Dr. Stephen Long

Editor, Global Change Biology

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Dear Editor Long,

We are re-submitting our manuscript “Grazing enhances belowground carbon allocation, microbial biomass, and soil carbon in a subtropical grassland” following thorough revisions suggested by handling editor and reviewers. We would like to thank both reviewers for their helpful comments and constructive criticism of our manuscript. Overall, we feel that our manuscript has been significantly improved and clarified in the process of revising according to their feedback. Our revisions have focused on strengthening our description of the study site, as prompted by reviewer 2, and linking these expanded descriptions to the discussion of generalizability for our findings. Moreover, we have clarified the specific purpose of our experimental design in isolating long-term consequences of grazing exclusion on belowground carbon allocation and carbon stocks, given the representative pasture management practiced at this study site. We believe that the manuscript is now significantly clearer on all these points. Many small corrections also have been made. Below we provide point-by-point responses to suggested edits and comments.

Thank you for the opportunity to revise our manuscript. Complete point-by-point responses to reviews are found below.

Chris H. Wilson (on behalf of all co-authors)

November 6th 2017

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Handling editor decision: Both referees were positive regarding GCB-17-1217 and had numerous suggestions for improvement. While the comments of referee #1 are relatively minor, referee #2's suggestions include major comments that require considerable attention in order to clarify important uncertainties. Therefore, this version of GCB-17-1217 cannot be accepted but we welcome a revised version that has effectively responded to the referees concerns.    
  
Reviewer(s)' Comments to Author:  
  
Reviewer: 1  
Comments to the Author

1) This is an excellent contribution to the study of mechanisms of grazing impact to soil carbon stocks. Extremely well-written, this paper presents convincing evidence highlighting a pathway for SOC increase under grazing in a subtropical C4 system, something that is currently lacking in the literature. This is a step in the right direction in resolving why grazing has such variable effects across systems and also represents applicable information for use by managers of grazing in C4 systems. A few minor corrections to further improve the manuscript are provided below.

R1) We thank reviewer 1 for the constructive and positive feedback!

2) Line 62: the last bit of this sentence is unclear, likely missing the word 'plant' or similar (i.e. "to assess the relative contribution of microbial versus plant products to total SOC.")

R2) Good catch. We have inserted the word plant as indicated, “We quantified allocation of carbon to root tissue and measured root exudation across grazed and ungrazed plots and quantified lignin phenols to assess the relative contribution of microbial versus plant products to total SOC” (lines 60-63).

3) Line 155: Reference for using the pulse-chase methodology in plants?

R3) Fixed. “We combine an *in situ* 13C-CO2 pulse-chase experiment to resolve patterns of carbon allocation within plants and soil microbes (e.g. Bradford et al. 2012), with soil surveys inside and outside of grazing exclosures to quantify SOC pools and characterize their isotopic and molecular composition.” (Lines 155-158).

4) Line 165-66: This deserves another sentence of explanation. Can you explain further why a disproportionately high mineral-associated SOC fraction might indicate microbial C sequestration to be a driver of variation in SOC response to grazing?

R4) We agree that another sentence of explanation could be helpful and have elaborated our reasoning here by inserting the following sentence, “While the molecular composition of this SOC fraction has not been specifically assessed in Florida, synthesis of data from other systems suggests that mineral-associated SOC is disproportionately of microbial origin (Grandy and Neff 2008, Bradford 2012). As such, it is possible that variations in microbial carbon sequestration could drive variations in mineral-SOC and thus explain overall patterns of SOC responses to grazing.” (lines 165-170).   
  
5) Line 193: use full scientific name for 1st mention of grass species.

R5) We have added the full scientific name such that the first part of the sentence now reads, “MAERC utilizes both planted Bahiagrass (*Paspalum notatum* Fluegge) pastures…” (lines 197-198).   
  
