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Interactive comment on "Changes in soil carbon stocks in Brazil due to land use: paired site comparisons and a regional pasture soil survey" *by* E. D. Assad et al.

E. D. Assad et al.

jdgroppo@gmail.com

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REVIEWER #1

General comments: #REVIEWER 1: This paper is about soil carbon stocks under different land uses in Brazil. The topic is very relevant as there is not yet much information and data available about soil carbon stocks in Latin America. Especially for Brazil it is important to get a good quantification of the soil carbon stocks, because of its large area and the important land use change that have occurred over the last decennia. Therefore it is important to publish this dataset. However, the paper needs to be improved, both for the English grammar and for the methodological description

and interpretation of the results, see comments below. Therefore I recommend a major revision.

Response: We apologize for the bad English grammar. As may be noted in the author's list there is not any English native speaker, and it is always difficult to write in another language that it is not our native language. The second version of the paper was revised by Jim Hesson, an English editor of the Academic English Solutions. We sent again this third version to Jim Hesson, we believe that the English is better now. As this editing service is kind of expansive, we did send this response letter for editing, and we apologize for unavoidable mistakes.

#REVIEWER 1: Main comments: #REVIEWER 1: In the introduction or in the methodology should be explained why isotopic analyses were included, what is the relevance.

Response: We agree that this part is missing in the Introduction. We included stable carbon isotopic composition in order to investigate the origin of the soil carbon according to the photosynthetic pathways (C3 x C4). As it well known tropical grasses following the C4 photosynthetic pathway have a quite distinct isotopic composition in relation to the forest plants that follow the C3 photosynthetic pathway.

#REVIEWER 1: Why is there an uneven distribution between native vegetation (forest), pastures and crop livestock systems, I would expect one of each for each paired site.

Response: Because we collected these land uses in experimental stations and in commercial farmers suggested by the Embrapa's regional officers. Therefore, although we tried to find a place that encompasses native vegetation, pastures and crop livestock systems, not always this was possible, depending on the cropping systems adopted by each experimental stations or farmers. We tried to include in Table 1 all sort of information that we had available for each paired site.

#REVIEWER 1: In the discussion is mentioned that the state of management of pastures is important for the soil carbon stocks. It would be good to include this aspect in

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the paper, from the soil sampling on pastures there should also be information about the grassland status, making some groups of e.g. very degraded, degraded, well managed pastures and relate these the soil carbon stocks would be very interesting and more useful than the comparison with the other literature values, as these are not directly comparable.

Response: We fully agree with the reviewer, but do to time and financial constraints we do not have time neither the tools to perform a soundable evaluation of the grassland status. The only thing that was made was a visual evaluation by our team in the field. We think that was not enough to group the grasslands in degraded or non degraded. According to Braz et al. (2012) the best indicator of grazing conditions would be the live animals weight gains, which is difficult to obtain at farm level simply because there are not such records in most Brazilian ranches. As an alternative these authors have used as a proxy of pasture productivity the following parameters: regrowth rate of the sward, existing litter and that which was deposited after 12 to 28 d, light fraction of the SOM, and microbial biomass carbon. Braz et al. (2012) were able to determine these proxies in four farmers, but this would be virtually impossible in more than 100 farmers as is the case of our work. After the reviewer suggestion we run an ANCOVA using as a grouping variable degraded and non degraded status according to our crude evaluation and as the dependent variable the soil carbon stock, as co-variates we included mean annual temperature, precipitation, soil density, and the clay, silt and sand content. At 10 cm depth the average carbon stock of "non degraded pasture" was equal to 22.5$\pm$10.8 Mg C.ha-1 (n = 75 sites) and the average stock for "degraded pasture" was slightly lower 19.3$\pm$9.0 Mg C.ha-1 (n = 27 sites). However, this difference was not statistically significant ($F(1,95) = 0.22$, $p = 0.64$). At 30 cm depth the average carbon stock of "non degraded pasture" was equal to 48.0$\pm$23.1 Mg C.ha-1 (n = 75 sites), and for "degraded pasture" was slightly higher 55.1$\pm$20.9 Mg C.ha-1 (n = 27 sites). This difference again was not statistically different ($F(1,95) = 0.40$, $p = 0.53$). In relation to the observation made by the reviewer in the second part of the comment (more useful than the comparison with the other literature values, as these are not directly compa-

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rable) we did not understand if the reviewer is suggesting that we do not compare our results with those obtained by Bernoux et al. (2002).

