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Discussions

Interactive comment on "Long-term nutrient fertilization and the carbon balance of permanent grassland: any evidence for sustainable intensification?" *by* Dario A. Fornara et al.

Anonymous Referee #2

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The manuscript "Long-term nutrient fertilization and the carbon balance of permanent grassland: any evidence for sustainable intensification? " analyses changes in top soil C stock of 43 years of data from a permanent grassland experiment on organic fertilizer amendment (cattle and pig slurry in different application rates). The manuscript assess key questions such as : how long-term inorganic vs. organic fertilization influences soil C stocks, and how soil C gains (or losses) contribute to the long-term C balance of managed grasslands. The manuscript and data set is interesting and worth to be published. Furthermore the outcomes may give further insight of the effect of C amendment on soil C sequestration of managed grasslands and their latter role to compensate non CO2-farm emissions. However, from the present version needs some

clarification (details on plot experiment) on serval points (see general comments) and I also encourage authors to look on the data set from different angle: e.g. 2D plot on yield vs. soil C changes (N vs soil C changes) and eventually a 3D (multiple regression) with yield /soil C changes/ N inputs , as this may give further information on slurry amendment thresholds with repect to yield and GHG emissions. .... Accordingly, I suggest to have (majors) revision before publication

General comments

Introduction I am not sure if authors wanted to avoid the subject or other, but I recommend to cite/mention at some point the equilibrium idea of soil C stocks under grasslands under constant management (Smith et al 2015) (see P2L10-15ff)... authors have mentioned bit and pieces "....it is not clear, however how soil C sequestration will ultimately contribute to the net C balance of intensively managed grasslands.... " ; but I think they can even say that more and more land was used for agriculture in the past 100 year and management was improved over time and barely any management grassland has been managed in the same manner for more than 30yrs.

M&M For reader it′s quite difficult to estimate the role of treatments to soil C changes without the necessary information on C_input and N_input (and water?, soil texture??) from slurry and mineral fertiliser. I suggest to add at least a bit more information on the fertilization amendments to the text (P3L14ff), as the volume applied is very vague for slurry and for non farmers the differences between pig and cattle slurry and NPK is difficult to see. Accordingly parts of Tab1-discussion should go to M&M: I had a table in mind like this

Concerning the estimates of soil C stock, personally I found the soil corer of 3cm diameter a bit small to capture spatial heterogeneity. Thus, I missed some information on replicates within a plot, if there were any. Estimates of soil C stock, the description in M&M is a bit shallow. Did authors take into account changes on bulk density over time? (See Stahl et al 2015 but also Ellert et al 2002 SSSAJ) As the soil may have get compacted or eroded over the 43 years and soil stock analyses should account for that. Accordingly authors should apply a mean BD per layer for all years .... Needs to be clarified

Results/Discussion In the section C retention, I suggest to add some estimates on C_outputs vs. C inputs. This will give an idea if slurry amendment (C inputs) could compensated C outputs (removed biomass), and if there is a threshold. For example Cinputs from cattle slurry would approximately correspond to 2 to 8.4 Mg biomass/ha.yr which is half of the harvested biomass (19Mg DM) of cattle(H) and a 10th of the biomass for pig slurry. In the context would be interesting to see 'Slurry-C retention coefficients' (ie C sequestered / C added as slurry) in the context of C removed (output) from the plot. Besides, this joins my early comment on more information of the N content of the slurry treatments. As there is another point, given that there is little information on N_input reader wonders how biomass production can be similar between cattle and pig slurry? Accordingly , to reader doesn't get evident why authors discussion N_compounds in slurry (eg P8L1-10 may be further developed). Along the same lines, would be nice to see a plot on Yield vs. soil C changes (N vs soil C changes) and eventually a 3D with Yield /soil C changes/ N input . This would give further information on slurry amendment thresholds. And help to provide evidence why pig slurry contributes less to soil C changes (eg P8L13ff). Another 3D (or multiple regression test) to test, may be the yield vs. root biomass vs. soil C changes. As there is a coupled effect (at least what I can interpret from the presented data!). Pig slurry has less C input and higher N_input than cattle slurry (eg Fig 2a and 4b). For control and NPK Ninputs is low and moderate, while C outputs are low and high, respectively Another point the efficiency of the plots yield/Ninput may give and idea on the C sequestration and root growth (i.e. P7L1). The higher the N_input is and the higher there a C outputs the less C is sequestered by plant (and root) litter. This may be compensated by C inputs from slurry! (ie P8L24 may be further developed). Accordingly, the best fertilizer is cattle>pig> ??? either control or NPK (zero C-inputs).(Necessary information is missing for NPK). Suggest to develop a bit further discussion section (P8L25, 30 ff), as there

