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Interactive comment on “Deep CO₂ soil inhalation/exhalation induced by synoptic pressure changes and atmospheric tides in a carbonated semiarid steppe” by E. P. Sánchez-Cañete et al.

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This study is highly relevant for flux measurements in semiarid regions where deep CO₂ sources and reservoirs can be flushed from the soil surface in response to pressure fluctuations. The data show a compelling influence of pressure variations on apparent soil CO₂ efflux rates that could be erroneously interpreted as biologically controlled.

The fact that this is a one-off observational study rather than a replicated experiment

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does on my view not limit its value. It is important to draw attention to the existence and extent of the role of deep soil CO₂ sources and storage, and this study clearly achieves this. Future work is likely to address more detailed investigations into this field.

I think that this study can be published pretty much in the form it is in now. I only have one comment regarding the final paragraph of the introduction (p. 5594, l. 7-14), where I suggest that you limit yourself to the key aims of your investigation in relation to the soil gas storage and transport issues you have outlined in the preceding paragraphs, and possibly outlining also the methodological approach. Don't however summarise results – this is appropriate for the abstract, but should not in the Introduction.

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