#REVIEWER 1: Conclusion: it is stated that at some pasture and CPS sites higher soil carbon stocks were found compared to the native vegetation which could be due to the management practices. However, this is a too strong conclusion, since the spatial variability in soil carbon stocks can be very large, thus conclusions should not be based on the individual sites, but only on the aggregated averages. I agree with the conclusion that paired sites should be used for assessing carbon stock changes due to land use change, but it would be better to take more samples at each paired site to account for the local variability of the soil carbon stocks.

Response: Following the reviewer suggestion such part of the manuscript in the Conclusion was excluded.

#REVIEWER 1: Specific comments: #REVIEWER 1: Page 2, line 3-4: improve sentence

Response: The sentence was improved

#REVIEWER 1: Page 2, line 8: don't use term soil plasticity here

Response: OK, the term "plasticity" was deleted

#REVIEWER 1: Page 3, line 10: not clear to what the GHG reductions refer, total GHG emissions from Brazil, or from agriculture?

Response: The above comments were already addressed in the second version of the manuscript.

#REVIEWER 1: Page 4: how where the locations selected, for the regional sampling it states that it was done randomly, but this should explained how, and also for the paired sites

Response: For the regional sampling we think that randomly it was not the best way

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to describe the sampling strategy. Sites were selected using a road map coupled with a remote sensing image. The first criterion for selection was accessibility by a road, since Brazil is a large country and in order to collect more than 100 sites in feasible time accessibility was fundamental. Second criterion was that independently of the grassland status the pasture was in use and it was not an abandoned pasture. This second criteria was check in the field by the presence of animals and-or asking for local residents. For the paired sites the sites selection was done together with Embrapa regional offices that indicated either an experimental station or a commercial farm to be sampled. Both of these criteria were detailed in the new version of the manuscript.

#REVIEWER 1: Page 4, line 15-17: It is unclear how the sampling has been done. How many samples for carbon and texture were taken? And is it 60 cm depth? This should be described better.

Response: In each site a trench of 60 cm by 60 cm, yielding an area of approximately 360 cm2 was open (photo 1). In the pasture regional survey the depth of the trench was approximately 30 cm, and in the paired sites the depth was approximately 60 cm. The trenches were open by interval depths, first the sample for bulk density was collected (photo 2) as well as approximately 500 g of soil for chemical analysis. We explained this sampling procedure in more detail in the revised version of the manuscript.

Photo 1 – Soil trench with steel rings for bulk density sampling in the bottom. (attached)
Photo 2 – Sampling of soil for bulk density determination. (attached)

#REVIEWER 1: Page 4, line 18: ground is not the correct wording

Response: This word was deleted.

#REVIEWER 1: Page 5, line 11: Strange symbol for carbon, besides use carbon content instead of concentration

Response: The symbol [E] was replaced by [C], and the word “concentration” was replaced by “content”.

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#REVIEWER 1: Page 5, line 13: This is indeed quite important and some further explanation about why this is done would be useful, see e.g. paper of Wendt and Hauser (2013)

Response: We would like to thank the review for reminder us about details of the methodology used for soil mass correction. We conducted such correction in the earlier version of the manuscript according to Ellert and Bettany (1995). In this new version we recalculated the soil mass correction according to Ellert et al. (2008), which had much more detailed about the methdology than in Ellert and Bettany (1995). We realized that we made a mistake in the earlier calculations because we did not consider the cumulative soil mass for each profile, by doing this there was a slightly change in the soil carbon stocks, but such change did not change the overall results the statistical tests. It is also relevant to mention that we consulted the Wendt and Hauser (2013) as recommended by the reviewer. These authors proposed a much easier method of soil mass correction, which involved the estimation of soil density based on the weight of the soil sampled by a soil core and the volume of the soil core. Our sampling was conducted in 2010-2011 when this method was not published yet. Below we detailed how our calculations of soil carbon stock on a fixed mass were done. The soil carbon stock was estimated based on a fixed mass in order to correct for differences caused by land use changes in soil density using the methodology proposed by Ellert et al. (2008), which is briefly described below. Cumulative soil mass were estimated to 0 – 10 cm and 0 – 30 cm soil depths according to the following equations, respectively:

Msoil(0-10) = (0-5 x z0-5) + (5-10 x z5-10). . . . . (1)

