Reply to Referee #2

We would like to thank referee #2 for the detailed review of our manuscript and the suggestions that have helped to improve our manuscript. In the following, we will answer each of the referee's comments.

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 their data.

General comments

1. The authors concluded that "subterranean ventilation of this vadose zone CO2 largely drives the differences in C dynamics between them". This conclusion as 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).

Although we did not test the direct link between subsoil CO_2 molar fraction at 1.5m and net CO_2 exchange fluxes, our results lead to this hypothesis, since inter-site differences in meteorological variables are minimal. On the other hand, although measurements of the metabolic activity of microbial communities would help to understand inter-site differences in heterotrophic respiration patterns, we think that

a potential difference in soil microbial communities should not totally explain the important carbon release observed in the "degraded" site during the dry season, mainly due to a lack of water in the soil at both sites. Apart from that, a previous study performed at Amoladeras relates this carbon release to atmospheric turbulence through linear regressions with friction velocity (u_*), and also shows an influence of net radiation and VPD in the CO_2 fluxes (López-Ballesteros et al., 2017). Overall, based on other investigations performed by this research group in several ecosystems located in the same province (Kowalski et al., 2008;Sanchez-Cañete et al., 2011;Serrano-Ortiz et al., 2009;Sánchez-Cañete et al., 2013;Pérez-Priego et al., 2013), we strongly believe that there is an advective transport of stored CO_2-rich air from the vadose zone to the atmosphere, especially under high hydric stress and high turbulence conditions.

Apart from this, we have included in Table 4, the linear regression results for subsoil CO_2 at 1.5m and net CO_2 exchange fluxes, and as expected, a higher correlation was obtained in Amoladeras.

***Table 4:** Spearman correlation coefficients (r_s) for every paired simple correlation among maximum quality net CO_2 exchange fluxes (µmol m^{-2} s^{-1}), absolute and differential pressure (hPa) at 6, 12, 24 and 72hours time-step and absolute and differential CO_2 molar fraction measured at 1.50 m below ground (ppm) at the same time-steps. Bold values represent the highest correlation coefficients while shaded ones denotes non-significant relationships (p-values>0.05).*

	Amoladeras						***Balsa Blanca***					
	Net CO_2exchange	*P*	*dP_{6h}*	*dP_{12h}*	*dP_{24h}*	*dP_{72h}*	*Net CO_2exchange*	*P*	*dP_{6h}*	*dP_{12h}*	*dP_{24h}*	*dP_{72h}*
$CO_{21.50m}$	***0.30***	*-0.66*	*-0.33*	*-0.46*	*-0.56*	*-0.55*	***0.11***	*-0.33*	*-0.51*	*-0.53*	*-0.62*	*-0.45*
$dCO_{21.50m\_6h}$	*0*	*-0.1*	*-0.84*	*-0.5*	*-0.26*	*-0.01*	*0.02*	*-0.07*	*-0.63*	*-0.19*	*-0.13*	*-0.04*
$dCO_{21.50m\_12h}$	*0.06*	*-0.08*	*-0.57*	*-0.87*	*-0.55*	*-0.05*	*0.03*	*-0.03*	*-0.46*	*-0.50*	*-0.31*	*-0.03*
$dCO_{21.50m\_24h}$	*0.03*	*-0.13*	*-0.47*	*-0.78*	*-0.85*	*-0.15*	*0.02*	*-0.04*	*-0.40*	*-0.59*	*-0.58*	*-0.04*
$dCO_{21.50m\_72h}$	*0*	*-0.28*	*-0.28*	*-0.49*	*-0.64*	*-0.74*	*0.00*	*-0.13*	*-0.28*	*-0.43*	*-0.57*	*-0.48*

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.

As explained by López-Ballesteros et al. (2017), the potential origins of the released CO_2 could be geological degassing and/or subterranean translocation of CO_2 in both gaseous and aqueous phases. In this publication, there is a detailed argument of these two hypotheses and also it is recognized that future research is needed in order to understand how CO_2 transport and production processes interact and modulate drylands' terrestrial C balance.

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.

In accordance to the comment of the referee #1 and your suggestion, we have performed the flux partitioning proposed by Lasslop et al.(2010) in order to estimate the magnitude of GPP and R_{eco} for both sites. However, in the case of Amoladeras, we first subtracted the flux magnitude corresponding to subterranean ventilation by applying a ventilation model (Pérez-Priego et al., 2013). The methodology used as well as the results are explained in the reply to referee #1, and would be included in the revised version of the manuscript.

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 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?

We have verified that the chosen pixels match the location of our ground measurements. Since referee #1 made the same remark, please read the answer to the fourth general comment of the reply to referee #1.

Specific comments

1. Page 1, line 16-20. The background information is a bit too long.

We have modified these sentences as follows: "Currently, drylands occupy more than one third of the global terrestrial surface and are recognized as areas vulnerable to land degradation. The concept of land degradation stems from the loss

of an ecosystem's biological productivity, due to long-term loss of natural vegetation or depletion of soil nutrients."

