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Interactive comment on "Deep CO_2 soil inhalation/exhalation induced by synoptic pressure changes and atmospheric tides in a carbonated semiarid steppe" *by* E. P. Sánchez-Cañete et al.

Anonymous Referee #3

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General Comments and recommendation

The authors present results from an interesting study on soil gas transport, which, I think, is a valuable contribution for the scientific community. Soil CO2 concentrations were monitored at 3 different depth in a Leptosol in a semiarid steppe for 2 years, together with CO2 fluxes (eddy covariance) and a set of environmental parameters. The CO2 concentration at 15 cm depth seems to be controlled by soil temperature, soil moisture, and atmospheric turbulence, which are "normal " factors controlling soil CO2 concentrations at many sites. The CO2 concentration at 0.5 m and 1.5 m depth

exhibited a large variability, even on daily scales, which were correlated with changes in barometric pressure (synoptic and semi-diurnal pressure changes). This was related by the authors to advective flow of air due to changing atmospheric pressure, that pumped CO2 enriched air out of the soil during periods of falling barometric pressure, and pumped atmospheric air into the soil during rising barometric pressure. During such a period of falling barometric pressure a relative decrease in ecosystem CO2 uptake (eddy covariance measurement) could be observed, supporting the concept of "soil ventilation" due to pressure changes. Changes in barometric pressure can cause airflow in the soil as described in Fig 6 (e.g. Clements and Wilkening , 1972 about Rn222). Whether this effect is relevant at a site or not, depends on the magnitude of the pressure changes and the volume of air in the soil. Barometric pressure changes are usually less than 5-10% of the mean pressure. In the case of the observed semi-diurnal changes it was $\sim$3hPa, so less than 0.5% of the mean barometric pressure, which is very low. Hence, a very large volume of air-filled soil pores is needed to be compressed. The speed of the propagation of the pressure wave into the soil (and geological formation) depends on the air permeability and air filled pore volume, so that the pores have to be well connected (high air permeability; cracks and fractures?). A (maybe geological ?) CO2 source at a greater depth (much more than 1.5 m !) seems to be the reason for the observed semi-diurnal variation of CO2 at 0.5 and 1.5m depth. But with this concept in mind (Fig 6), I would have expected a time lag between the measured concentrations at 0.5 m and 1.5 m depth. But this time lag, in fact, seems to have been inverse sometimes. There are some minor issues that I would like to address, especially regarding the statistics, and information on the study site. But I believe that the paper is overall interesting and substantial, and therefore I recommend publishing after appropriate revision.

Specific comments

Statistics and conclusions:

First of all: I′m not a professional statistician, so please correct me or ignore me if I′m wrong! But I′m not too happy with some details and conclusions drawn from the statistical analysis, i.e. the approach to use standardized values and a moving window of different width (0.5 days and 3 days). I would be strongly interested to see also the correlation coefficients of the non standardized data or at least of data standardized for time span which includes the periodicity of soil moisture changes. The standardization can have intended but also unintended effects. An example:It is possible to have minor artefacts like a tiny temperature dependence in the measurement in the soil water content (due to tiny effects in the electronic circuits), which might be negligible on the time scale of a year- they fall into "accuracy". If now a low and stable soil water content is measured during the dry season, this temperature dependent artefact becomes "amplified" due to standardization, because the (actually tiny) major variability during the 0.5 or 3 day window is due to the temperature effect. The real effect of soil water content on soil gas transport is due to the slow changes over the dry season, however , which is filtered by looking only into the 0.5 (or 3) days window. Therefore, I recommend to include the correlation coefficient for the entire seasons in Table2 and 3. Another related question: a window of 0.5 days was chosen to focus on the semi-diurnal changes, like those of the barometric pressure wave and deep soil CO2? What does this mean for if the other parameters which have a periodicity of 24 h like the temperature?

Terminology:

In the beginning I felt uneasy with the term "ventilation", which – I thought first - seemed to be used equivalent to advective transport (p.5593 L.10: "Scientists have confirmed subterranean advective transport in laboratories"). The terminology is explained later when ventilation is defined in the paragraph following p.Âă5593 L.27. This could be easier when the definition was given before that paragraph.

