We note that this is the third review we have received for this paper. This review was solicited in response to concerns we raised with the editorial board over the first two reviews. The second of these reviews was, verbatim, the same as a review we received from the editorial board when we initially submitted our paper to BGD. We revised the paper, and the paper published in BGD already addresses to the editor's satisfaction the comments made in that review. The other review (by Anonymous Reviewer #1) made, we felt, unsubstantiated claims and assertions and bordered on the intemperate; we did not feel it represented a considered and unbiased review. We made a detailed rebuttal of that review but the editor chose to reject the paper for publication in BG. We wrote to editorial board expressing concern over the tone of the reviewer's comments and over the fact that the editor appeared not to have read our rebuttal before making his decision on rejection. It seems that at least some of our concerns were shared by the editorial board, who agreed to re-open or re-start the review process. The first review under that re-starting of the process (from Anonymous Reviewer #3) is the one we respond to below.

Opening paragraph of the review.

The reviewer notes that "Flux chambers are, and create, artefacts". We agree. This is a truism that applies to virtually all measurement instruments. In a similar sentiment, the statistician George Box, famously said "… all models are wrong; the practical question is how wrong do they have to be to not be useful.". When using any instrument it is important that users are aware of its capabilities and what it can and cannot do. In our paper, we use a novel flux chamber (the toroid) nested within a wind tunnel to explore the effects of wind speed and pressure differentials on the flux of two gases across the atmosphere-soil boundary. We show how wind speed provides a more secure means of estimating flux than pressure and explain why this is so. Our apparatus also allows us to consider how small-scale spatial variations in wind speed and pressure above the soil surface affect gas fluxes across the atmosphere-soil interface. Such spatial variation can occur naturally (e.g., at boundaries between different vegetation types) and cannot be investigated using instruments and methods that integrate over larger spatial scales such as flux towers and eddy correlation.

The reviewer notes "I find this an interesting study, but also wonder what it could provide in terms of generalisable insights". Perhaps we can help the reviewer here. We looked at two gases (for which soil-atmosphere fluxes were significantly different) and three different soil types (one under two types of hydrological condition) and considered a range of wind and pressure conditions on each soil type. Obviously, we have not looked at every soil type, but we have not restricted our attention to one soil under a narrow range of conditions. Therefore, we are strongly of the view that our results are of general interest because they apply to a wide range of conditions and should be applicable to all trace gas fluxes from terrestrial systems (including, for example, nitrous oxide).

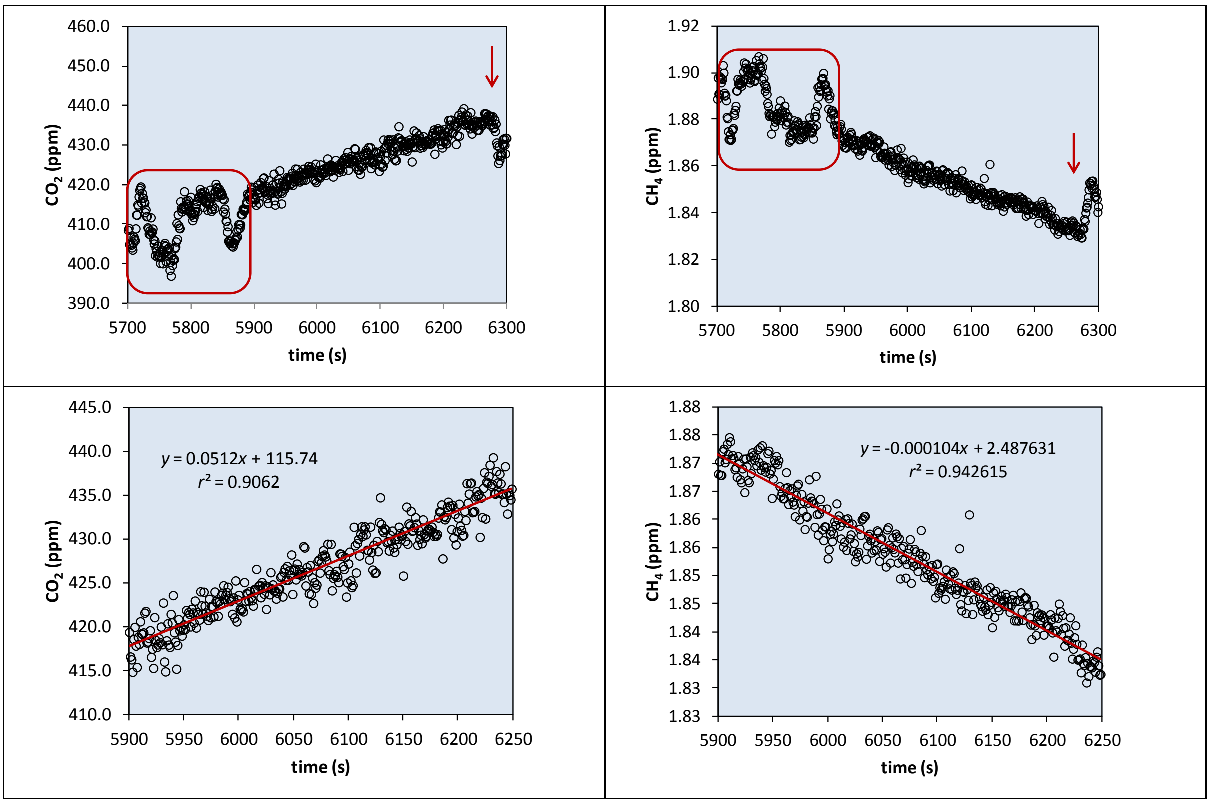
The reviewer suggests wind flow in the toroid is "cyclonic". We agree that air will move across an annulus of soil surface as it flows through the toroid but we don't see why that is a problem as implied by the referee. The referee also seems to imply that wind speeds in the toroid were unusually high. Maximum wind speeds in the toroid were actually quite modest at 3.2 m/s (see Table 1 in the paper), which is equal to 11.5 km/hour or ~7.2 miles/hour. Such speeds are not uncommon even close to ground level, so we cannot understand why the referee seems to think they are a problem; they certainly do not represent conditions that might be expected in a cyclone. We also note that we looked at a range of wind speeds, including zero flow conditions, and did not restrict our attention to higher speeds.