Msoil(0-30) = (0-5 x z0-5) + (5-10 x z5-10) + (10-20 x z10-20) + (20-30 x z20-30)... . . . . (2)

where Msoil is the cumulative soil mass, is the soil depth and z is the soil depth interval. The cumulative soil carbon stock to fixed depths of 0 – 10 cm and 0 – 30 cm is calculated by the following equations respectively

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SOCFD(0-10) = (0-5 x z0-5 x C0-5) + (5-10 x z5-10 x C5-10). . . . . ..(3)

SOCFD(0-30) = (0-5 x z0-5 x C0-5) + (5-10 x z5-10 x C5-10) + (10-20 x z10-20 x C10-20) + (20-30 x z20-30 x C20-30). . . . . ..(4)

where SOCFD is the cumulative soil carbon stock to fixed depths of 0 -10 cm and 0 – 30 cm and C is the soil organic carbon concentration at the designated depths. In each sampling site, encompassing different land uses, the lowest cumulative soil mass to 10 cm and 30 cm soil depths were selected (Mref) and the excess soil mass (Mex) was calculated for all land uses (native forest, pasture and CPS) according to the following equation:

Mex = Msoil - Mref. . . . . .(3)

For each land use the cumulative soil carbon stock to fixed mass is calculated by the following equation:

SOCFM = SOCFD – Mex x Cds

where, SOCFM is the cumulative soil carbon stock for a fixed mass and Cds is the soil organic matter concentration in the deepest soil depth interval. Table A illustrates calculations of soil carbon stock for a fixed mass in one of our sites (Sete Lagoas), which encompass a pasture, a CPS and a forest. Figure 1 compares the soil carbon stock to a fixed depth with the soil carbon stock to a fixed mass to soil depths of 10 cm, 30 cm and 60 cm.

Figure 1. Comparison of soil carbon stock to a fixed depth with soil carbon stock to a fixed depth. Dashed line is a 1:1 line and the continuous line is the regression line between the two variables. (attached)

#REVIEWER 1: Page 5, line 21-27: This is unclear, how do you know that the lowest value is for sure from a C3 source and the highest from a C4. Please elaborate this a

bit more, preferably with another reference.

Response: It always tricky to find appropriate end-members for the isotopic mixing model. It is always possible to use as end-members the δ13C of the vegetation itself, but by doing this, the fractionation during the vegetation decomposition and incorporation into the soil organic matter it is not considered. That is the reason why we choose as end-members the δ13C values of the soil itself. That said, we recognize that our choice it is not well explained in the text. Additionally, in the text is stated that we selected the end-members values from the paired sites, this is correct for the C3 end-member that was selected among the forest paired sites, but the C4 end-member was selected from the pasture regional survey. We also recognize that we are not sure that the forest of the paired sites was an undisturbed forest that had never been cultivated with a C4 crop. On the other hand, we think that it is reasonable to assume the δ13C values of -13.1‰ and -13.7‰ as representative of the C4 end-member because this is the highest δ13C value that we measured for pasture soil samples. Maybe there are values higher than that, but we could not find them. Note that the soil in the grassland native vegetation located in the South part of the country (Ponta Grossa, 21.12°S, 50.06°W) had a maximum δ13C of -14.5‰ and -14.9‰ for 10 cm and 30 cm soil depth, respectively. As an alternative for C3 end-members we could use the average value of forest sites that were probably undisturbed sites according to Martinelli et al. (1996) and Saniotti et al. (2002). The average δ13C values among these sites were -27.0‰ and -26.6‰ for 10 cm and 30 cm, respectively. Note that these values are only 0.3‰ and 0.5‰ lower than the end-members that we use in the manuscript for 10 cm and 30 cm, respectively. Consequently, the calculated relative proportion of C3 and C4 contribution would not change significantly.

#REVIEWER 1: Table 1: What is meant with the column years?

Response: It is not well explained in the table. We tried to fix that by adding one more column to the table. In experimental station there is available information about when the CPS started and the crop rotation adopted. In commercial farmers this kind of

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information it is much more difficult to obtain. See new Table 1 attached.

Table 1. Characterization of sampled sites: native vegetation (NV), pastures (P), crop-livestock systems (CL), livestock-forest systems (LF), crop-forest systems (CF) and crop-livestock-forest systems (CLF).

#REVIEWER 1: Table 1: In case of crop-livestock systems there are often more land uses described (e.g. pasture and soybean), how was sampled? What I understand is that only at one location is sampled, thus on a specific land use. This should be better explained.