2. Page 1, line 21-24. The sentence is too long and complex. Considering dividing it into shorter sentences.

We have rewritten this sentence as follows: "In the present study, we compare net carbon C and water vapor fluxes, together with meteorological and satellite data and vadose zone measurements (CO_2, water content and temperature) between two nearby (~23 km) experimental sites representing "natural" (i.e. site of reference) and "degraded" grazed semiarid grasslands. We utilized data acquired in two eddy covariance stations located in SE Spain during 6 years with highly variable precipitation magnitude and distribution."

3. Page 1, line 20. In "global C balance", symbols should be defined upon first mentioning.

We have defined the symbol at this sentence.

4. Page 1, lin3 21. Replace "needs further research" by "still need to be investigated".

We have followed your suggestion.

5. Page 1, line 25. Please specify what "_" stands for.

We did not find that character in the sentence.

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.

We have shortened this paragraph as follows: "The concept of land degradation stems from the loss of an ecosystem's biological productivity, which in turn relies on several degradation processes such as long-term loss of natural vegetation, deterioration of soil quality, depletion in biodiversity or water and wind erosion (UNCCD, 1994). Drylands (arid, semiarid and dry sub-humid areas), which occupy more than one third of Earth´s land surface and are inhabited by more than 2 billion people (Niemeijer et al., 2005), have been recognized as areas vulnerable to land degradation processes. In fact, they have expanded globally for the last sixty years at an estimated annual rate of 5.8 million hectares in mid latitudes alone (Lal, 2001), and are projected to expand under future climate change scenarios (Feng and Fu, 2013; Cook et al., 2014), especially in the Mediterranean region, where major expansions of semiarid areas will occur (Gao and Giorgi, 2008; Feng and Fu, 2013)."

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.

We have reduced the case studies as suggested. We want to mention regions or countries where desertification has been assessed previously in order to assess the spatial representativeness of land degradation research globally. However, we have followed your suggestion by deleting the last sentence of this paragraph which talks about the global studies using modelling approaches, since our study is local.

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.

We agree and have deleted that sentence.

9. Page 3, line 16. What did you mean by "absorb fast changes"?

Here, we are talking about resilience, hence we have rewritten this sentence as follows: "Owing to the high temporal resolution of the EC method, we can assess the effect of land degradation as a slow change or disturbance legacy in the studied ecosystems and how, in turn, it influences ecosystem resilience to short-term disturbances, such as climate extremes (i.e. droughts, heat waves)."

10. Page 3, line 25-26. Please specify what kind of "short-term disturbances" you are talking about.

As written before, we meant climate extremes, such as droughts or heat waves.

11. Page 3, line 30. Replace "Experimental sites description" with "Site description".

Done.

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?

The stronger degradation effects observed in Amoladeras ("degraded" site) compared to Balsa Blanca ("natural" site) are probably due to its proximity to populated areas. The main factor provoking degradation in this Mediterranean area was the increase of rural population from the beginning of the 20th century until late 1950s (Grove and Rackham, 2001). At that time, timber extraction, the use of tussock fiber for textile manufacturing and extensive farming were common economic activities potentially causing a higher anthropic pressure on the

"degraded" site. Afterwards, rural exodus during the mid-century involved the abandonment of this agriculture and farming practices. However, although degradation drivers are not currently active, their effects are still observable in the area; this is a case of "relict" degradation (Puigdefábregas and Mendizábal, 2004).

We will add this information in the revised version of the manuscript.

13. Page 5, line 6. Please specify what "" stands for.

We did not find that character in the sentence. It seems that the referee has a problem with his/her PDF viewer, particularly regarding the symbol "±".

14. Page 5, line 8. More details on estimating uncertainty should be provided.

We have rewritten this paragraph as follows: "Missing data were gap-filled by means of the marginal distribution approach proposed by Reichstein et al. (2005) and uncertainty derived from the gap-filling procedure by using the variance of the measured data, which was calculated by introducing artificial gaps and repeating the standard gap-filling procedure. Twice the standard deviation of sums of total data was taken as the uncertainty for the several aggregating time periods we used in the analysis."

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?

We have included the following sentence to clarify it: "The storage term in the soil heat fluxes was included in the estimates while in case of sensible and latent heat fluxes, this term was negligible given the short height of the vegetation (~50 cm)."

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.

Done.

17. Page 6, line 5-9. Please reword the sentence.

We have reworded these sentences as follows: "This test was chosen because the variables used satisfied the independence and continuity assumptions but not all were normally distributed. The confidence level used was 95%. The effect size was evaluated using the median of the difference between the samples (Amoladeras minus Balsa Blanca), which was expressed as a standardized value (divided by its standard deviation; $Diff_{st}$; dimensionless) in order to be able to compare results among different variables."

18. Page 6, line 18. Delete "over the study period".

Done.

19. Page 6, line 20. The term “annual average precipitation” should be changed to “mean annual precipitation (MAP)”.

Done.