6) Line 206: Can you quantify "sufficient Bahiagrass"? (i..e. estimated % cover or similar)

R6) We thank the reviewer for opportunity to clarify how we made this determination. That sentence now reads “Sufficient Bahiagrass (i.e., at least three spatially distinct patches, separated by at least 1 m, each with area >1 m2) remained in three out of the four exclosures to compare soil under Bahiagrass inside the exclosure to soil under Bahiagrass outside exclosures in grazed patches, and thus test for long-term impacts of grazing exclusion on SOC.” (lines 219-222). The goal was to ensure that a randomly sampled soil core from underneath a Bahiagrass patch represented an independent unit of replication.   
  
7) Line 209: Same here as above. About how much is sufficient?

R7) We have clarified that the percent cover was very high in that exclosure. That sentence is now, “In one exclosure, Bahiagrass cover exceeded 70% and thus permitted a full-scale replicated field experiment to resolve variations in carbon allocation above and belowground using stable isotope 13C pulse-chase methodology (Hafner *et al.*, 2012).” (lines 222-225).   
  
8) Line 220: Include total N = 18 samples? (3 exclosures x 2 treatments (in/out) x 3 sampling locations)

R8) Yes. We have inserted our total N to be absolutely clear here: “To quantify grazing exclusion effects on SOC and soil organic nitrogen (SON) stocks we randomly sampled three locations inside (ungrazed) and outside (grazed) of each exclosure (N = 18) to a depth of 5 cm using a hammer core (AMS 5.08 cm x 15.24 cm Signature SCS Complete/354.26).” (lines 231-234).   
  
9) Line 244: How many pulses per plot? Just the one? So 4 total pulse events for grazed and 4 for ungrazed? Make this info explicit.

R9) We thank the reviewer for the suggestion and opportunity to clarify our experimental protocol in greater detail. We have more thoroughly explained both the total number of pulse events as suggested, as well as how we arranged the order of operations in order to avoid temporal effects confounding our comparison of carbon allocation inside versus outside the exclosure. “Pulse-labeling occurred over a two week period in early September 2016, when each 1- m2 plot was pulse-labeled once. In order to minimize temporal sampling effects, we blocked our pulse events so that pulse labeling occurred for one plot inside and one plot outside the exclosure on the same day, alternating the order in which these events occurred each day. Thus, our comparisons of carbon allocation patterns inside versus outside the grazing exclosure are not influenced by day to day variation in environmental factors (e.g. soil moisture, solar radiation, etc.)” (lines 248-254)  
  
10) Line 250: Is "each replicate" here the same as "each plot" in line 245? Because in line 258, you say "thus, we have 4 replicates inside and outside..." If so, try to use consistent terminology throughout.

R10) We have clarified that in the context of this paragraph, we meant to say that these are 4 independent replicates, one for each plot, for the purpose of combining with the background survey to infer long-term effects of grazing exclusion. But note that we specifically did NOT pool plant or microbial data across dates, which would defeat the purpose of pulse-chase experiment, and thus it is important to distinguish between plots and replicates for the purpose of soil analysis. “At the end of the experiment, all oven-dried soil samples from each plot were pooled, and re-sampled prior to analysis. Thus, for soil analyses we have four independent replicates inside and outside the grazing exclosure (N=8), which we ultimately combine with the background soil survey (N=18) to infer long-term consequences of grazing exclusion.” (lines 288-292).   
  
11) Line 257-58: meaning soil from each collection period (2, 7, 32 days) was aggregated? Or all soil from all harvest dates?

R11) See our response to R10 above. For the purpose of soil analysis (i.e. %C/N, δ13C/δ15N, and lignin phenols) we pooled soil from each plot across all harvest dates, in order to obtain as representative a sample as possible.

12) Lines 271 & 274: Define terms in equations.

