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Solid Earth
Discussions

Interactive comment on "X-ray microtomography analysis of soil structure deformation caused by centrifugation" *by* S. Schlüter et al.

Anonymous Referee #1

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The article presents some interesting work related to describing the dynamics of soil structure as observed with X-ray CT. This is a rapidly advancing technique, increasingly being used in soil science, yet very few have used the technique to date to quantitatively describe soil structure dynamics. The paper reports on this and uses the method by which a water retention curve is measured via centrifugation as an example. As a result, the paper is a bit between two objectives: (1) testing of the centrifugation method, and (2) testing of the deformation method. I found aspects of the paper interesting to read and certainly some of the imaging show the advancement that they have made. I nevertheless have some concerns with the paper which I highlight below. I would like the authors to comment on these, and/or address these issues. Water retention method: I first comment on the water retention method as this is also one of the strands in the paper. Obviously the method is not new and I am not sure what the

paper adds to this method. We know soil is deformed during this technique; we may now have a different way of quantifying this deformation, but we are no further in this helping us in understanding or interpreting the data. In fairness, the authors haven't made testing the method an explicit objectives, but they nevertheless state that the scrutinize the assumption of a rigid soil matrix (2810). I would suggest that by measuring 1 single sample you are not scrutinizing a method, so I suggest you down play any conclusion you make in relation to suitability of the method, or at least be clear not to generalise. So when we jump to p 2821 and the first line of your conclusions states that the method is a fast method to obtain water retention curves; this is not a conclusion you can draw from your work, or you certainly do not show any data towards this. I think throughout the paper the method should just be seen as a way to generate structure deformation. - I note that you have no replication. Can you justify this why you think all this work could be done on a single sample? - I note the unusual high residual water content as estimated with the Van Genuchten fit. Could you explain this? - There is not enough information in the methods about the method. What speeds were used; was there any replication; how long did you let this spin? How do you know that this was long enough? You mention that you did this long enough but give no indication how this was assessed and the high residual water content may suggest that it still wasn't in equilibrium.

Background: I can understand why you separated this information from the introduction, however this section mixes literature overview reflecting the current state and also seems to introduce as background some personal opinion/experience. For example (2811) ' a straightforward implementation will most certainly lead to a failure…' what is this statement based upon and where do the 'best practices' you put forward come from. This appears to me to be part of the method development and the way it is currently written I do not have the information to scrutinize this opinion. It may be true, and it can be valuable information, but if this is based on your own experience derived from, what appears to be 1 single sample, then can you really make these statements? In this entire section it would be could to explain in more detail what your statements are based upon: published work or your experience. If the latter, we may need a bit more detail and it may be better placed in a section method development? Image analysis: This was generally OK, but I would like to see more explicit description of the methods and not just references to software such as Quantim. In particular I would like to know if there are user defined settings in your method and if they differed for each of your samples. Equally so, you mention that you 'denoised' the images. I suspect you mean that you reduced the noise, but here too we have no indication of noise present before and after (unless you truly removed all noise?) and we also have no indication how relevant this is for the method you are showing. If this paper is about the first application of the method, then more information on the method (and factors influencing the outcome such as noise) would have been welcome. Do you have such information? Deformation: I understand you focus on the rocks for displacement, but this section could have been more explicit. This is actually, if I understand you correctly, where the method is less suited for soil unless there are rocks in it? Is that correct, or did I misunderstand this? Overall you have mixed your results on deformation with a discussion on deformation and I suggest you separate these.

Methodological limitations: You mention computational cost and accuracy, but I fail to see where you show the data on this. You have undertaken, and described, some handling you did (voxel size, e.g.) but you give no data on computational benefits, nor on how this affected the accuracy of the method, so I am left a bit wondering if this is something we should adopt or not. More detail on this will help the reader to understand if this method us useful for their application.

Analysis: I couldn't detect if you did your analysis on a region of interest or on the entire soil sample. Fig 5. Clearly suggest that you have selected a region of interest and the question is of course how you selected this for a material which got deformed.

Fig 3. This could do with a bit more explanation: frequency of what? Also, throughout the article it may be worth to remid the reader about the resolution

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Interactive comment on Solid Earth Discuss., 7, 2807, 2015.