

Interactive comment on “Effect of grassland cutting frequency on soil carbon storage – A case study on public lawns in three Swedish cities” by C. Poeplau et al.

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The article titled: Effect of grassland cutting frequency on soil carbon storage- A case study on public lawns in three Swedish cities has essential prerequisite to be published in SOIL. In addition, this article displays the necessity to research the different sustainable management practices in grassland in relation with C and N stocks. Since the most of grassland studies have been assessed in financial term but sometimes the functions of soil like C and N storage had been forgotten. In addition, Poeplau et al. include public areas like experimental sites demonstrating the originality and functionality of this study. Nowadays, is important consider too the urban public areas from the different countries because they have environmental properties which are contributing

C1

in the soil C and N storage. Although the part of material and methods, the authors considered in the line 172: “basic assumption was that the underlying pedology and initial soil carbon stocks were similar...” I think would be necessary at least do a mention about the soil types in every study site because is essential when you work with soil.

We thank the reviewer for the very positive review. Unfortunately, the soil type has not been determined in this study, since most of the field work was conducted by technicians and students. However, regarding soil carbon sequestration, the measured parameters such as soil texture and pH should have a more important influence than soil type.

On the other hand, I believe that in the line 210-211 the correlation between SOC stock and clay content is down. Is possible that the different studied site has different clay mineralogy?. In a recent study from Han et al., 2015 showed that in the most of cases the clay mineralogy is a better control factors in the content and stabilization SOC than clay content. Or maybe there are other soil related parameters?

Unfortunately, clay mineralogy is not either available. As mentioned above, we have used soil texture and pH, as well as climate variables as explanatory variables. No other parameters are available. Furthermore, we detected a mistake regarding the clay effect: In the first review of the manuscript, the editor commented that we should test whether the differences in clay content were driving the observed SOC stock differences, which we have done. During this analysis (in the previous version), we also found that average clay content (of the site) was correlated to SOC stock difference ($R^2=0.26$) and included it in the manuscript. However, before it did show in the generalized linear model, which we however did not question at that stage. So, clay content (as explanatory variable in the model) was not significant. We have changed the sentence accordingly, which now reads: “Although clay content did not improve the model fit of the generalized linear model, difference in SOC stock did also increase with average clay content ($R^2=0.26$, ns).”

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Is possible in the future the implementation of new grassland management practices like reduced tillage (1 time a year) together with a more cut times to incorporate part of the organic matter and so decrease the N₂O formation? or other management practices?

In urban systems, grassland renovation tillage is obviously not a practical option, since this would destroy the lawn. Also, such renovation tillage has been found to increase N₂O emissions (short-term) due to the rapid mineralization of organic matter and thus release of nitrogen and shortage of oxygen (e.g. Velthof 2009, Nutrient cycling in agroecosystems). But those studies are mostly not conducted in mulched systems, but in harvested systems. Therefore, the effect of tillage in such a mulched system as investigated here is highly uncertain, but might in fact be positive for SOC stocks (due to the higher stabilization of clippings). However, as mentioned in the next comment, the discussion is already quite long as compared to the results section, therefore we decided not to enlarge it with some speculations on how (in agricultural systems) the greenhouse gas budget of managed grasslands can be further improved.

Finally, I also think that the part about results is very short related to the discussion part.

We agree, but we do not see why this should be a problem here. We consider the result section to be complete.

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