Details

1. Introduction

In p 5593 L19 " barometric " before pressure? P 5593 L 12 : Clements, W. & Wilkening,

M. 1974. is a nice paper about pressure changes and RN222 transport Last paragraph of the introduction (p. 5594 L 10): This is the last paragraph of the introduction. So, I would have expected the objective of the study together with an hypothesis, and the outline of what was done. Yet, a hypothesis is lacking , and results are already discussed and conclusions drawn.

2.1 Study site:

I would be interested in more information about the soil, to have a more detailed picture of this specific site with its interesting gas transport "anomalies". What′s the reference for the soil classification? Please include directly some information given by the cited reference Rey et al. (2012) like "The carbonate formation has high porosity and permeability values with the presence of fissures and fractures." . What is the soil texture? Are there also cracks visible in the soil? (thinking of Weisbrod et al. 2009) Rock content? (you measured a SWC of $\sim$ 15 % in 1.5m depth – this can be "dry" in a clay soil or "wet" in sandy soil with a lot of stones..) Where is the ground water level?

3.1. Results

p5597 L.5 : Which soil T? 3.3. Daily patterns p5599 L 7: "Excepting synoptic pressure changes such as the events on 8 and 14 April and 6 and 12 August" In figure 3 the dates given do not exactly fit to the changes.

3.4 Coupling deep soil CO2 variations with the atmosphere

p5600 L13: I am surprised that it is beta and not gamma which is changed! If we have an an "additional" soil CO2 efflux this should affect gamma, if I′m right? 4. Discussion P5600 L. 26: "Shallow c shows maxima in winter and minima in summer coinciding with vegetation activity during winter (Rey et al., 2012a)". Yes, that′s right. But CO2 concentration in the soil are affected by productivity and transport conditions. Hence,the shallow c shows maxima in winter and minima in summer coinciding also with higher soil water contents which mean a lower soil gas diffusivity. P5601 L 10: Even if the variations of the measured CO2 concentrations at 1.5 m depth appear to be large, the concentration is still less than 4000ppm –(=0.4%) and I'm not sure whether this is sufficient to affect microbiological activity, unless there are other gases involved.. P5601 L 11l "The CO2 respired in the root zone increases air density (Sanchez-Canete et al., 2013; Kowalski and Sanchez-Canete, 2010), and so enables gravitational percolation through the pore space toward deeper layers where it can be stored". To apply this idea to root respired CO2 is new for me. Why should this occur here, but was not observed at other sides, where there is even more soil respiration (production of CO2)? P5601 L14: "Although in this study, p is the main factor implicated in deep c variations, Fig. 1 shows that c variability is greater in summer when p variations are reduced. This highlights the important role of SWC in CO2 exchange: despite greater synoptic pressure variability, winter has lower c variations because soil pores are filled with water, limiting gas flows. In summer, by contrast, ventilation is facilitated by dry soil conditions with gas-filled pore space (Cuezva et al., 2011; Maier et al., 2010). This explains why the growing period shows a positive correlation between shallow c and SWC at 0.15m (Table 2) and a negative correlation during the dry season (Table 3), since during the dry season there is less water in the shallow soil layer allowing the flow of CO2-rich air from the deep soil to near surface layers." I got the impression that things are mixed up. As explained in the beginning I have the feeling that the observed negative correlation of the shallow CO2 and SWC in the dry season could be an artefact due to the short time-scale (width of the moving window) of the standardization. Please include a correlation coefficients for the entire seasons in Tab. 2+3. P5602 L20 As mentioned before , I expected that gamma and not beta would be affected. Can you explain this?

Fig. 2

I would appreciate an additional graph with the soil moisture, because soil moisture has an important effect on soil CO2 concentrations. You could include on graph with the time series of the three CO2 concentrations and one graph with barometric pressure and SWC. This also applies to Fig 3 (if you can show in Fig2 that SWC remains stable

during the period covered in Fig. 3 this is not necessary for Fig3)

Literature:

Clements, W. & Wilkening, M. 1974. Atmospheric pressure effects on 222Rn transport across the earth-air interface. Journal of Geophysical Research, 79, 5025–5029

Noam Weisbrod, Maria Inés Dragila, Uri Nachshon, Modi Pillersdorf, 2009 Falling through the cracks: The role of fractures in Earth-atmosphere gas exchange , Geophysical Research letters.

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