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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.

M. Hardie

marcus.hardie@utas.edu.au

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Congratulations on an excellent and superbly written paper. It's a relief to read something so well put together. The findings in the paper happen to be the exact opposite of what my own studies in this area have found, and thus are of considerable interest to me. I have a number of comments about the paper and some suggestions for improvement if the opportunity still exists. Many of these comments also apply to much of the literature on determination of SWRC and understanding of the effects of soil amendments on porosity. I think the biggest omission is some detail of the biochar porosity. The paper as a whole doesn't make the logical connection between the SWRC and

soil porosity, if you know the soil potential then the approximate soil pore size can be easily determined by the capillary equation. When explaining changes in PAWC or the SWRC, knowledge of the pore size distribution within the biochar is extremely useful and can indicate if changes in porosity are due to the internal porosity of the biochar or the creation of packing pores as you allude to with reference to biochar particle size. There are two ways to determine the pore size distribution, firstly mercury porosimetry (best), secondly through image analysis of SEM images using photoshop and Image J (Schneider, Rasband et al. 2012) (Impoco, Carrato et al. 2006). You could possibly do this with the images you have. The manuscript is heavily dependent on analysis of SWRC by centrifuge which is referenced as a internal document (I don't know if it is peer reviewed). The centrifuge method for determination of SWRC is not popular, there are only a handful of papers in which it has been shown to be correlated with pressure plates ie (Reatto, Da Silva et al. 2008). Im not familiar with the approach, from my understanding it gives reasonable approximations on soils, but does that also hold true for biochar with a density of 0.3 g/cmˆ3. It would have been good if there was also some pressure chamber data to support the centrifuge method. There is a large section of the soil physics community who would dismiss the paper without further comment simply because you chose to use the centrifuge method. Given the central importance of the method to your study I think the text should have discussed the approach in more detail and presented an argument for the procedure over more traditional and proven approaches. Im not very familiar with the use of non linear mixed models, however I suspect that the approach is finding significant differences in data that might not otherwise be significant using more traditional approaches. I would have been more convinced if you had presented a table of the mean and SD values of the retention function at each of your equilibration points. Being somewhat of a traditionalist it would have been interesting to see if ANOVA or t-tests also found significant differences at each equilibration point. I strongly suspect that more traditional statistical analysis would have reported fewer differences between treatments. Table 1 shows only 5 out of 24 combinations of year, biochar rate etc had a significant effect on alpha or N. Furthermore there was no effect on Qr in both years and only Qs in the last year. From this analysis you would have to conclude that the biochar had minimal if any effect on the SWRC. This is really important but is largely ignored in the discussion and conclusion. In many ways its whether you have a significant difference in these van Genuchten parameters that is more important than differences at any particular equilibration point. Looking at your data in Figure 4 it appears that differences are in fact quite minimal between treatments, variation of 5% moisture (especially below -10 kPa) is really common in in situ soils. The significant increase in MAC in the second year is a bit spurious, as there was no significant difference in bulk density. In theory the total porosity calculated from bulk density should equal the Qs, but I have also found difficulty matching these two points from saturated cores. I suspect with such a short wetting up period that you had air entrapment within the cores, I typically wet up cores for 1-2 weeks to allow the air to dissolve into the soil water. I think what it shows is quite a bit of uncertainty around the saturated water content. If you had traditional desorption data between -1-10 cm tension this would resolve the issue. However whilst there is no difference in bulk density but a difference in Qs, the more conservative approach would be to assume there was no meaningful difference in the saturated water content or macroporosity at saturation. Again I wonder if more traditional statistics would have found a difference. I think the discussion is missing a deeper explanation of the mechanisms by which the reported results could arise. For example, its commonly assumed that biochar is dominated by micropores (this is why you need the pore size distribution of your biochar) if so then why did biochar application REDUCE the porosity below 100 KPA, or reduce the moisture content of the permanent wilting point -1500 kPa, surely it should be increased with biochar. This is quite odd, the only mechanism I can think of is fine biochar particles blocking fine pores or biochar reducing aggregate stability. By the looks of your biochar in Figure 2 you have a dominance of 10-15 um pores, which is equivalent to a matric potential of around 30 kPa, this is within the RAW and PAW. However looking at figure 4 it appears that the matching point between the retention curves of the control and biochar treatments is around -30 kPa (hard to tell).Also from

figure 4 it appears that biochar application had the greatest effect on near saturated water content or bulk density at potentials between 0 and -1 kPa (ie greater than 300 micron), but your biochar didn't appear to have pores larger than about 10 um. If there was a real increase in MAC or Qs then I think you also need to propose a mechanism, it wouldn't appear to have resulted from the internal porosity of the biochar ?, could it be increased soil fauna burrowing as we discovered, or due to packing pores between the aggregates and the biochar particles. You say in several places in the manuscript that the biochar resulted in an increase in overall porosity of the sandy soil. Your data doesn't actually support this statement. If biochar increased overall porosity then the Qs and Qr should have been significantly higher…they wernt. What did appear to happen is that Qs may have increased (not supported by BD data) and Qr decreased (although not significantly). So you actually had more larger pores and less smaller pores, such that these two minor adjustments in the SWRC combined to yield a significant difference in RAW and PAW. There was not an overall increase in porosity, you actually had a decrease in porosity below about -10 kPa. I found the write up overly positive, which is common in the biochar literature….i tend to be glass half empty. To summarise what you didn't find. There was no significant effect of biochar application on van Genuchten functions alpha and n at any depth, or rate of biochar after 3 years. There was also no effect of biochar on Qr in any depth, rate or year. In only the last year was there an effect of biochar on Qs in the surface horizon, although this is spurious as biochar had no effect on bulk density. There was no effect of biochar on any soil property other than MAC after 3 years at 15-20 cm depth. The change in MAC is difficult to understand as neither BD or Qs demonstrated significant changes. ??. The only changes appear to be RAW and PAW in the topsoil, however figure 4 shows these differences to be slight (perhaps in the range of 2-5% ???) and almost entirely due to lower microporosity which is not supported by Qr data. In other words quite a lot didn't change after biochar application, and what did change was fairly small and not supported by multiple lines of evidence. What didn't change probably needs more weight in the discussion and conclusion. I hope these comments are useful and not overly negative, many of my comments reflect my own concerns over the measurement and statistical analysis of soil porosity, which apply across much of the soil physics literature not just your manuscript. As I said at the start it's a really interesting and superbly written paper that your rightly should be proud of.

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