20. Page 7, line 5. Please delete “(C)” as you have defined it in Introduction.

Done.

21. Page 8, line 20-25. I would shorten or remove these sentences since interannual variability is not the focus of this study.

We have shortened this paragraph as follows: “On the other hand, differences in the inter-annual variability of EVI were found between years. Concretely, 2009/2010 and 2013/2014 were the years with maximum and minimum annual precipitation and EVI observations, respectively, for both sites. In 2009/2010, EVI observations were 28% and 20% higher than the six-year averaged values in BB and AMO, respectively. In case of the driest year, 2013/2014, the growing season (winter-spring) EVI was reduced 35% and 28% in BB and AMO, respectively.”

22. Page 9, line 3. Why did you use a threshold value of 1 for Diffst.

We chose that threshold just to comment the data, all results are shown in Tables 4 and 5, and therefore they can be interpreted by the readers.

23. Page 11, line 25. By saying “stable” did you mean “resilient”?

Yes. We have substituted “stable” with “resilient”.

24. Page 11, line 27. No need to give the definition of “ecosystem resilience”. It is a textbook concept that everyone knows.

We have removed the definition of “ecosystem resilience”.

25. Figure 1. It would be nice if you can add some photos of landscape or vegetation at the two sites.

We agree and we would include the picture below.

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?

We agree so we have deleted the first part of Table 1. Regarding the SOC measurement, given that these soils are shallow with maximum depth of 20 cm, we only took a composite sample of the profile.

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

Given the lack of flux data in both sites, instead of adding a figure of cumulative GPP and R_{eco}, we have added the following figure showing the monthly cumulative NEE (biological), GPP , R_{eco} and Water Use Efficiency (WUE) for both sites over the whole study period.

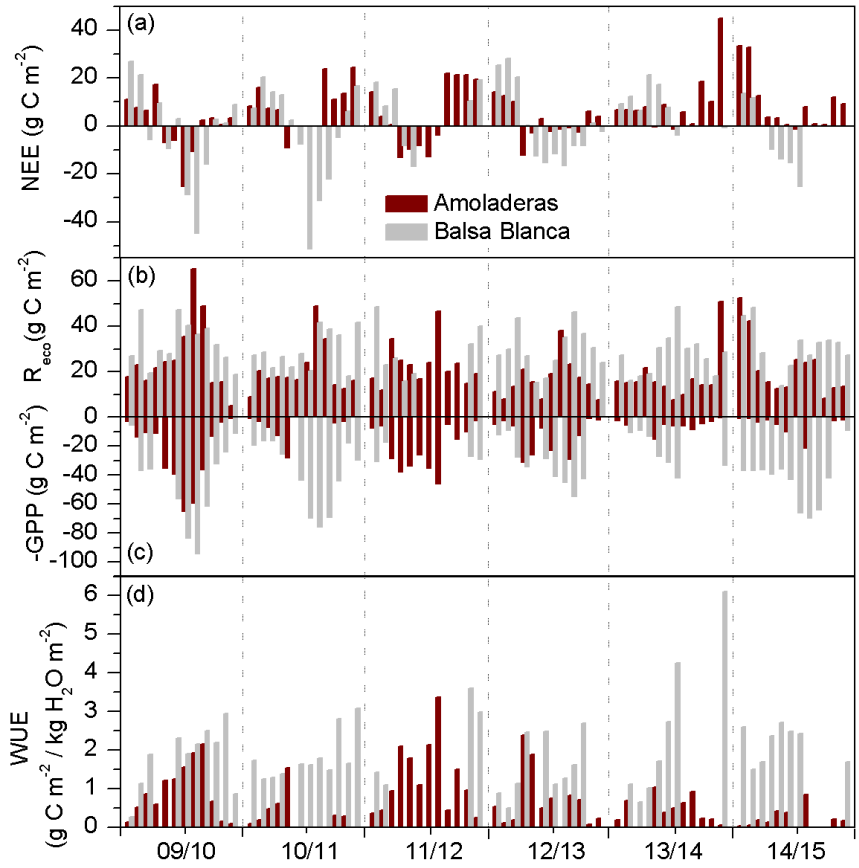

Figure 7: Monthly cumulative fluxes of (a) biological net ecosystem CO_2 exchange, (b) ecosystem respiration (R_{eco}), (c) negative gross primary production and (d) water use efficiency over the six hydrological years of study (2009-2015) for Amoladeras (dark red) and Balsa Blanca (grey). Lacking bars correspond to long-term data losses.

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

We believe that this information is crucial to interpret and compare these results with other studies. Hence, we do not think that it should be placed in the Supplementary Online Materials.

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

dCO2, 1.5m_6h means the difference between the CO_2 molar fraction measured at a time "t" and the CO_2 molar fraction measured 6 hours before "t-6h". This analysis was performed to show how different is the influence of pressure variations in the subsoil CO_2 molar fraction between sites, as demonstrated at another study in the area (Sánchez-Cañete et al., 2013).

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