R12) We have clarified the terminology in these equations. This section now reads:

“First, we converted all delta values into atom %, as:

|  |  |
| --- | --- |
|  | (4-1) |

where is the measured delta 13C value for a given C pool, and is the 13C/12C ratio of the standard (i.e. VPDB). Then, we multiplied the mass of carbon in a pool (Cpool) by the difference between the atom % in the enriched pool and the atom % of the background, un-enriched pool to estimate 13C label recovered as follows:

|  |  |
| --- | --- |
| 13Clabel = Cpool \*(atom%enriched – atom%­background) | (4-2) |

This metric of ‘13Clabel’ allows us to quantify patterns of carbon allocation within plants and between plants and microbes, and naturally accounts for variation in biomass carbon observed in various pools.” (lines 301-310).

13) Line 287: This equation is not labelled (i.e. 4-3).

R13) Fixed  
  
14) Line 291: Reference equations as labelled above (i.e. 4-1 and 4-2).

R14) We realized that the labeling 4-1 was a typo, and replaced with Eq.1,2,3.   
  
15) Line 385: typo: "estimates" should be "estimated"

R15) Fixed.   
  
16) Line 444-45: Was bulk density actually measured?? If not, where do estimates come from? Not sure if I missed this in the Methods earlier, but this is important to know.

R16) Yes, bulk density was measured. We thank the reviewer for reminding us to provide more information about bulk density across this system of exclosures. The procedure used was more detailed than unusual because we needed to account for significant differences in root-soil structure in grazed versus ungrazed pasture. However, the method ultimately does not quantitatively impact our results so we have opted to present this information in an appendix, and leave the methods and results less cluttered (235-236).   
  
17) Line 451: Remove "as".

R17) We are unsure what the reviewer is referring to since that line does not appear to include the word “as”.

18) Line 459: Need period at sentence end here.

R18) Fixed. “Allocation of carbon to root tissues was positively correlated with exudation (r = 0.82, Fig. 4-2b).” (Lines 479-480).   
  
19) Line 518: Figure 3, missing colon.

R19) Fixed. “Figure 3: Regression of SOC concentration (mg g-1) versus lignin phenol concentration (mg g-1) on a dry soil weight basis. Triangles represent samples collected from individual cores as part of the background survey of three long-term grazing exclosures, circles represent pooled samples from the 1 m2 plots in the pulse-chase experiment.” (560-564).   
  
20) Discussion: Reference figures from Results when discussing them here (i.e. Line 539-540: Figure 3)

R20) We agree with reviewer 1 that where specific reference is made, it is appropriate to include figure reference and have accordingly added that here (line 605 now), and also later in discussion (655) in reference to Figure 2.

Reviewer: 2

21) This MS describes some very interesting and relevant work regarding grazed pasture systems. The MS is generally well organized and written. My major concern is that while the experimental methodology is described in great detail, the agroecosystem in which the work was conducted has received much less emphasis. This casts considerable uncertainty on the results and implications. In addition, the authors may have overstated the significance of their work to pasture systems.

R21) We thank reviewer 2 for their generally positive, and constructively critical review of our manuscript. We have significantly fleshed out our description of the study site and clarified pasture management and relevance to regional pasture systems, and provide additional references for further reading (see responses 22 and 24 below).

In short, this study site is representative of a dominant land-use in south-central Florida, planted pasture. Although heterogeneity among sites is to be expected, we are confident that conclusions from this site should generalize as well as any. However, we have carefully vetted our language throughout and believe that our conclusions are duly qualified (e.g. lines 589-591, 620-625, and 690-691).

22) Study System. Even after a couple of readings, it is unclear what type of agroecosystem is being investigated. A Bahia grass pasture with some secondary succession is not very descriptive. Can actual species composition be presented? What percentage of shoot biomass is made up of Bahia grass or other C4 grasses?