Response: The crop-livestock systems works by rotation among different crops. There are cases that one crop is cultivated in the winter and other in the summer. For instance, it is very common to cultivate soy in the summer and Lolium multiflorum (grass) for livestock in the winter. Finally, there are cases where Eucalyptus is also planted and crops are cultivated during the growth of the Eucalyptus. The soil sampling was done on the crop that was growing in the sampling date.

#REVIEWER 1: Table 2: the values for the confidence intervals seem to be expressed wrongly

Response: Please not that these are not confidence intervals, but instead the minimum and the maximum values. But, anyway, the minimum and maximum relative contribution of C3 and C4 was wrong for forest soils at 10 cm and 30 cm. These values were fixed.

#REVIEWER 1: Table 4: Caption too long, explanation should be included in the main text

Response: If the reviewer agrees, we would like to maintain such long caption because Tables 2 and 3 referred to the pair sites and Table 4 to the pasture regional surveys and ΔCabs (Mg.ha-1) and ΔCrel (%) are estimated differently in the two types of sampling.

#REVIEWER 1: Figure 2: Title and units of the x-axis are missing

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Response: That is strange because I have them in the copy that was submitted to the journal. Maybe they disappeared during the building of the pdf copy.

#REVIEWER 1: Figure 4 and 5 are switched

Response: Fixed in the second version of the manuscript

#REVIEWER 1: Figure 5: The axis for sand content runs until -20

Response: Fixed

#REVIEWER 1: Page 8, Line 26: What is Ns, this is not included in Table 4

Response: Fixed

#REVIEWER 1: Page 9, line 11: Linear interpolation of C stocks from 0-20 to 0-30 cm is very tricky, especially for native vegetation, where most of the carbon is often located in the top of the soil

Response: This part was removed in the second version of the manuscript.

#REVIEWER 1: Page 9: Make more clear how many samples were included from the other studies and how they were compared to the samples from your study, was this done at the sample level, or the biome level?

Response: In this new version of the manuscript we only compare our results with those obtained at the biome level by Bernoux et al. (2002).

#REVIEWER 1: Section 3.4: I assume this section only refers to the regional pasture results? Make this clear, otherwise land use should be included as explaining variable as well Page 10, line 4: here is referred to Table 4, but there is nothing in that Table about MAT and sand content

Response: Fixed.

#REVIEWER 1: Page 10: include the r2 for both equations ïËŸA In the caption of Figure 4 is referred to equation 3, this should be equation 5.

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Response: Fixed.

REVIEWER #2

#REVIEWER 2: Major land use changes have taken place in Brazil over the last decades. Understanding effects on soil carbon stocks and quantifying stocks in different biomes is therefore of high importance. The article provides a dataset of soil carbon stocks in different land uses for Brazil. Despite the high relevance of the topic the paper needs to be improved substantially before being published. I fully agree with the comments made by reviewer 1 and want to stress the importance of further improving the consistency and English of the manuscript (see comments below)

Response: We apologize for the bad English grammar. As may be noted in the author's list there is not any English native speaker, and it is always difficult to write in another language that it is not our native language. The second version of the paper was revised by Jim Hesson, an English editor of the Academic English Solutions. We sent again this third version to Jim Hesson, we believe that the English is better now. As this editing service is kind of expansive, we did send this response letter for editing, and we apologize for unavoidable mistakes.

#REVIEWER 2: Page 2, 3: The introduction needs to be improved as some parts are hard to understand and are not consistent yet (jump from soil composition to land use change, tillage systems, agricultural policies). Morover references are lacking (Trumbore 1995 represents 50% of the first page). I would suggest skipping the first paragraph and start directly on the importance of land use change as a driver of soil carbon changes in Brazil in order to highlight the relevance of you research and introduce the motivation for the article.

Response: The Introduction was rewritten in the second version of the manuscript. We tried to better contextualized our study and we added several new references.

#REVIEWER 2: Page 2, line 11: don't use "carbon saver"

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Response: Fixed

#REVIEWER 2: Page 2, line 15: jump in text from land use change to extensification (give examples) to intensification (I would not call no-till an intensification option).

Response: We rewrote the Introduction in an attempt to improve the links between paragraphs.

#REVIEWER 2: Page 2, line 20-27: only 1 quote, again Trumbore 1995

Response: Fixed

#REVIEWER 2: Page 2, 28: rephrase and improve motivation for the article ("not yet enough studies")

Response: We added a phrase in the Introduction mentioned the continental area of Brazil and the lack of enough studies to cover such a big area.