is little (incomplete) discussion comparing slurry with control and NPK. Nonetheless, difficult to draw general conclusions with top soil analyses. As >30cm fraction may behave differently and loose C over time (eg P9L1ff). How do authors consider the deeper soil layers? Supplementary material Fig 4 and 5 nicely show that in more lower layers (10-15 and 15-30cm) soil C stock is affected by other mechanisms than C and N (in-/out)puts than top soil layer . What authors may add here is the transport of C to deeper soil layers (Rumpel et al.. Kögel-Knabner et al) . . . which may event further increasing the soil C changes.

LCA, though I understand the aim of authors to highlight the importance of soil C sequestration provided by grasslands and the possibility to compensate for non CO2-emissions (see also Soussana et al 2010). I found this part is little developed in the manuscript (mostly in SupplMat). So not sure if there is an added value in the present version. There is also a need for clarification as authors did not mentation the grassland use and how estimates were done. Or did authors only account for cut grassland. (Needs to be clarified) . For ever estimate one would need animal stocking rates /ha. Grazing /barn period to estimate partition between manure and dejection emissions in the field, etc. The same for supplements of concentrates to diet »>Accordingly for reader doesn't get clear what is the part of each of the mentioned emissions sources : (3) enteric fermentation of ruminant livestock, (4) manure management (CH 4), (5) manure management (N 2 O-N), (6) managed soils (CH 4), (7) managed soils (N 2 O-N), (8) milk yields, (9) beef yields, (10) feed concentrate production and transportation, (11) fertilizer production, and (12) machinery use. Anyhow to improve the section and readers attention I suggest to add GHG emissions in in Fig 6 in order to better capture the "offset idea".

Specific comments

Table 1 add NPK and control

P2L15 guess you have to cite Smith et al 2015 at some point Authors may even say that more and more land was used for agriculture in the past 100 year and management was improved over time

P2L20 authors may even cite Aneja et al 2008 showing the fertilizer procution and consumption since the 60s P3L14ff suggest to add a bit more information of the fertilization amendments to the text, as the volume applied is very vague for slurry. Suggest to add at least some proxies of N and C to the L, M, H amendements.

P4L12 suggest to precise how estimates dealt with grassland use (grazing/cut)

P6L38 what is the yield of NPK??? Should be in between slurry and control??!!

P7L 22ff suggest to better clarifies the sentences otherwise the conclusion can become mis leading The application of high rates of cattle slurry (i.e. XX % of Cinputs Nfertilisation) significantly contributed to increase soil C sequestration up to 0.86 Mg C ha -1 yr -1 (86 g C m -2 yr - 1), whereas the application of pig slurry and inorganic NPK fertilizer with lower or non C input did not have any significant C sequestration benefit when compared to unfertilized (control) soils.

L26 ff idem to above "soils may can act as C sinks but when C losses from frequent grass mowing are compensated primary productivity (ie results of N fertilization) and organic C inputs (i.e. slurry)

P7L32 "Our results provide evidence that, under comparable biomass production, cattle slurry applications are more effective than pig slurry applications in terms of top soil C accumulation. This this positive effect could have multiple explanations.

P8L2 "We did not find any significant difference in total N% (2.9 $\pm$ 0.9 vs. 3.1 $\pm$ 0.7) or total C% (32.3 $\pm$ 2.8 vs. 34.8 $\pm$ 3.7)" suggest to move information also to M&M section

P2L31ff. "...As opposite in the control plots which do not receive any C and N addition, soil C accumulation may be partly explained by low C outputs via harvests and higher root mass production (possibly resulting from positive grass-legume interactions), which contributes to higher C inputs to soils when compared to the reduced root

systems of fertilized slurry plots (see Fig. 5b).

P9L20 "...to beef management simply because dairy cattle are associated with higher CH4 emissions from enteric fermentation and manure management." Suppose that dairy cattle has also higher emissions due to the fact that for milking they stay more in the barn and less on the paddock as beef cows. IE barn emissions (manure, etc) are higher for dairy cows

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