Page 4807, line 17-19.

The referee questions the dimensions of the toroid. We thank them for this comment and note that we stated the diameter of the toroid wrongly in the paper. It is 1.18 m and not 1 m as we originally wrote. Our other dimensions as given in the paper are correct. We apologise for this typographical error.

Page 4813, lines 14-20.

The referee suggests we use r^2 to suggest linearity in the relationship between gas concentration ([gas]) and time. We don't and would have been naive to do so for the reason the referee gives. We simply note in the paper that there was a linear change in [gas] over time and that we applied a linear fit to the data. We report the r^2 values to indicate the goodness of that linear fit. We show below an example of our data to demonstrate that it is reasonable to use a linear fit. In the figure the red boxes indicates fluctuations in [gas] at the beginning of a test during the setting up of the toroid, while the arrows indicate the end of the test. The data are from one test and show that we used the same time period to estimate the fluxes of both target gases which were measured concurrently.



The referee suggests that there is a single state-of-the-art-paper that should be used to analyse flux chamber data. Many 10s (and probably 100s) of studies are published each year where chambers have been used to measure atmosphere-soil gas exchanges and these employ a wide range of methods to analyse their data. There is no single correct approach and, when they happen, departures from linearity can occur for a variety of reasons that require different approaches. For example, a departure from linearity due to ebullition (bubbling) is very different from departures due to high chamber concentrations of a gas affecting diffusion gradients between the soil and atmosphere. With such considerations in mind we think it is misleading for the referee to suggest that there is an 'industry standard' for analysing flux chamber data and to imply, therefore, that our analysis has not followed best practice. As our data show, our assumption of linearity is entirely appropriate. It is interesting to note that the paper cited by the referee (Pedersen et al., 2010) actually recognises that a linear model can be appropriate; it is only necessary to account for non-linearity when the data 'demand' it. It is unfortunate that the reviewer has not seen fit to note this part of the cited paper. It is also noteworthy that there are other papers, such as Forbrich et al. (2010) which advocate the linear model as providing a conservative estimate of chamber fluxes.

Forbrich, I., Kutzbach, L., Hormann, A., and Wilmking, M. 2010. A comparison of linear and exponential regression for estimating diffusive CH4 fluxes by closed-chambers in peatlands. Soil Biology & Biochemistry, 42, 507-515.

Page 4817, first paragraph.

We did not quite follow the point being made by the referee here. As far as we can tell, the referee seems to assume that a pressure increase in the chamber can only be dissipated through a change in the volume of the air in the soil below and around the chamber. Soil air will partly buffer increases in chamber pressure, but pressure increases in the chamber may also lead to air flow between the chamber and the soil, a process incorporated into our conceptual model of atmosphere-soil gas exchanges. We did not observe leaks in our sand seals but they are possible. However, if they did occur, it seems unreasonable to assume that all of the air moved out of the chamber in this way; some compression/air flow/buffering was likely to have happened. In addition, and importantly, our point in the paper remains: pressure (differential) measurements do not provide a good predictor of atmosphere-soil gas exchanges. The referee's comment only strengthens this argument.

Page 4821, lines 6-8.

We are not sure what point the reviewer is trying to make here and would welcome some elaboration/explanation. Longer time scales of soil-atmosphere gas exchanges are made up of shorter time scale fluxes so our analysis will apply to a range of time scales, and in fact this is an assumption made across many studies in which fluxes are extrapolated from shorter time scales to longer time scales. Likewise, in nearly all terrestrial environments wind and its effects on fluxes have been essentially ignored to date. This is in strong contrast to ocean flux studies where there has been a rigorous debate on the exact scale and behaviour of the (perceived to be) wind based effect since the mid 1980s (e.g., Liss and Merlivat, 1986).

Liss, P.S., and Merlivat, L. 1986. Air-sea gas exchange rates: Introduction and synthesis, in The Role of Air-Sea Exchange in Geochemical Cycling, edited by P. Buat-Menard, pp. 113-129, D. Reidel, Hingham, Mass.

Overall, we are concerned that we have received a review where a few concerns have been raised by the reviewer but where there is no explanation or detailed argument to support those concerns. We appreciate that reviewing is a time-consuming and onerous task but are disappointed that the reviewer makes statements such as those in the first and last paragraphs of their review that are difficult to follow and/or which lack substantiation. More generally, we note that none of the reviewers have substantively challenged our results or methodology; they have tended to challenge details and exact interpretation.