R22) We thank the reviewer for prompting us to include this descriptive information about our study system. Species composition in this system of grazing exclosures was assessed in detail the previous year by our colleague Julia Maki. In grazed pasture, Bahiagrass is dominant overall (86% cover), with a low level presence of sedges, other graminoids, and the occasional forb. Inside the grazing exclosure, Bahiagrass cover in the area over which we randomized plot placement exceed 95%. In lines 215-218 we explain how plant succession has occurred in the grazing exclosures, and then clarify our intention to specifically compare carbon allocation under Bahiagrass inside versus outside the exclosure in lines 222-225. We have added more site-specific detail for where we conducted the pulse chase experiment in lines 238-247:

*“Pulse-chase experiment*

At one site, we randomly located four 1-m2 plots inside and outside the grazing exclosure to conduct a 13C-CO2 pulse-labeling experiment. Inside the exclosure, cover of Bahiagrass exceeded 95% in all plots utilized for pulse labeling, while plant composition was somewhat more mixed in the grazed pasture. A previous detailed plant community composition survey found that Bahiagrass % cover was 86 in the grazed pasture at this site, with a low-level presence of sedges (e.g., *Cyperus* spp.), and other graminoids (Julia Maki, *personal communication*). In turn, this plant composition is very typical of grazed planted pasture at this site (e.g. Arthington et al. 2007, Willcox et al. 2012), and in south-central Florida more generally (Bohlen et al. 2009, Silveira et al 2012, Swain et al. 2016).”

23) The statement that ‘sufficient Bahia grass remained’ raises a yellow flag regarding treatment comparisons (line 209).

R23) This is an insightful observation that we need to clarify. As we now briefly describe in the methods, a very thorough study of plant succession has been made by our colleague Julia Maki. Although those data are not yet published, we anticipate their publication in the near future. In short, each pasture’s exclosure appears to have undergone a unique successional trajectory in the 15-year long exclusion of grazing. Moreover, despite their relatively modest size (10 m X 10 m), there is significant heterogeneity in plant composition inside these exclosures. In making comparisons inside versus outside of the exclosures, we accounted for the uncontrolled nature of this heterogeneity by ensuring spatial independence of soil samples in our background survey (see response 6 above). Additionally, as explained in response 7, we performed the pulse chase experiment at an exclosure with > 70% cover of Bahiagrass, thus enabling real spatial independence of our four 1-m2 plots.

24) The grazing regime outside of the exclosure needs to be described. Research has demonstrated that the timing, frequency and intensity of grazing can induce very different physiological and demographic plant responses. The value of your results is greatly minimized without this critical information. For example, 13C labelling was done only one time in September. How were these allocation patterns influenced by the phenological growth stages between the two treatments? How closely was labelling done relative to grazing? Allocation to roots may resume within several days of intensive grazing or be unaffected by moderate to slight grazing.

R24) We agree with reviewer 2 that the grazing management at the site of our pulse-chase experiment could have influenced our results and thus deserves further explanation. Overall, pasture management was typical for the season and pasture type (planted pasture) and the grazing management style has been described in previous publications, e.g. Arthington et al. 2012, Bohlen et al. 2009, Swain et al. 2016.

Accordingly, we have more completely described grazing management at the study site in lines 207-215:

“To infer the impacts of grazing, we analyzed plant, microbial and soil responses to grazing exclusion across long-term 10 m x 10 m grazing exclosures at MAERC. Grazing management in the pastures surrounding the grazing exclosures is representative of the ranch as a whole (e.g. Arthington et al. 2007, Swain et al. 2016): moderate-density mostly continuous grazing during the summer growing season, and occasional utilization during the winter dormant period. Overall grazing intensity on the planted pastures is moderately high, about 1.5 acres/animal-unit (Gene Lollis, ranch manager, *personal communication*), but well within standard commercial ranges for south-central Florida (Silveira et al. 2012)”

During initiation of the pulse-chase experiment, plots were located in grazed pasture that had been previously grazed that season, but had a minimum of 10 cm of forage regrowth (thus had not been grazed for at least a couple of weeks). Our goal was to emphasize long-term comparison of grazed pasture with long-term ungrazed pasture, rather than study the short-term effects of defoliation (as in e.g. Hamilton and Frank 2001). Accordingly, we have added the following explanation on lines 254-257:

“Moreover, since our goal was specifically to study the long-term consequences of grazing exclusion on carbon allocation, rather than short-term impacts (e.g. Hamilton and Frank 2001), we located plots on patches that had been grazed earlier in the season, but that had a minimum of 10 cm forage regrowth.”

