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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.

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Review of: Long-term nutrient fertilization and the carbon balance of permanent grassland: any evidence for sustainable intensification? D. A. Fornara et al.

This paper examines changes in soil C and N in cut pasture systems following application of different pig and cow manures. Control and NPK fertiliser treatments were included. Soil samples were collected to 15 cm. There are few long term trials of this nature and they can provide very valuable information. Like many of these trials, it is not likely that they were primarily established to determine changes in total C stocks and so there can be inevitable short comings. Here the relatively shallow sampling and few bulk density measurements might be criticised. However, I believe it is better to

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make the best of the rather unique data that is available for interpretation.

Very interestingly, this study finds increases in soil C and N over 43 years for all treatments including the control (no fertiliser treatment) and the accumulation rates are substantial, as great as 0.86 Mg/ha/y. Establishment of a new carbon stock is reasonably well accepted when there is a major change in land use and management.

The major questions in my mind are: âĂć Why the establishment to a new C stock equilibrium is taking so long particularly in the control treatment (still gaining 0.35 Mg/ha/y) which presumably has been under pasture for some time? âĂć Was there really a difference in C accumulation rate between pig and cattle slurry given these were largely applied at different rates (cattle rates were higher or the same than pig rates). Expanded below. Specific points

1. In the site description, the authors state the site was established in 1970 on an existing sward of ryegrass clover. If it has been in pasture for decades previously but still increasing in soil C stocks this would seem very odd. Was the site cropped in the past and so still recovering from previous C loss? I understand that getting previous land use can be difficult but this is important as the increase in soil C is up to 20% of initial stock in the control soils in ∼40 years (gain of 13 Mg from a base of about 59 Mg).

2. Figure 2b. All the replicates are plotted which presumably gives the tight error bounds, is this reasonable? Error band are not defined in legend.

3. Figure 2b. I am not sure that the authors can assume a linear fit – to me a broken stick model could be fitted that is essentially flat to inputs of about 1.2 Mg C/y and then increases. If correct this could simplify interpretation of why pig and cattle slurry gave different responses. A broken stick model or similar curve would suggest that the first C load of added C is mineralised and the remainder is available for stabilisation. This is an important distinction, the current figure could be interpreted to mean that any addition of external C will build soil C but a broken stick model (or similar model) would

argue that there is a threshold load that is needed before C accumulates. I would have thought the authors need to defend the linear fit. This curve is strongly dependent on the two high C loading from cattle and so the discussion about differences in composition of cattle and pig manure leading to different carbon accumulation (first three paragraphs of the discussion) might more easily be explained by a lower loading rate of pig slurry relative to cattle slurry.

4. The highest pig slurry loading was 1.11 Mg C/ha/y in comparison to lowest cattle slurry application of 0.92 Mg/ha/y both of which had standard errors of about 0.1 (Table 1 – I think these are SE - not stated). Are these significantly different loads? The relative soil C stock changes of 1.05 for pig slurry and 1.09 for cattle slurry with SE of 0.04 and 0.03, respectively (table 1). So for the same slurry load from pigs and cattle gave same amount of C accumulation and no need to try to justify a difference between cattle and pig slurry? Looking at fig 2b the slightly higher C accumulation for the cattle slurry at 0.92 Mg C/ha/y inputs is driven by one high point – the other three points fit within the scatter of the slurry C input of 1.11 Mg C/ha/y. It is important to be clear about this otherwise the reader might conclude that there were indeed differences in C accumulation for cattle and pig slurry when I think this is hardtop justify

5. Figures in general need attention – superscripting missing in some (e.g., fig 4a) and not used in some places (e.g., xaxis fig 2b), different fonts e.g., figure 6. Figure titles don't state what error bars are. Figure 1 what is dashed line – an overall linear fit?

6. Pg 3 (ln 34-36) %C was measured using LECO and then also by muffle furnace – please explain why two different measurements approaches were used– is this to get at inorganic C?

7. Pg 5 ln 7-10. Was the accumulation rate from the control subtracted before calculating Slurry retention factor? I see this is stated in table 1 but missed it in data analysis section.

8. Pg 5 ln 7-10. I guess this assumes that the extra stored C comes only from the slurry

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but there was increased plant production also and this has potential to be stabilised in soils also? How was this accounted for? Pg 6 ln 19 states that 16% of slurry C was accumulated in soils but does this ignore the extra pasture production and inputs? But pg 7 ln 36 states 14-15% retention.

9. Pg 7 ln 10 rounding error? should be 10 Mg?

10. Pg 8 ln 31 – I do not understand sentence starting "As opposite . . .

11. Conclusions – I suggest need to tone down the statement "Our findings suggest that permanent grasslands act as a sink rather than source. . ." this may well be true for the current study at present but other have in some cases found losses of C from pasture soils: Schipper et al. (2014), (Meersmans et al., 2009, van Wesemael et al., 2010) for specific soil types. I also think that reference should be made to saturation likely occurring at some stage even with ongoing manure inputs – I think this has been well demonstrated for some of the long-term manure experiments at Rothamsted (e.g.Johnston et al., 2009).

12. I would strongly encourage the authors to provide numeric data rather than just bar graphs, if others want to use this data for comparison or modelling purposes bar graphs are not helpful as you have read off the graphs. Numeric data could be provided in supplementary materials.

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