Anonymous Referee #1 Received and published: 4 March 2020

The study has addressed potential 'Management-intensive grazing' (MiG) effects on soil health represented by 11 soil variables. Results show significant positive changes in soil biological properties after the land use change (from cropland-to-grassland) and negative effects on soil bulk density and soil P availability. The authors conclude that MiG could have positive benefits for soil health and environmental and economic sustainability. Although the results of this study are interesting (increases in microbial biomass and extra-cellular enzyme activity, decreases in bulk density), it is very difficult to state that MiG is actually responsible for these changes. This is because: - The study measured these variables only 1 and 2 years after soils had been under cropping for at least 10 years. Thus changes in soil variables could be simply due to a change in land use from cropland-to-grassland and/or to water irrigation (and thus to increased soil moisture) and not necessarily to MiG. - The impact of MiG was tested only between August 2017 (when cows were firstly introduced to paddocks) to May 2018 (when soil samples were taken for the last time). Thus comparing changes in soil properties between 2017 and 2018 when grazing was applied only between August and October 2017 leaves lots of uncertainty about potential grazing effects on soil health. Most of these changes could be simply related to the land use change and the establishment of a grassland ecosystem. To address MiG effects on soil health data from at least few years should be collected and MiG should be compared to a different permanent grassland system perhaps not irrigated. The fact that the grass swards did not establish well due failure of legume growth 2017 and 'hostile' climatic conditions between 2017 and 2018 adds more uncertainty to net effects of grazing on soil health. I would collect data from more years to provide evidence that MiG is benefiting soil health, which could well be the case.

Authors Reply: First, we appreciate this reviewer's comments with respect to the issues with such a short study. We agree that collecting more data would be best. Unfortunately we only received two years of funding for this project and don't have the financial support to continue at this moment in time. We are attempting to find additional funding to continue this work. We would state that the comment of collecting more data likely could be applied to the majority of published field studies, since most field studies are simply not long-term. Our study also falls within that realm.

We also agree that the changes may have been due to converting the system from a cropping to a pasture system. BUT, this is why we were careful with our wording in the manuscript. The reviewer even pointed this out above that: "MiG **could** have positive benefits for soil health and environmental and economic sustainability". Again, our careful word selection so as to not be exact with our thought process as to why these processes were occurring.

Lastly, we can tell you that others have found similar, short-term results with MiG in different ecosystems. If you visit this article: https://onpasture.com/2016/06/20/does-management-intensive-grazing-grow-more-better-quality-forage/ that describes MiG research from University of Madison (Wisconsin, USA), the researchers found that MiG soils lost less C than other treatments, at least in one year of their two year study. They also found no change in soil microbial activity over their short-term study when comparing treatments to MiG. Furthermore, the authors found greater net N mineralization with MiG over ungrazed systems, in their short-term (i.e., two-year) study. It appears that there is an

associated, peer-reviewed article from these researchers, found here: https://www.onpasture.com/wp-content/uploads/2016/01/Management_IntensiveRotationalGrazing.pdf We, unfortunately, did not cite this work in the original manuscript. If possible, we will work this into the revised manuscript in key locations, and will point out those locations using track changes.

Specific comments

Lines32-34: I would rephrase the last sentence, which at the moment seems to consider only MiG positive effects on soil biology but not negative MiG effects on soil P availability and soil structure (increased compaction). Given these contrasting results one could argue why the authors conclude that MiG is 'promoting soil health for environmental and economic sustainability'.

Authors Reply: Agreed and rewritten as: *If managed correctly, compaction and soil P issues could be avoided, with MiG systems having potential success in supporting grazing while promoting soil health for environmental and economic sustainability.*

Line 40: after 'benefits' needs literature reference(s).

Authors Reply: We added a citation.

Lines 46-49: This sentence needs rephrasing to introduce the definition/concept of 'soil health' supported by literature references.

Authors Reply: Rewording and added references, including one of our own to show that we have been working on soil health as well.

Lines 56-60: These lines would need to be rewritten to explain better the role of extracellular enzymes in soils. In particular, microbes produce extra cellular enzymes to acquire C or nutrients from SOM and as a consequence affect C, N dynamics.