We agree with reviewer 2 that seasonal variations in allocation could be important and is worthy of future study. Logistical constraints precluded measurements over multiple seasons in our study but given the much larger root biomass (and root C allocation) in the grazed pasture, we would expect the grazing exclosure treatment effect to be even larger than what we report here, rather than smaller. Thus, although we look forward to future work on seasonal allocation patterns in this system, its importance will be more for accurately constraining ecosystem carbon budgets, and is exceedingly unlikely to impact the qualitative comparison of long-term ungrazed versus grazed pasture.

25) Results Interpretation   
The two carbon allocation pathways provide an effective presentation, but I was uncertain in which pathway mortality and senescence of fine roots would be located. I assume that some fine root mortality may have occurred in a 30 day period.

R25) We appreciate the reviewer’s positive view of our two pathways framing.

As we write in lines 176-184: “Therefore, we speculated that grazing-induced changes in this pathway of labile carbon input could lead ultimately to greater accumulation of microbial necromass in these soils, thus promoting SOC accumulation over the long term (hereafter the “exudate-microbe pathway”). By contrast, grazing effects on SOC may be best accounted for by impacts on fine root biomass and turnover, given the substantial evidence that root litter inputs are conserved in SOC more effectively than shoot litter (Rasse *et al.*, 2005). Thus, if grazing promotes root system production, hence root litter deposition, we hypothesized that this effect would be associated with larger SOC pools, and vice versa (the “root litter pathway”).”

Therefore, mortality and senescence of fine roots is located in the “root litter pathway”. We agree that a very small amount of senescence may have occurred in the 30-day period of our pulse-chase experiments. However, we account for this by utilizing the microbial enrichment at 2 days to estimate the relative strengths of the exudate-microbe pathway (lines 336-340), thus eliminating root senescence as a possibile contributer to observed microbial enrichment Additionally, as seen in Fig.1e,f there is no evidence from isotopic signature of significant turnover occurring within the timeframe we sampled (0-32 days).   
  
26) Consider whether the results can be integrated further to help the reader interpret them. For example, if there are fewer roots in the ungrazed pasture would you anticipate that 13C enrichment should be higher? I assume that higher enrichment implies a strong C sink in that specific plant organ. A clear indication of these relationship would be very useful.

R26) We thank the reviewer for prompting us to clarify the interpretation of sink strength in the context of this type of pulse-chase experiment. We have accordingly strengthened our reference to this concept in the methods section:

“This metric of ‘13Clabel’ allows us to quantify patterns of carbon allocation within plants, and between plants and microbes, and naturally accounts for variation in biomass carbon observed in various pools. By contrast, only examining isotopic enrichment (i.e. 13C) would not allow us to estimate the “sink strength” of various biomass compartments, since a small enrichment over a large biomass can represent a larger sink than a large enrichment over a small biomass.” (Lines 308-313).

Moreover, we tie this concept back together in the results in lines 462-465:

“Although root δ13C values were not significantly different between exclosure and grazed plots (Fig. 4-1e), total label 13C enrichment was seven-fold lower in excluded plots compared to grazed plots (Fig. 4-1f, Table 1). Thus, root systems were a significantly stronger carbon sink in grazed pasture compared to ungrazed pasture.”

Discussion and Implications  
27) The sandy nature of the soils and the unique environmental conditions (e.g., water logging) may require that you qualify your results relative to tropical pastures throughout the globe.