#REVIEWER 2: Page 3, line 12: you never mention the Baseline in paper again. Does it refer to initial carbon stocks under native vegetation? Please elaborate

Response: We changed our objectives and we did not mention about "baseline" in this new version of the manuscript because was really out of context.

#REVIEWER 2: Page 3, line 24: replace "made" with "make"

Response: OK.

#REVIEWER 2: Page 3, line 20-25: can be merged with paragraph above and information on MAT and MAP can be put in the study area section 2.1. otherwise repetition later

Response: Done.

#REVIEWER 2: Page 4, line 9-12: merge with section 2.1. (Funding information could go in the acknowledgements?).

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Response: Done.

#REVIEWER 2: Page 6, line 28: wrong decimal according to Table 2 -19.6

Response: Fixed.

#REVIEWER 2: Page 7, line 5: on page 6 line 7 you write for the top soil that values are “decreasing to -19.5 in the CPS and to -17.7 in pastures” compared to natural vegetation and that “the same tendency was observed for the 0-30 cm interval”, but on page 7, line 5 you say the opposite (“native vegetation soil was significantly lower”). Please explain

Response: Fixed.

#REVIEWER 2: Page 7, line 8: numbers don’t match with Table 2, probably C4 and C3 plants mixed up either in the text or table

Response: Fixed.

#REVIEWER 2: Page 9, line 10: How was this done exactly? Please explain in detail.
#REVIEWER 2: Page 9, line 26: quote missing

Response: Fixed.

#REVIEWER 2: Page 10, line 4: Can you provide more information on the regression analysis and coefficients (3.4 controls of carbon stocks)? Numbers can’t be found in Table 4

Response: Fixed.

#REVIEWER 2: Page 10, line 14-15: Results are in line with literature however a small sentence with a reference could be added e.g. Post W. M., Emanuel W. R., Zinke P. J., Stangenberger A. G. (1982) Soil carbon pools and world life zones. Nature, 298, 156-159.

Response: Fixed.

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#REVIEWER 2: Page 10, line 19-22: the sentence is confusing as it indicates that the following sentence("this trend was confirmed: : :"also refer to nitrogen. Please clarify.

Response: Fixed.

#REVIEWER 2: Page 11, line 6-13: Can you explain why pasture carbon contents in your study are lower than those in cropland? Are most pasture areas degraded?

Response: We think that this difference is due to a better management in cropland farmers when compared with ranches were only grasslands are cultivated.

#REVIEWER 2: Page 11, line 18: Couldn't the shallow sampling depth influence results as agricultural soils accumulate carbon in the topsoil especially under conservation tillage? Why did you choose a sampling depth of 30cm? Overall there is a need to increase sampling depth beyond the topsoil in order to avoid artificial redistribution effects in the soil see: Baker J. M., Ochsner T. E., Venterea R. T., Griffis T. J. (2007) Tillage and soil carbon sequestrationâËŸAËĞTWhat do we really know? Agriculture, Ecosystems & Environment, 118, 1-5

Response: For the pasture regional survey as we sampled more than 100 sites we choose 0 -30 cm due to time and economic constraints, otherwise the duration of the sampling trip would not be affordable. Additionally, the 0 – 30 cm is the default depth established by the IPCC for soil carbon stocks comparisons among different land uses. On the other hand, for the paired sites, as we had fewer sampling sites we were able to sampled down to a depth of 60 cm. We choose not to show this set of data because the pasture survey was done only to 30 cm. However, after the reading the paper of Baker et al. 2007, recommended by the reviewer we decided to include carbon stocks from this soil depth in this version of the paper (see Table below). It turned out that, although the soil carbon stock of the native vegetation was higher at this depth than the pasture and CPS soils, the variability was high and numbers were not statistically different from each other. Therefore, the differences in soil carbon stocks were restrictedly to a depth of 30 cm. Overall, it is important to mention that our main objective was not to compare

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soil managements like till x no-till, but survey the soil carbon stocks in different land uses and compare them with the native vegetation.

#REVIEWER 2: Page 12, line 5: improve sentence

Response: Improved.

#REVIEWER 2: Page 12, line 8: not clear which studies “the other two studies”, is it Zinn et al.?

Response: Fixed.

#REVIEWER 2: Page 12, line 10-12: difficult to understand

Response: Fixed.

