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Interactive comment on “Biochar increases plant available water in a sandy soil under an aerobic rice cropping system” by M. T. de Melo Carvalho et al.

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Dear Dr Marcus Hardie,

Thank you very much for your review of our manuscript. We appreciate your comments and suggestions. Below you will find some remarks regarding the points raised in your review:

1) We recognize the importance of a detailed analysis of biochar in studies testing its effect on soil water retention. We also raised this point of view in the conclusion (Page 906: Line 2-5). There are many different ways of doing this. There is for instance an inspiring study on effectively designing biochar for water retention by Gray et al. (2014).

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You also covered this aspect very elegantly in your study. In our study, we combined SSA via BET analysis and SEM images, which are good indication of the pore structure of the wood biochar used in the field trial. The results are very insightful and helpful in our analysis, though there are always more observations one could make if finance and time would not set boundaries to what one can do.

2) The centrifuge method we used is adapted from Freitas Jr & Silva (1984). An advantage of this method is that it uses undisturbed soil samples. We believe that undisturbed soil samples better preserve the soil structure, which is important in case of testing the effect of soil management in long term field trials. We collected the samples when soil was moistened (Page 894: Line 3-5) and for that reason we are convinced that 12 h was sufficient to completely fill porosity of samples with water. It should also be realized that any alternative would have its own set-backs and limitations. Ventura et al. (2012), for instance, reported some unexpected results in treated samples with biochar when analysing disturbed samples in the Richards pressure apparatus.

3) When reading through your comments, you seem to suggest that we selected the specific novel statistical approach because it would allow us to find significant differences in data that might otherwise not be significant (when using more traditional approaches). This really is an important misconception. Our paper in fact really contains two important items. The first is topical and is about the effect of biochar on soil water holding capacity and implications for rice yield. The second is methodological and is about overcoming three important statistical shortcomings when using the traditional isolated treatment-specific model fitting (Page 890: Line15-28; Page 891:Line 1-2). We have used a nonlinear mixed (NLM) model precisely because it allows to take into account the high spatial variability on a field trial by: 1) taking into account the whole structure of variance among treatments in one specific season and soil layer, and 2) by adding to the model correlation among measurements taken within the same soil sample. Spatial variability is a systematic issue mentioned in other field studies, such as by Liu et al. (2012) and also yourself (Hardie et al. 2014). We are of the opinion

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that this is an aspect that should not be neglected. The traditional statistical method (ANOVA) is inadequate because it ignores the quantitative nature of the 'tension', the fitted van Genuchten model and the repeated-measures structure of data. Whether results would be equal or different of the ones we obtained with NLM model is irrelevant. We are convinced that the methodology presented in this paper is an important step forward and will serve as a valuable tool for future field studies. The outcome of our analysis is presented as fitted SWRC for each treatment. Comparison of shape parameters is then complementary to the visual inspection of these curves. This is the reason that we are presenting both in our paper.

4) We indeed found a significant increase in MAC without a significant effect on bulk density. This implies, that the effect of biochar on MAC is, most likely, purely due to an increase in soil pore space, rather than due to formation of new aggregates. Particularly since the increment in MAC was proportional to the rate of biochar, we are confident that the observed response is not an artefact. We agree with you that we should substitute our statement “an INCREASE in overall porosity of the soil” (Page 888: Line 15; Page 902: Line 9; Page 905: Line 24) by “an EFFECT on overall porosity of the soil”, because that’s precisely what we mean: that biochar has affected the soil water retention capacity of the sandy soil at both low and high matric potential.

5) We do not agree that our paper is exaggerating our findings. In fact, our intention is to present what rice farmers might expect from a single application of biochar. For that reason we have deliberately used a biochar that is available in the region of this study, and we also used feasible amounts of biochar. Our results show that the water retention capacity is increased at low matric potential and decreased at high matric potential in both seasons. Unfortunately, rice yields were not affected. These are the findings which we report in our paper: no more no less.

Kind regards,

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Interactive comment on Solid Earth Discuss., 6, 887, 2014.

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