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Biogeosciences
Discussions

Interactive comment on "Soil nitrogen response to shrub encroachment in a degrading semiarid grassland" *by* Thomas Turpin-Jelfs et al.

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We extend our sincere gratitude to both you and Anonymous Referee #1 for taking the time to review our manuscript, "Soil nitrogen response to shrub encroachment in a degrading semiarid grassland". The comments you both provided were constructive, and enormously beneficial to improving the overall quality and accessibility of the paper. Below we address the comments you made (indicated by a hyphen) in the order in which they were raised.

- The appeal of this paper could be broadened by incorporating more literature addressing these ecological phenomena in other biogeographic regions beyond the desert southwest.

This is a fair comment. To highlight the global significance of this phenomenon, we will add the following to the first sentence of the second paragraph on line 27, page 2 in our introduction, "Occurring in Africa (Hudak et al., 2003), Asia (Zhang et al., 2016), Australia (Cookson et al., 2006), Europe (Quero et al., 2013), and the North (Knapp et al., 2008) and South Americas (Cabral et al., 2003), extensive research has. . ." Further, we will amend the sentence on lines 15-17 on page 16 to, "This is supported by a study for a semiarid grassland in central Spain which showed the availability of inorganic N is largely influenced by the abundance and functional diversity of the microbial community (Delgado-Baquerizo et al., 2015).".

- Page 2, Lines 12-13: This sentence is not a particularly new or novel result. It would be good to summarize and synthesize your findings in a way that adds something new to what we already know about the effects of shrub encroachment on N cycling in drylands.

We will change this sentence to, "Overall, these results provide a greater insight into how grass- to shrubland transitions influence the soil N pool through associated impacts on the soil microbial biomass."

- Page 3, Lines 26-31: I appreciate that the objectives are presented clearly. Are there some hypotheses that could be offered to enhance the objectives?

We agree with the suggestion of supplementing our objectives with hypotheses and thus, we will include the following, starting on line 32, page three: "We expected that reductions in grass biomass resulting from higher levels of shrub encroachment would cause soil N to be progressively redistributed from intershrub zones to areas beneath shrub canopies. As the spatial distribution of soil microorganisms is positively influenced by resource availability (Schlesinger and Pilmanis, 1998), we hypothesised the enhanced fertility and environmental conditions in soils under shrubs would support a higher soil microbial biomass and promote N mineralisation processes leading to an increased availability of inorganic N. Accordingly, we predicted that rates of asymbiotic

BNF would decrease in shrub soils as inorganic N increased relative to bioavailable P (Smith, 1992)".

- Page 3, Lines 32-33: This sentence should be moved into the Study Site Description section.

Thank you, we will move this to the Study Site Description.

- Page 3, Lines 33-35: This sentence isn't necessary to your story.

We agree, and we will remove this sentence.

- Page 5, Lines 10-11: It would be good to provide a bit more information on the soils across the study areas. Is the taxonomy (i.e. order, series) identical across all of the sites?

Three soil orders have been described by Clemmons & Wheeler (1970) for the SRER. Soils from all of our sampling sites fall in the Combate-Diaspar Complex series (SRER Digital Database, https://cals.arizona.edu/srer/data.html). However, from the available information, we are unable to identify which order(s) our soils belonged to. We will amend the sentence beginning on page 5, line 11 to, "Soils at these sites were formed in alluvium from igneous rock of Holocene and Late Pleistocene origins (Batchily et al., 2003) in the Combate-Diaspar complex (CoB), characterised by excellent drainage, sandy loam textures and 1-8% slopes (SRER Digital Database, https://cals.arizona.edu/srer/data.html).".

- Page 7, Line18: Since ammonium and nitrate are ions, you should show their charges when you abbreviate their chemical formulae (i.e., NH4+-N and NO3–N).

Thank you, we will make the changes as you suggested.

- Page 13, Line 9: These C/N ratios for soils seem remarkably low. Although there may be exceptions, C/N ratios in arid and semiarid soils seem to be generally around 10-12. I'm wondering if your very low values are a consequence of the way that soil

was prepared for measurement of organic C concentrations. On page 6 line 13, you indicate that all visible organic matter was removed from the sample using forceps.

Apologies, line 13 of page six was erroneously included in section 2.3. Where necessary, forceps were used to remove any remaining loose plant matter (roots and shoots) from soils used in the acetylene reduction assays only. This was to ensure that plant-Rhizobium symbioses did not interfere with the asymbiotic nitrogen fixation aspect of our study. Consequently, we will move this sentence from section 2.3 to section 2.8.

We agree that the soil C:N ratios presented in our study appear low; however, similarly low ratios have been observed in other dryland studies (e.g., Thomas et al. 2018 Land Degradation & Development 29: 1306-1316). As the total nitrogen content of soils in this study are comparable with those from other areas within the SRER (Wheeler et al. 2007), we believe the discrepancies must be due to differences in concentrations of soil organic carbon. To acknowledge this issue, we will amend the sentence on lines nine and 10 of page 15 to, "However, like Wheeler et al. (2007), we found that soil C:N ratios, which are lower than those previously reported for the area (Wheeler et al. 2007), are not affected by shrub encroachment processes in the SRER (Figure 7a).".

- Page 15, Line 4: Another good paper to include with your citations on this line would be Schlesinger et al. 1990 (Science 247: 1043-1048).

Thank you, we will include this citation as suggested.

- Page 16, Lines 6-19. As another very relevant point of comparison for your work, see Creamer et al. 2016 (J. Geophys. Res. Biogeosci. 121: 1675–1688, doi:10.1002/2016JG003347). Thank you for making us aware of this study. In order to recognise its findings, we will add the following sentence to the end of the paragraph concluding on page 16, line 19: "As fungi have been shown to preferentially utilise grassland C (Creamer et al., 2016), declines in the shrub soil fungal PLFA content with increasing encroachment may be a response to the progressive depletion of grass-derived SOC."

- Page 17, Line 17: Perhaps specify if this value for N deposition is total N, wet deposition only, or dry deposition only. Also, it might be good to check to see if your value derived from a 2006 paper is still valid at the present time.

We will specify that the value reported is for total nitrogen deposition. Further, we will change the citation to the more recent Schwede and Lear 2014 (Atmospheric Environment 92: 207-220) publication, which is used to inform total nitrogen deposition estimates produced by the National Atmospheric Deposition Program.

- Page 17, Lines 28-29. The phrase “preferentially selects the development of” makes this phenomenon seem conscious and deliberate. To avoid this, replace that phrase with the word “modifies”.

Thank you, we will do as suggested.

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