R27) We appreciate that the soil texture certainly is a necessary qualification of our results. Accordingly, we have strengthened our discussion of this issue in paragraph 5 of the discussion. Specifically, lines 672-676 read:

“We speculate that it is possible that particulate forms of SOC are promoted by grazing, while the absolute concentration of mineral-associated C is similar due to saturation of physical-protection capacity in coarse sandy soils. Thus, given a larger capacity to stabilize microbial necromass, grazing may be associated with an even greater SOC stock (i.e. enhancing the ‘microbial carbon pump’ Liang *et al.*, 2017).”

Moreover, we are careful to avoid sweeping global conclusions. For instance, the last sentence of the first paragraph of discussion says “Taken together, these results demonstrate that grazing C4 pasture grasses

*can* (emphasis added) promote root system production and turnover and thereby higher SOC/SON stocks.”

Despite these considerations, our findings are consistent with the results of the meta-analytical review by McSherry and Ritchie (2013), who found that grazing promotes SOC especially in C4 systems receiving high levels of precipitation. However, since their review identified the subtropics as particularly deficient in data, our results are an extremely valuable addition to the literature. We clarify this connection with a new sentence in lines 610-614:

“Our finding that grazing is associated with higher SOC stocks is consistent with the pattern noted by McSherry and Ritchie (2013), whereby grazing may especially promote SOC in C4 grasslands receiving higher precipitation. However, no subtropical sites were included in their survey, and thus it is encouraging that our results support this general pattern, suggesting a common underlying mechanism.”

Likewise, lines 620-626 qualify our findings in light of soil properties of this particular system:

“Given the generally accepted relationship between particle size fraction distribution and capacity to physically stabilize labile organic molecules such as microbial products (Grandy & Neff, 2008; Wieder *et al.*, 2014; Kallenbach *et al.*, 2015), it would stand to reason that our coarse-textured soils (>97% sand) should contain little stabilized microbial necromass, and that plant inputs should be predominantly represented in the SOC stock.”

28) Might some interpretation of root:shoot allocation patterns be appropriate? You mention the R:S ratios but do not discuss them. A R:S of 1:1 seems highly unusual for grasses. However, I generally work with more arid systems.

R28) The reviewer correctly notes that there is a very high R:S ratio (around 1:1) for the pasture found **inside the grazing exclosure:** “Thus, overall root:shoot ratios were lower under grazing exclusion (1.2 inside exclosure compared to 5.3 outside exclosure)” (Lines 460-461). Thus, under grazing, the R:S ratio is much higher.

We have strengthened our discussion of this subject in lines 638-646:

“Grazing exclusion dramatically altered plant carbon allocation in this system. Grazed plots allocated over 5X the C into root systems, and sustained a standing biomass stock more than 3X as large as excluded plots, resulting in a 4-fold amplification of root:shoot ratios, from a relatively low value around 1 in the absence of grazing, to a more typical value around 5 given grazing. Given these shifts in root:shoot ratios, it appears that in the absence of grazing, this pasture community exhibits phenotypic shifts consistent with greater competition for light (e.g., larger leaves, wider plant spacing), and lower requirement for rapid mobilization of nutrients through fine root system (Milchunas *et al.*, 1988).”  
  
29) C4 grasses are often considered to be mycorrhizal obligates relative to C3 grasses. I can’t help but wonder if this effect exudation patterns. These hyphal networks are critical to nutrient absorption.

R29) We agree that the mycorrhizal angle is potentially important.It is interesting to speculate whether increased mycorrhizal associations play a role in aiding nutrient access to sustain rapid re-growth post-grazing in this system. Moreover, hyphal turnover of soluble carbon and nutrients is certainly an important component of exudation, and is thankfully captured by our pulse-chase methodology, which effectively indexes net root-mycorrhizal carbon flow. Unfortunately, it is very difficult to partition exudation from mycorrhizae separate from fine roots, although we welcome further study in this area, and plan to consider this angle in future research.

30) The sentence on line 583 seems to contradict the previous conclusion that most SOC was of root origin.