#REVIEWER 2: Page 14, line 8-11: From your analysis you cannot conclude that “suitable management practices may exert an important factor in carbon accumulation in the soil” as you didn’t include different managements as a dimension in your analysis. However, a sampling of pastures sites according to management would be very interesting and allow for such conclusions.

Response: Ok, we deleted such statement.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C3421/2013/bgd-10-C3421-2013-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 10, 5499, 2013.

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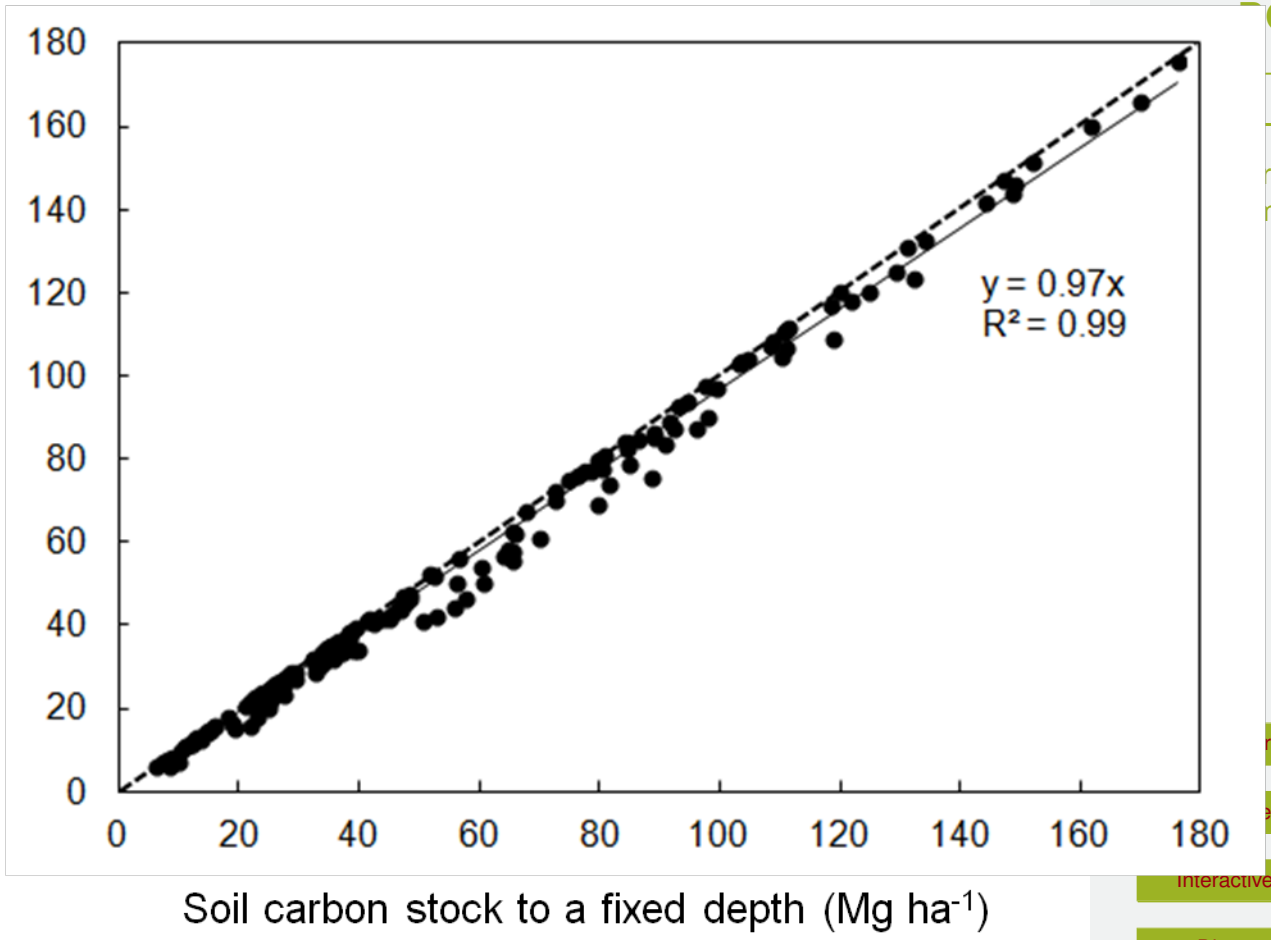

Fig. 1.

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Fig. 2.

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Fig. 3.

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