limiting factor and negative feedbacks. Response:

The reviewer focuses on a rather small, pedantic point. It is likely that CO2 does not *directly* limit root respiration but there exist multiple hypotheses on how it may indirectly affect root respiration (Qi et al., 1994). While we appreciate the reviewers scepticism of this point specifically, we suggest that they address the article (referenced by us above and on page 4805, line 2) from which we referenced this concept and that, since this is a part of the published literature, we may use this evidence as a component of our argument despite the reviewers misgivings.

Furthermore, the reviewer argues that "CO2 can also be consumed by biological processes in soils (for carbon even in chemoautotrophs)". Again, the reviewer misses the forest for the trees. The point under discussion is that CO2 and methane undergo very different production and consumption processes in soils. Unless the reviewer can point us to literature that suggests that 5%->90% of the CO2 produced in soils is then consumed by biological/chemical processes before diffusion/advection out of the soil profile (as happens with methane) then the purpose and point of the sentence remains intact. Having spent some few minutes looking for the references which the reviewer may have used to make this point we respectfully request the reviewer to provide suitable references to back up their assertion that biological CO2 consumption plays a significant role in soil CO2 fluxes.

10. Line 12 on C1709: Controls on wind flow. Response: What the referee says here is an over-simplification of what happens at the ground surface and she/he must know it is an over-simplification. Yes, of course, wind flow is ultimately controlled by the pressure gradient force, but local accelerations and turbulence in wind flow are not related in a simple way to pressure and can, indeed, cause changes in the pressure distribution in the soil. We are not dealing with a laminar incompressible fluid here that flows slowly along a pressure gradient. We believe we are quite correct in stating "Variations in wind speed at the soil surface both over time and spatially can lead to variations in pressure within the soil profile." A simple example can demonstrate our point. If one blows across the top of a bottle one can affect the air pressure in that bottle. The air

pressure in the bottle does not (immediately) affect the air flow across the bottle top. In this example, the (pressure) controls on the wind flow across the bottle top are separate from (unrelated to) the pressure in the bottle. We have started to explore these effects using a computational fluids dynamics approach with engineering colleagues and will report the findings in due course.

11. Line 17 on C1709: Cite Fang and Moncrieff. Response: We are happy to do so but already cite quite a few other studies. It is normal to cite examples of studies that substantiate the point being made rather than all studies (reference lists would become unwieldy if the latter were done).

12. Line 18 on C1709: "similar pressure" is vague. Response: The reviewer must be aware that this is a semantic point. At which exact position do they wish us to "specifically" list our pressure differentials? We do, in fact list specific pressure differentials in Table 1 (which the reviewer at least considered since they draw attention to our significant figures later in the review). Likewise, Figures 2 and 4 provide very specific values for the pressure differentials we consider to be "similar pressures". Again, contrary to the intentions of the reviewer, the purpose of the sentence was not to provide individual, discrete examples of each pressure differential that we measured but to note that with increased wind speeds both on the interior and exterior of the toroid we could minimize the pressure differential. We feel as authors that this purpose is clear to readers, especially considering the text, tables and figures that follow later on within the article.

13. Line 20 on C1709: "in many cases" is vague. Response: We are correct in stating "in many". We have not counted every study so cannot provide an exact proportion, but the referee is being silly in saying that "in many" implies "in all cases". Of course it doesn't and we don't say that it does.

14. Line 20 on C1709: clear hypotheses are needed. Response: No, they are not. We have set up a simply-formulated experiment to look at how soil-atmosphere trace gas fluxes are affected by wind/pressure differential. There is no need to guild the lily by

adding hypotheses that are already clearly implied; to do so would actually look quite awkward and clumsy. Table 1 provides details of our experimental setup.

15. Line 1 on C1710: label figure. Response: This is a helpful suggestion, and we are happy to add labels to the figure. We already provide dimensions in the text and believe the figure would be too cluttered with information if we added them to that.

