

Interactive comment on “Remediation of degraded arable steppe soils in Moldova using vetch as green manure” by M. Wiesmeier et al.

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We thank Dr. Garcia-Franco for the helpful comments on our paper. Below our answers to the raised questions:

"Could be interesting the study of the effect of green manure using vetch on the different functional organic carbon pools (active = labile, slow, passive pool). I think it will be possible in the future because the knowledge acquired in this area could be very useful for the implantation of the best management practice with the aim of soil conservation and mitigation of climate change in Chernozems of dry areas."

We agree that data on OC pools would be helpful, this issue was also raised by Prof. Boincean. Right now we started to fractionate soil samples and hope to integrate the

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results in a revised version of the paper.

"- Pp. 512. Lin. 11-15. You emphasize the importance and advantageous of mixtures of hairy vetch and winter wheat than legume monocultures. . . It is the same in Pp.516, lin.25: "Remarkably, studies which applied mixtures of hairy vetch and cereals as in our study. . ."but then in the manuscript you always use hairy vetch only but your treatments was 80% hairy vetch and 20% winter wheat. In my opinion if you only say: "hairy vetch" emphasize of the importance of use mixtures in these soils disappears. I prefer "mixture of vetch and wheat" or "green manure" or use acronym: HVC = hairy vetch and cereal or HVW (hairy vetch and wheat)."

We agree and will introduce an acronym.

"- Pp. 513. Lin.11-12: "hairy vetch was incorporated into the soil using a disk harrow" (until which depth: topsoil, 15 cm. . .? Or until what horizon: Ahp1, Ahp2. . .?). I think it is important to say. In other similar studies with tillage (mainly reduced or minimum tillage) and green manure, the tillage is necessary because it favours the incorporation of plant material into deeper layers, promoting the formation of new aggregates with a high OC content in these layers, while no tillage only results an improve in topsoil."

We will add this information.

- Pp 513. In material and methods you did not anything about different depth intervals (from 0-12, 12-20, 20-35, 35-47 cm) or about horizon intervals (Ahp1, Ahp2, Ahp3, Ah).

We will add this information.

- Pp. 513. What is the duration of the experiment? One years, two years? When soil samples were taken. . .in 2011, 2012, 2013, every years? It is not clear in the text.

We will explain this in more detail.

- Pp. 513. Lin17-18. . . . "Adjacent control plots" has the same soil type? What is the

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main crops?

Yes, the control plots are very close and have the same soil type, we will add information on main crops.

- Pp.513. Lin 25. Why did you choose dry sieving? Why not wet sieving? Both dry and wet sieving of soil aggregates are approved methods. We decided to choose dry sieving based on the results of Sainju (2006), who concluded that "Dry sieving...can be used as a rapid and reliable method of separating soil aggregates for determining C and N pools compared with wet sieving, which reduces microbial activities and N mineralization because of the destruction of physical habitat of microbial communities in aggregates and excludes watersoluble C and N pools".

- Pp. 514. "...Calculated on the basis of an equivalent soil mass..." I agree. I think is necessary.

Ok

- Pp.514.Lin. 17-18. You determined root biomass...but I did not see the results about this parameter in a table or figure.

Root biomass data for vetch was presented in table 1.

- Pp. 515. Lin. 9-11. "...In Orhei, the yield of sunflower significantly increased ($P < 0.05$)...similarly, corn yields significantly increased ($P < 0.05$)..." But why you didn't use different letters in bars of the figure2? Why in the figure 2 is not written $P < 0.05$ in the end of Figure caption? I think these above mentioned are necessary to a better understanding.

We agree and will add this in figures.

- Pp.515. Lin. 2. Table 1. Is possible to include in table 1 the control plot? Do you have aboveground and belowground results from control plot? How do you know if your aboveground and belowground results in your study site (Orhei and Cahul) are high or

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low without the results of above- and belowground from control plot?.

The information provided in table 1 is only related to above- and belowground biomass of vetch, there is no need to show biomass data of main crops.

- Pp. 515. Lin. 13. I think is better tell about depth or horizon interval than say "the topsoil (0-47 cm)". I addition, in table 2 are horizon with their corresponding depth (I agree. I think is better that only write horizon or depth). I think is necessary in Table 2 to write " $(P < 0.05)$ " in the end of footnotes. Are there significant differences between study site, and depth in basic soil properties? In table 2 is not the control plot, is not necessary to compare changes?

We will add these details. The basic soil properties shown in table 2 represent soil conditions of experimental sites before the start of the experiment.

- Table 3. There are depth intervals but not horizon intervals while in Figure 3 there are horizons intervals but not depth intervals. I think is necessary to homogenize. Also is better write: " $(P < 0.05)$ " in the end of figure caption and footnotes.

We agree and change this in a revised version of the manuscript.

Respect to the references:

- In the text

1. I believe that is necessary the same trend: an increasing or a decreasing order of years of authors. Example. Pp 511. Lin.14-15: "Eitzinger et al., 2013; Supit et al., 2010 and Trnka et al, 2012" is better the most earlier first: "Eitzinger et al., 2013; Trnka et al., 2012 and Supit et al. 2012. Please is necessary to review it in all manuscript.

The references are sorted in alphabetical order, we used the official Copernicus style for references.

2. When the authors are only two, you use both of them in the reference. Example: Tisdall and Oades, 1982, but then, in Pp 514, lin 2 you write Ellert et al., 1995 and in

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Pp 514, lin.5: Ellert and Bettany, 1995. The same occurs with Rochester and Peoples 2005: sometimes is with both of them and sometimes is only Rochester et al., 2005 (in text and in table 1)

We will change this.

Interactive comment on Solid Earth Discuss., 7, 509, 2015.