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Interactive
Comment

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 #1

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This paper describes a study of soil trace gas fluxes when perturbed by a combination of two nested wind tunnels. The authors demonstrate that with a wind tunnel they can influence trace gas fluxes with a few days measurements at 4 sites, and offer some discussion about it. The approach is innovative, and I was initially quite excited to read it. However, I cannot see that this work provides new insight relative to current knowledge. The main point is that applying a major wind forcing influences trace gas fluxes in soils, which we have known for a long time. The text does not indicate a thorough understanding of the current state of the science regarding gas transport in soils. For example, the authors highlight what they feel is their most important contribution with this statement from the discussion "We propose that boundary layers develop at the near surface in soils, similar to that of plant canopies or the near-surface ocean". This "new" concept has been known to the earth system science community for decades,

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and is present in every text I am aware of that deals with micrometeorology or microclimate.

The authors state on page 6 (line 14) that they are providing an enhancement to the very comprehensive theoretical model of soil gas transport and pressure pumping of Massman (2006). There is no linkage to Massman's model and no rigorous physically-based theory offered.

The paper would be easier to read and to understand if it was guided by testable hypotheses. However, even after revision to improve the writing, I don't feel that the experiments performed, and the knowledge gained, are of sufficient rigor to merit publication in Biogeosciences.

Specific comments:

Abstract: Wind and pressure are not independent quantities. Wind velocity is bulk fluid flow velocity cause by a pressure gradient in the direction of the gradient. Hence a statement like "the combined effect of wind and pressure on these fluxes" in the abstract is awkward. This awkwardness is pervasive through the paper. On page 16 is this phrase "Our study is the first to consider both wind and pressure effects simultaneously. . .".

Abstract: "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". A more general model would allow for reactions (biogeochemical consumption or production or isotopic exchange).

Page 3 line 9: "different types of diffusion" – this is confusing, and I don't agree that there are different types of diffusion – diffusion is the process of energy or mass transport resulting from gradients of energetic content of molecules – there are special cases (eg, ion diffusion, isotope diffusion, Knudsen diffusion but these are all special cases of the general property) - why is this artificial distinction needed?

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Page 3 line 22: “slow flow” means advection – if there is bulk fluid flow then by definition there is advection

Page 4 first paragraph – this section could be enhanced by citing Fang and Moncrieff (papers below)

Page 4 line 14-15: there is more than half a century of literature establishing that diffusion is the dominant form of transport in soils – this sentence is misleading

Page 4 line 25-26: How is CO2 concentration a factor influencing biological production of CO2? (I don’t agree that it is.) Further, CO2 can also be consumed by biological processes in soils (for carbon even in chemoautotrophs)

P5 lines 1-2: those negative feedbacks are almost certainly due to oxygen limitation – CO2 excess does not mechanistically limit biological respiration

P5 lines 8-10: this is awkward – variation in wind velocity at the soil surface is caused by atmospheric pressure gradients (larger spatial scale than your site) – pressure is the force that moves fluid mass – wind is a result of pressure gradients on the landscape – you are correct in the following sentence that other factors (gravity, cohesion-tension+ gravity, and temperature) can also induce P gradients

P5 lines 14-16: again Fang and Moncrieff if you want to include chamber effects here

P7 line 2: “similar pressure” is vague – P differences < 1 Pa lead to substantial changes in soil efflux rate – please be quantitative

P7 line 5: “in many cases” is vague – not all “flux” measurement techniques are based on this assumption, some in fact are based on advection or turbulence or radiation etc.

P7 line 8-15: this paper needs to be guided with clear hypotheses – this paragraph states that the authors “test a number of . . . scenarios”, then provides only some possible examples “for instance” – be very clear – what exactly are the hypotheses being tested?

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Figure 1– the elements listed in the caption are not immediately obvious in the figure – perhaps label the figure – dimensions would be helpful as well on the figure or in a supplemental drawing – these are critical as the advection that is induced will be a function of not only the fan speed but also the physical dimensions, especially related to the depth of soil and size of toroidal versus linear chamber

The “toroidal” chamber looks to me like nested cylinders – isn’t a toroid the volume that results if you rotate a planar circle around a straight-line axis within the same plane? (eg, a bicycle tire inner tube?)

P7 line 18: the description of the “inner chamber” is not sufficient – I don’t understand how this chamber is used to monitor trace gas fluxes (but then later I learn that this is described in section 2.3 – so better to indicate that here “see section 2.3”)

P 12: what was the flow rate to the trace gas analyzer?

Pg 12 line 10: “2-3 mins”. If I understand correctly, this means that measurements were made with varying wind speeds in the tunnel(s), then the trace gas composition of the chamber was monitored (aboveground), and stabilized after 2-3 mins. This then indicated to the authors that the perturbation to diffusion caused by the massive wind from the tunnel was gone within 2-3 mins, and so they went ahead with the next measurement? This makes absolutely no sense to me – it would take the diffusive system much longer to recover to pre-disturbance values (many 10s of minutes, although this can be estimated using calculations of planar diffusion and some assumed gradients). This seems a serious experimental flaw.

P 12 last paragraph: Some quantification of pressure differential (difference between pressure in the soil and in the trace gas chamber, as the tunnel and toroid wind speed vary) associated with this chamber/tunnel system is sorely needed. Xu et al. (2006, cited by the authors) highlight that this cannot be done with the chamber on the soil, because advection through the soil eliminates the P difference.

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P 13 line 21: I don't understand the scientific value of this phrase: "Trace gas fluxes are likely to be significantly different for different trace gas species". Of course CO_2 fluxes and CH_4 fluxes are different, by definition.

As I finish the methods section, I don't know what to anticipate for results. The sets of experiments performed are not described except vaguely.

Pg 14: "Our data show that wind speeds were better at predicting trace gas fluxes than pressure differentials (Figs. 2–4)" This sentence conflicts directly with this one from page 12 "Pressure differential, soil temperature, ambient air temperature and internal wind speeds were not measured within the isolated toroid and straight line wind tunnels during each measurement period.". If you didn't measure pressure differential and wind speed, where did the data in figures 2-4 come from?

I cannot see any relationships in the three-dimensional Figures 2-3. This should be presented in some other fashion.

Description of Eq. 1: terminology is not consistent with SI recommendations (eg, T for time, L for liters)

bar is not the SI unit for pressure (Pa is correct, so pressure should be reported in kPa)

Figures in general: it is quite unusual to include the word "in" when describing the units on an axis label (eg, (in min) instead of (min))

Tables in general: there are too many significant digits presented for the uncertainty associated with these measurements (wind speed to 3 decimal places?)

References:

Fang, C. and Moncrieff, J.: An improved dynamic chamber technique for measuring CO_2 efflux from the surface of soil, Functional Ecology, 10, 297–305, 1996.

Fang, C. and Moncrieff, J. B.: An open-top chamber for measuring soil respiration and the influence of pressure difference on CO_2 efflux measurement, Funct Ecol Funct

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Xu, L. K., Furtaw, M. D., Madsen, R. A., Garcia, R. L., Anderson, D. J. and McDermitt, D. K.: On maintaining pressure equilibrium between a soil CO2 flux chamber and the ambient air, Journal of Geophysical Research-Atmospheres, 111(D8), D08S10, doi:10.1029/2005JD006435, 2006.

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