16. Line 6 on C1710: is the inner chamber really a toroid. Response: This, we think, is another example of petty pedantry. Yes, strictly-speaking, the referee is correct. However, 'toroid' or 'torus' can also be applied to any ring-shaped object and that is our intention here. We think 'toroid' is suitable shorthand for the volume enclosed in the chamber.

17. Line 9 on C1710: add reference to section 2.3. Response: This is a helpful suggestion and we are happy to add the reference.

18. Line 11 on C1710: analyzer flow rate. Response: The rate is 0.45 litres per minute (when operated in low flow mode), and we are happy to add this information to the revised manuscript.

19. Line 11 on C1710: "massive wind", experimental flaw. Response: We did not simulate "massive wind", whatever that means. The reviewer criticizes us earlier in their review for using imprecise language yet uses a far more egregious term here. Our wind speeds are reported in Table 1 and can also be read from the figures and are actually quite modest at up to 3.5 m s-1 (equivalent to 7.7 miles per hour). The reviewer misunderstands how our measurements were taken and we're happy to revise our explanation to make what we did clearer. In the paper we note that the gas concentrations IN THE TOROID (we do not say the soil) were allowed to return to ambient levels before the next reading was taken; we do not say that the soil returned to the same state as before the reading was taken (which would have made Figure 5 impossible if it were true). What is clear from our data is that we did not exhaust our soils (in the cases when net fluxes of the gas were from soil to atmosphere). Thus,

as wind speeds increased, so did fluxes. If we had purged the soils, such increases would not have occurred (as shown in Figure 5). It should be noted that at each wind speed our measurements were made over relatively brief periods (<6 minutes) but that fluxes were steady/linear in these brief periods. Of course, it is possible that if we had left longer gaps between measurements we may have observed an even steeper relationship between flux and wind speed, but such a relationship would only serve to reinforce our conclusion that advection is an important process in the studied soils. It is notable that one of our soils (Forsinard peat bog) DID show evidence of purging and for that one we did leave longer periods between measurements as made very clear in the paper (beginning of section 3.3 and data shown in Figure 5); this demonstrates that we were alert to the issue raised by the reviewer. To re-iterate: we strongly reject the suggestion that there are serious flaws in what we did but we accept that some additional explanation could and should be added to the manuscript to help readers.

20. Line 22 on C1710: pressure differential. Response: We wonder "why" the pressure differential between the soil and chamber is needed here? Given our data, and what we are trying to say (that only knowing aboveground wind speed is sufficient for an accurate prediction of the effect on soil-to-atmosphere trace gas fluxes) why is this information relevant?

Beyond this consideration, we describe in fairly thorough detail why this information would be irrelevant in Section 4.1 (lines 19 on page 4816 through line 22 on page 4817), in which location we cite the Xu et al., 2006 paper noted by the reviewer. We note in this section that the specific location (depth within the soil profile and surface topography) of any sub-surface prove will change its noted pressure differential (due to the sub-surface pressure, temperature and concentration gradients as described previously in the paper), not to mention the difficulty in placing the probe without significant disturbance to the soil (leading to changes in recorded pressure differential relative to undisturbed soils). Our point specifically in section 4.1 is that pressure (and pressure differential) measurements ARE NOT HELPFUL AS HELPFUL AS WIND SPEED

MEASUREMENTS, a point with which the reviewer does not engage within their review.

21. Line 1 on C1711: CO2 and CH4 fluxes. Response: This is another somewhat curmudgeonly point. We're simply trying to justify why we normalized the data. We're happy to amend the wording in a revision to something like "Generally we might expect CH4 and CO2 fluxes to differ. To compare the fluxes of the two gases...". The referee is, however, too strong in his/her assertion. Sometimes different gas species have similar flux behaviour and the flux of one gas is not defined in terms of how it differs from another gas; that is an odd assertion to make. The flux of a gas can be considered in isolation; it does not have to be referenced to another gas.