R30) We realize that this sentence was a bit confusing. Thus, we have re-worded this section to emphasize our intention, which was to point out that the lignin data, while demonstrating a compelling and strong relationship with overall SOC, do not suggest that plant-tissue is the ONLY source of SOC in this system. Rather, the data suggest that microbial contributions to SOC are likely to be highly collinear with plant tissue. This pattern is also consistent with the very tight relationship we observed between root biomass and estimated root exudate flux. We have accordingly re-written this part of the paragraph to read:

“However, soil lignin concentrations were less than half the concentration observed in plant source pools in both treatments. Theoretically, if all SOC were from plant tissue, the proportion of lignin would increase in SOC relative to source pools, because the lignin fraction is the most recalcitrant and selectively preserved pool of plant tissue during decomposition (Six *et al.*, 2002; Thevenot *et al.*, 2010). Thus, these data suggest a large mass of non-plant-sourced (i.e., microbial) C is contributing to SOC. Whether this microbial carbon originates from root exudate flux (Bradford *et al.*, 2012), or from the microbial decomposition of plant tissues (Grandy & Neff, 2008) is unclear. However, based on both our lignin data, and on the compelling relationship between fine root biomass and estimated exudation, both sources of microbial carbon will be highly collinear with observed root biomass.” (lines 627-637).

31) mass is unlikely to be a metric grazing managers are likely to reference. Consider linking this

R31) Unfortunately, it looks like we do not have a complete comment from the reviewer here. However, we would note that although measuring root biomass may not be standard practice for grazing management at this point in time, it is clearly much easier than having to estimate processes and patterns such as root exudation or microbial efficiency. If any kind of routine soil monitoring (e.g. for SOC/SON, cations, pH) is being conducted, it would not be particularly difficult to include biomass of rhizomes and roots collected from the same cores.   
  
32) Is the paragraph beginning on line 614 largely speculation?

R32) The purpose of this paragraph largely was to suggest future lines of research based on our results. We have clarified the somewhat speculative nature of these suggestions in lines 672-674. “We speculate that it is possible that particulate forms of SOC are promoted by grazing, while the absolute

concentration of mineral-associated C is similar due to saturation of physical-protection capacity in coarse sandy soils.”

33) You may wish to reconsider the ‘profound stewardship implications’ on line 633, especially given the considerations regarding the grazing regime and agroecosystem stated above. Fine root result to specific grazing patterns if possible.

R33) We agree with reviewer 2 that the results of this study need to be duly qualified before making general recommendations. We have detailed our modifications and qualification in our response 27 above. In short, we believe that while site-specific factors inform and constrain the mechanism by which grazing impacts SOC (i.e. the root-litter pathway versus exudate-microbial pathway), the linkage between belowground biomass and SOC accumulation is positive, regardless of pathway. Thus, we have re-worded this section to say “potentially significant implications…” (line 685), and emphasize the conditionality of our conclusions in 691-694, “At least for coarse-textured soils under C4 pasture, we suggest that management of SOC stocks ultimately devolves into a relatively straightforward task of optimizing root system health and proliferation.”

As for agroecosystem relevance, as noted in the ‘study site’ section of the methods, and in our overview of the field experiments (206-229), this site and its grazing management are reasonably typical of south-central Florida, which contains regionally significant grazing lands (e.g. Silveira et al. 2011), as we explain in lines 193-195,“MAERC operates a full-scale commercial cow-calf ranch (~3000 head) to study ecological aspects of cattle ranching, a dominant land use in the Northern Everglades watershed (Bohlen *et al.*, 2009; Silveira *et al.*, 2011).” In fact, the goal of management at Buck Island Ranch is to emulate commercial cow-calf ranching in the region to facilitate ecological research on its impacts (Bohlen et al. 2009, Swain et al. 2016). As we explain in R24, our goal was not to study the short-term impact of any specific grazing pattern, but rather to examine long-term consequences of grazing exclusion given typical south-central Florida pasture management. We hope that our modifications have sufficiently clarified that point.