

## ***Interactive comment on “Aggregate structure and stability linked to carbon dynamics in a south Chilean Andisol” by D. Huygens et al.***

### **Anonymous Referee #1**

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#### General Comments

This very interesting paper presents new insights in the influence of land use and differences in soil physical and chemical parameters on aggregate and SOM stability in Chilean Andisols. I liked the idea to conduct mineralization analyses in both, bulk soil samples and the light fraction of the MOM pool in order to differentiate between the recalcitrance of SOM from different land-use systems and the effect of SOM protection in the soil. Overall the methods seem to be acceptable to answer the main questions of the manuscript though some limitations are addressed in the specific comments. The discussion could be overall a bit more structured in different subchapters and not as much focussed on aggregate stability, since also other processes affecting SOM stability in the soils might be relevant.

#### Specific Comments

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The test of aggregate stability by comparing WAD in soils before and after a mineralization experiment (which mainly includes the rewetting of the soils) is not a standard method but seems to be appropriate to get an overall idea about differences in aggregate stability among sites - assuming that the decrease in OM by mineralization did not affect aggregate stability. Still, I am not fully convinced that the absolute value of delta-WAD is overall a suitable factor to test for a functional relation between aggregate stability and the mineralogical composition by using regression analyses. On page 217 the authors argue that less Alox concentrations should lead to a lower aggregate stability, indicating that grassland sites with highest aggregate stability should have the highest Alox values. But this was neither the case for 0-10 nor for 10-20 cm soil depth. Therefore, the relation between Alox and aggregate stability (and probably also the correlation between WAD and soil C mineralization rates) should be considered with caution, although it fits well with some theoretical models. Since this relation appears at many places in the discussion, the correlation matrix should maybe be presented in a graph.

During a particle size fractionation, the pool  $>150$   $\mu\text{m}$  was separated from finer material, with macroaggregates  $>250$   $\mu\text{m}$ , but not the microaggregates being destroyed. This fraction was used to separate the MOM pool, which was further divided into different density fractions. One question that came to me was what is the functional difference between the C pool in the heavy fraction of MOM and the large C pool in the remaining mineral fraction  $> 150$   $\mu\text{m}$ ? Wouldn't it be reasonable to combine these fractions? If so, it might be more useful to demonstrate differences in the better defined light fraction in table 4. Furthermore, correlations between the relative contributions of MOM to total C with Alox as determined in the present study might be of limited significance. Overall, the mineral fraction  $< 250$   $\mu\text{m}$  might still contain some occluded POM in microaggregates which is not assessed with this fractionation scheme since no attempt was made to disperse soil particles properly e.g. by ultrasonic treatment to destroy microaggregates. The incomplete destruction of microaggregates might also be responsible for the minor differences observed in  $\text{d}^{13}\text{C}$  within size and density frac-

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tions. Also C concentrations in different particle size classes were remarkably similar.

In the discussion in part 4.1, a stronger separation between the function of Al-humus complexes and allophane as stabilising agents for SOM alone, and their possible role in the formation of larger aggregates is necessary, maybe by applying another subchapter in the discussion. Besides the physical protection in aggregates, SOM in Andisols can also be stabilized by physico-chemical processes, such as metal-humus complexes, ligand-exchange with allophane, or sorption to clay particles. The relevance of Al-humus complexes was for example stressed by Percival et al. (2000, Soil Sci. Soc. Am. J. 64 1623-1630). The Alpy contents on the GRASS sites were significantly higher than in the forest sites, so that the possibility of increased protection of SOM by metal-humus complexes on the GRASS sites (maybe due to a higher release of Al) should also be discussed besides the aggregation. Similar to the presented results, Hoyos and Comerford (2005, Geoderma 02355, article in press, online publication) found no aggregate hierarchy in Andisols and they also suggested a contribution of amorphous inorganic minerals to aggregate formation. With this respect, the increase in positive charges with decreasing pH values seems to be a good reason for the higher aggregate stability of the grass sites. Concerning the effect of soil pH on Al-humus complexes in Andisols, the new paper by Takahashi et al. (2005, Geoderma 02359, article in press, online publication) might also be interesting to you.