Authors Reply: By rewriting these lines, it drives the discussion away from the point we are trying to make, which is that beta-glucosidase is used as a means of (partially) quantifying soil microbial health in the framework we utilized for this study. We did not make a change to this sentence.

Line 93: Needs to explain/summarize why grazing has positive effects on soil 'health'.

Authors Reply: This statement was indirectly referring to the aforementioned information above. We clarified the sentence.

Lines 112: I am not sure that hypothesis two is well supported. For example, microbial biomass C and enzyme activities can be stimulated by animal waste more than lack of tillage? Need to better support this hypothesis in the Introduction.

Authors Reply: We actually feel that we did a nice, concise job of writing the supporting information in the introduction. We focused an entire paragraph on this hypothesis. In the original manuscript, see lines 50 to 65. In the revised manuscript, see lines 56 to 74.

Lines 136-139: Perhaps few lines explaining the rationale behind species assemblages (e.g. simple vs complex) would be useful.

Authors Reply: Good suggestion. Information was added here.

Line 174: I think this is August 2017 and not 2018

Authors Reply: In the original manuscript, Line 174 (revised manuscript, line 185), the verbiage is correct. On August 18 (2017). However, we added "2017" after August 18 to be clearer.

Lines 210: A significant paper problem associated with the robustness of the dataset assembled under this project is that potential grazing effects on soil health have been addressed only after one grazing season between August 2017 (when cows were firstly introduced to paddocks) to May 2018 (when soil samples were taken for the second time). This is quite a short period of time to address potential grazing effects on multiple soil parameters given that cows grazed only between August and October 2017 on swards which had not established well (legumes were almost absent from swards), the soils were cropped until 2016 and climatic conditions were quite variable and 'hostile' at 1500 m asl. If more soils had been collected in 2019 (after at least two grazing seasons) this would have been better and provided more data to compare. . .

Authors Reply: See our reply to this author's first comment, above.

Lines 236-245: It is not clear how the SMAF works, how field measurements are 'transformed' in indexes through SMAF. More details need to be given here.

Authors Reply: Information has been added, and the reader is directed to Andrews et al. (2004) for additional details.

Lines 246-253: Statistical analyses are not properly described, clear description of independent and depend variables is not given. 'Years' is the variable indicator of potential grazing effects on soil parameters?

Authors Reply: The statistical description seems clear to us. We used a linear mixed effects model as suggested by our university's statistician, with comparisons made between the various environmental factors and indictors as listed on: a) in the original manuscript, lines 251-253; or b) in the revised manuscript, lines 264-267. No change was performed.

Lines 284-285: The fact that BG activity increases it could be due (as the authors suggest) to land use change from cropland to pasture. This could occur however following any cropland-to-grassland land use change and not necessarily only to MiG on irrigated grasslands. Also there is a problem with 'perennial' pasture because the grassland in this study is only 1 year old.

Authors Reply: Again, see our reply to the reviewer's first comment above. We do, however, back up this claim that the observed change may have been due to land use change by citing other works within this paragraph.

We clarified that the perennial pasture was only 1 year old in the text in these lines.

Lines 296-300: Increases in microbial biomass, again, could be related to the land use change and not necessarily to MiG. To test for the effects of MiG on multiple soil parameters, data should be collected

at least for few years and also compared to permanent grasslands, which are not irrigated for example. Other variables such as soil N mineralization could have increased because of greater soil moisture due to irrigation and not necessarily to MiG. It is not surprising that soil C has not changed because of the short period of time considered (2017-2018) (line 372) It is actually surprising that soil P availability decreased (and not increased) under the MiG system with more cattle dung and urine being returned to soils. I think this could be due to the fact that most P has been retained by soils and partly perhaps because more P was uptaken by plants. This however shows the difficulty to interpret these results after only a short grazing season had occurred on these newly established grasslands.

Authors Reply: We agree with the reviewer that this observation could have been due to the recent land use change. Thus, we added a sentence that states that this could have been due to land use change (see lines 312-313 in the revised manuscript). This sentence still leads the reader into the remainder of the discussion that we had originally written.

The reviewer is still concerned about this being a short-term study based on the above comment, and we again refer the editor to our reply to the reviewer's first comment above.