22. Line 12 on C1711: Relationships in figures. Response: We are bemused by this comment. The figures show an inclined plane and clearly show increases in trace gas fluxes with increased wind speed over the measured surface. It also clearly shows that fluxes decrease when winds blow over regions nearby, but not directly over, the measured surface. These points are also clearly described in the text (Section 3.2, lines 1-19 on page 4819) and in Tables 2 and 3. We have sought views from colleagues (and the editorial board of Biogeosciences) on these figures and they either a) have found them easy to interpret or b) have not provided any better mechanism for displaying the data at hand. Our method of presentation is not unusual and it is possible to see how fluxes increase with wind speed in both the toroid and the outer tunnel.

23. Line 14 on C1711: Standard terminology. Response: For equation (1) we use standard dimensional notation, where M denotes mass, T time, and L length. This is a scientific standard and it is obvious that it is used here – for example L3 denotes a volume and M L-2 T-1 a mass flux per unit area. Equally, mbar is commonly used in atmospheric sciences, but we are happy to make the change to kPa recommended by the reviewer.

24. Line 17 on C1711: Use of "in" in the figure captions. Response: We are happy to remove 'in' from the figure captions.

25. Line 19 on C1711: Too many significant figures in tables. Response: We agree, and thank the reviewer for this comment although we note that the reviewer is confusing wind speed data with pressure differential (described in the Table caption, clear in the Table itself given that there is no such thing as a negative wind speed, and obvious from the data presented in Figures 2-4). We're happy to revise the tables so that they have numbers/values with fewer significant figures.

Referee #2. We were disappointed with this review, because it repeats – almost verbatim – one of the reviews made during the preceding round of reviews prior to publication of this version of the manuscript in Biogeosciences Discussions. In fact, the version of the manuscript published here has ALREADY BEEN REVISED to account for the comments made in the earlier, duplicate review. Given that we have already modified our manuscript, taking on board this referee's earlier concerns, this duplicate review implies that this referee has made no attempt to re-read our manuscript. For clarity in our rebuttal we have included the comments and our previous response to them below:

Response to Comment 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 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.

Original text in quotes, additional text /changes follows.

"Static and dynamic flux chambers are widely employed to measure soil-atmosphere trace gas exchanges, but are usually set up such that diffusion-only conditions prevail (no or slow circulation of fan air) or under unrealistic conditions of within-chamber air flow (constant air flow generated by a single fan or set of fans) (see, e.g., Denmead, 2008; Rochette, 2011) which give an undefined combination of diffusion and advection. Gradient flux measurements also rely upon this basic assumption (that diffusion is dominant) [Myklebust et al., 2008]."

In general there is considerable uncertainty about the degree to which chambers provide reliable measurements, and problems with chamber use are discussed in the reviews by Denmead (2008) and Rochette (2011). The use of fans provides a good example of this uncertainty. Some authors, such as Davidson et al. (2002), suggest that chambers fitted with fans give unreliable readings. In contrast, Christiansen et al. (2011) found that, only in chambers in which the air was mixed by a fan, was the measured flux similar to reference fluxes (they introduced methane (CH4) at controlled rates through the base of various laboratory sand beds – some dry and some wet – and used chambers to record the fluxes above the sand). Furthermore, Denmead (2008) notes that chambers without fans or with fixed wind speeds may give unrealistic flux estimates, especially during windy conditions in the environment outside of the chambers. To illustrate the problem, he cites Denmead and Reicosky (2003) who, in a study of a tilled soil, found that, while carbon dioxide (CO2) fluxes within a chamber with a fixed-speed fan stayed steady, those in the area around the chamber (as measured using a micrometeorological dispersion method) increased with ambient windspeed. (lines 53-66 in revised manuscript)

"Even if we assume that diffusive fluxes are an important form of gaseous movement in soils, such fluxes are highly sensitive to gradients in local soil gas concentrations...."