Furthermore, in the discussion about differences in soil C following land use changes, also the issue of differences in litter input as the main source for SOM should shortly be addressed.

Since this is supposed to be a sort of discussion forum I was wondering why the authors restricted themselves to Al. Fe, which is also a polyvalent cation present in Andisols, can react similar to Al in many ways and thus further add to the stabilization of organic C and aggregates in the studied soils.

Page 204, line 24, 25: Andisols do not have per se a high cation exchange capacity

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since the charge is mostly variable and thus highly pH dependant and nonallophanic Andisols can be rather acid. Maybe you should better say a high potential cation or anion exchange capacity.

Page 205 2nd paragraph: In the first part you give a short a short summary on the effects of land-use changes on C-stocks and then continue in the second part with a description of the vegetation history of Chile. You might think of having two separate paragraphs here. The paragraph on the effects of land use changes would be improved if you would also present the results of the listed studies on land-use changes (line 5-6)

Page 207 last paragraph: Information on the distance between the plots and the method of soil sampling (coring or from a soil pit) should be added. How did you make sure that the soil samples were representative for a certain vegetation type and how did you check the homogeneity of the analysed plots? The information that the GRASS sites were ploughed every two years should be included in the methods part (it first appears in the discussion on page 217).

Page 208 line 24: How did you destroy macroaggregates? This is not clear from the text.

Page 212: Results of bulk density are presented in the text and table 1, but the determination is not described in the methods part. The methods and results of texture analyses should also be included in the text and table 1. Since the soil was defined as a sandy loam, the sand content seems to be rather high for Andisols and possibly indicates the presence of stable aggregates.

Page 216-217: Stable micro-aggregates in Andisols containing allophane are formed irreversibly upon drying to pF values above 3-4 (Wada 1989, Shoji, et al. 1993). Thus, even air-drying can affect the particle size and micro-aggregate distribution. This effect can usually not be fully overcome by rewetting the samples or even usage of mild ultrasonic. Tillage on the Grass sites might have exposed the soil unprotected to the sun and led to increased drying of the upper soil cm. This might have led to an increase

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in stable micro-aggregates on these sites and presents another possible explanation for the higher aggregate stability at these sites. Continued ploughing would lead to a mixture of the aggregates in the top 30cm (?) of the soil.

### Technical Corrections

While significant differences between sites and fractions were usually indicated, you should not completely do without presenting any parameter indicating the variability of your results in the tables.

Page 205 line 14: schrublands, eliminate c

Page 207 line 1-18: The authors of this manuscript have added the author names to the botanical names of the grassland species but not to the forest species. Author names should be used or omitted consistently according to the specifications of the journal.

Page 209 line 7: (LF>IF>LF) one should be HF..

Page 211 line 16: which device type was used as AAS?

Page 212 line 10/11: Nitrogen stocks varied... not concentrations

Page 212 line 14: One space too much before ... . Soil texture ...

Page 213 line 1: One space too much before ... . The WAD ....

Page 214 line 1: ... macroaggregates (>150 ym)... shouldn't it be > 250 ym?

Page 216 line 5: ... which resulted of changes... replace of with from

Page 216 line 11-14: I do not understand the problem with the pH of the extract. You should either make it clearer or think about omitting the sentence.

Page 217 line 1: ... procedures results showed... omit results

Page 217 line 5: that the GRASSE omit that

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Page 220 line 11: ...in our statistical analyses... add "al"

Page 220 line 23: I am not sure if you want to write higher macroaggregate turnover here

Page 221 2nd paragraph: the results of the correlation analyses should probably better be presented in the results part first

Figure 4: Labels on axes and text in the graph should have a bigger size

Table 1: Alox, Alpy, Siox, TC and TN should be expressed as g kg<sup>-1</sup> instead of %

Table 3: Carbon concentration should be expressed as g kg<sup>-1</sup> instead of %, the letter b is too big and not italic at < 50  $\mu$ m / SGFOR.

Table 5: I think for comparisons with other studies it would be nice to have the MOM fraction also separated in heavy and light fraction and their relative contributions to total C.

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