

Interactive comment on “Can land degradation drive differences in the C exchange of two similar semiarid ecosystems?” by Ana López-Ballesteros et al.

Anonymous Referee #2

Received and published: 16 July 2017

The authors compared NEE and biophysical factors between a “natural” grassland site and a “degraded” grassland site in the semiarid area of southeast Spain. They found that the “degraded” site showed less carbon uptake during the growing season but substantially more carbon release during the dry summer months. They attributed the inter-site differences in NEE to higher belowground CO₂ concentration at 1.5-m depth and stronger subterranean ventilation at the degraded site. The reported temporal patterns of NEE, ET and EVI at the two sites could promote a better understanding of the effects of land degradation on carbon sequestration in semiarid areas, and provide important information on ecosystem resilience and vulnerability under changing climate. However, I have some concerns regarding how the authors analyzed and interpreted

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their data.

Major comments:

1) The authors concluded that “subterranean ventilation of this vadose zone CO₂ . . . largely drives the differences in C dynamics between them”. This conclusion was based on authors’ analyses that compare many biophysical factors between the two sites. It turned out that belowground CO₂ concentration at 1.5-m depth differed the most between sites. In addition, they found a negative correlation between air pressure and subsoil CO₂. However, the reasoning behind this conclusion should be viewed with great caution. Large differences in subsoil CO₂ does not necessary explain inter-site differences in NEE. There was no analysis showing a causal link between inter-site variations in subsoil CO₂ concentration and CO₂ fluxes. Although the authors examined many potential explanatory variables, there still could be other biophysical factors and processes that differ greatly between sites (e.g., soil microbial communities).

2) An unanswered question related to the previous comment is why the degraded site showed such large subsoil CO₂ concentrations compared to the natural site. In addition, the degraded site (with much less vegetation cover) showed more carbon release than the natural site. Subterranean ventilation is only a transport process for CO₂, but the question is who produced so much CO₂? Was it abiotic processes related to carbonate dissolution, or respiratory CO₂ production by plants and microbes? This question must be discussed in the paper.

3) The authors only examined NEE dynamics. I would encourage them to also partition NEE and check the two major components of NEE: GPP and Reco. These two components may respond differently to land degradation and interannual climatic variations. Separate analysis on GPP and Reco could provide more information on the differences between the two sites in terms of carbon dynamics.

4) Table 1 showed that the vegetation cover almost three times higher at the natural site than at the degraded site, while the EVI in Figure 5 does not show such a large

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difference, at least for most years. So I am wondering whether the pixels you used for extracting EVI well match the location of your ground measurements. Or are there any other reasons for this discrepancy?

5) Writing of the manuscript should still be improved to be more concise and clearer. In addition, there are some very long and complex sentences, which encompass too many ideas (I pointed out some in Specific comments).

Specific comments:

- 1) Page 1, line 16-20. The background information is a bit too long.
- 2) Page 1, line 21-24. The sentence is too long and complex. Considering dividing it into shorter sentences.
- 3) Page 1, line 20. In “global C balance”, symbols should be defined upon first mentioning.
- 4) Page 1, lin3 21. Replace “needs further research” by “still need to be investigated”.
- 5) Page 1, line 25. Please specify what “±” stands for.
- 6) Page 2, line 1-11. This paragraph is a bit too long. The importance of drylands has been well acknowledged and should only be mentioned very briefly here.
- 7) Page 2, line 14-22. These case studies are not directly related to this paper. The first sentence of this paragraph already well summarizes the subject of research. I would delete or reduce these case studies.
- 8) Page 3, line 12-14. I would delete this sentence since the EC technique is a widely used method, and is familiar to most researchers working on carbon exchange.
- 9) Page 3, line 16. What did you mean by “absorb fast changes”?
- 10) Page 3, line 25-26. Please specify what kind of “short-term disturbances” you are talking about.

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- 11) Page 3, line 30. Replace “Experimental sites description” with “Site description”.
- 12) Page 4, line 20. The expression “different degradation stages” is not clear to me. More information on the history (degradation, recovery and succession) of the two sites should be provided. A basic question is what caused the degradation?
- 13) Page 5, line 6. Please specify what “±” stands for.
- 14) Page 5, line 8. More details on estimating uncertainty should be provided.
- 15) Page 5, line 15. Please clarify whether or not you took into account above- and below-ground storage terms of heat fluxes when calculating the slopes?
- 16) Page 5, line 21-22. I would delete this sentence. As you said, it is a widely used index, so there is no need to justify using it.
- 17) Page 6, line 5-9. Please reword the sentence.
- 18) Page 6, line 18. Delete “over the study period”.
- 19) Page 6, line 20. The term “annual average precipitation” should be changed to “mean annual precipitation (MAP)”.
- 20) Page 7, line 5. Please delete “(C)” as you have defined it in Introduction.
- 21) Page 8, line 20-25. I would shorten or remove these sentences since interannual variability is not the focus of this study.
- 22) Page 9, line 3. Why did you use a threshold value of 1 for Diffst.
- 23) Page 11, line 25. By saying “stable” did you mean “resilient”?
- 24) Page 11, line 27. No need to give the definition of “ecosystem resilience”. It is a textbook concept that everyone knows.
- 25) Figure 1. It would be nice if you can add some photos of landscape or vegetation at the two sites.

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26) Table 1. The first part of the table (site characteristics) can be removed. These characteristics were well-described in the text and are therefore redundant here. What was the measurement depth for SOC?

27) Figure 3. Adding cumulative GPP and Reco may help understand inter-site differences in carbon dynamics.

28) Table 2. I would put this table in Supplementary Online Materials.

29) Table 6. This table is confusing to me. Can you explain, for example, what does “dCO₂, 1.5m_6h” mean?

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-77>, 2017.