Christiansen, J.S., J.F.J. Korhonen, R. Juszczak, M. Giebels and M. Pihlatie (2011) Assessing the effects of chamber placement, manual sampling and headspace mixing on CH4 fluxes in a laboratory experiment, Plant and Soil 343, 171-185, doi: 10.1007/s11104-010-0701-y.

Davidson, E.A., K. Savage, L.V. Verchot and R. Navarro (2002) Minimizing artifacts

and biases in chamber-based measurements of soil respiration, Agricultural and Forest Meteorology 113, 21-37.

Denmead, O.T. and D.C. Reicosky (2003) Tillage-induced gas fluxes: comparison of meteorological and large chamber techniques, in Proceedings of the International Soil Tillage Research Organizations 2003 Conference, Brisbane, Australia, July 13-18, 2003.

(References added between lines 533 and 548 in revised manuscript)

Response to comment 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.

Original text in quotes, new text follows.

"Here we close this knowledge gap by using a novel nested wind tunnel (Fig. 1) to investigate the role of advection in regulating soil-atmosphere gas exchange for two different trace gases", each of which is controlled by very different processes at different depths within the soil. Carbon dioxide, under dark conditions, is predominantly produced through plant, fungal and bacterial respiration and will have high soil concentrations (relative to the atmosphere) close to the soil surface. In contrast methane, whose biological response in soils is broadly insensitive to sunlight, is often consumed by aerobic soils and therefore has lower than atmospheric concentrations within the soil column. At greater depths within the soil profile, in anaerobic regions, methane can be produced by methanogenic archaea but much of this produced methane is consumed by methylotrophic bacteria in the regions directly above the production zone. (lines 107 to 117 in revised manuscript)

"Using four sites, we investigated three different ecosystem types: peat bog (two sites), evergreen coniferous forest, and managed grassland. We use the empirical data that we collected to build upon the model proposed by Massman (2006) in which diffusive flow is enhanced by pressure-based mixing."

Response to comment 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.

Original text in quotes, new text follows.

"The straight-line wind tunnel enclosing the toroid comprised a standard aluminium and wooden agricultural tunnel (FirstTunnels, Lancashire, UK) (3.5 m long x 2 m wide x 1.5 m high) with the option to be covered by PAR transparent or opaque plastic sheeting."

This option allowed the combined wind tunnel system to be capable of examining the soil-plant-atmosphere system under either respiration- or photosynthesis-dominant conditions (Fig. 1). Only 'dark' results are shown here. In terms of soil-atmosphere CO2 exchanges, diffusion will almost always occur from soil to atmosphere because soil CO2 concentrations are higher than those in the atmosphere above due to ongoing respiration by plants, fungi and bacteria. By using dark conditions, we were able to remove photosynthetic uptake of CO2 and its assimilation into plant tissue as a confounding factor. That is, we were able to interpret a decrease in chamber CO2 concentrations as due to advective transport processes without having to adjust our data for CO2 fixation by plants which can vary greatly with small changes in incident irradiance. (lines 173 to 182 in revised manuscript)

Response to comment 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 detailed and concise explanation about the wind tunnels/chambers impact on the data is needed.

Original text in quotes, additional text follows.

"Three high-volume drum fans (DF24S, Prem-I-Air, Manchester, UK) were placed at one end of the wind tunnel, each capable of moving 235 m3 of air per minute at the highest speed setting (for a maximum calculated wind speed of $\sim$10 m s-1)."

The toroid and outer tunnel were in place at the respective field sites (see below) for one or two days. Between measurements, which typically took less than 10 minutes, the toroid was unshrouded (the available sunlight between measurements was similar to that of a regionally cloudy day) and its vents opened. Therefore, the effects of the apparatus on the soil being studied were kept to a minimum; i.e., gas concentrations in the air above the soil were not allowed to build over long time periods which would have affected gas concentrations in the soil and soil biochemical processes. (lines 186 to 192 in revised manuscript)

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