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

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We thank reviewer 1 for his valuable comments and agree that implementing his suggestions will considerably improve the manuscript. Our reply is ordered according to his item list:

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

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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.

It's true that the genuine scope of the paper is a protocol to measure three-dimensional structure deformation in soil. We have chosen the centrifugation method mainly because it is suitable to evoke deformation under controlled conditions. We have shown the resulting water retention curve merely for sake of completeness, as it is the reason why the centrifugation method is applied after all. But it doesn't contribute anything to the central message of the paper. Therefore we will leave out the water retention data and its discussion in the next draft, including open questions and missing information about the high residual water content and equilibration time

We also agree that one sample is certainly not enough to generalize conclusions about

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(1) the effects of centrifugation on hydraulic properties and (2) the suitability of the developed protocol the measure deformation in soils with completely different texture and rock content. Therefore we will show the results for a second sample with a very low rock content ($< 1\%$) in the updated draft, but will only discuss it with respect to the imape processing results.

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?

These statements are based on our own experience and we will stress this in the updated draft. As requested, we will also show more data to proof our point. That is, we will compare registration results for the entire gray scale range (including pores) with the results for rocks only.

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

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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?

We will add a scheme to the updated draft that shows all steps of the workflow as small 2D sections to visualize the effect of each image processing step. We absolutely agree that all parameters should be stated and more importantly should be kept constant for all images.

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.

Yes, it's true that rocks play an important role in guaranteeing a good recovery of the deformation field. This immediately raises the questions, what the minimum amount of rocks has to be in order to still get good image registration results. Therefore we will show new data, for a soil sample with a rock content of 0.2%. For comparison, the rock content for the soil in the first draft was 3%. The question, whether the contrast enhancement for rocks has to be applied at all (or the orginal image including soil matrix and pores can be used) will be addressed as well. The quality of the registration for both modes (with and without contrast enhancement) will be evaluated visually. We

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will also make sure that the results and discussion are separated more clearly.

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.

We reduced the voxel size because of RAM limitations. This is a topic of its own and hard to generalize. The used memory is made up of the original and the deformed image which have to be loaded by elastix, a working copy of both with floating point precision on each pyramid scale and a block of memory into which the result is written again with integer precision before it is stored on the hard disk. So even a quantification of RAM consumption in terms of multiples of file size will depend on the chosen pyramid scale and the choice of float or double precision for the working copies. The effect on accuracy is hard to quantify. Only some general statements saying that registration cannot be more accurate than the resolution of the final pyramid scale can be made. We will discuss this in the updated draft.

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.

We always work with regions of interest (ROI). We used the maximum of available information by first generating a cylindrical region of interest that covers the undisturbed soil core without disturbances at the boundary. We also need to reduce

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our ROI in z-direction. At the bottom there are some severe shading artifacts due to the core holder and the aluminum plate that had to be cut out. At the top we cut away the gradual transition from empty head space to native soil that is due to the roughness of the soil surface. We made sure to plot all porosity profiles with correct z-offset by looking for fixed reference locations (e.g. holes in the core wall or z-position of the aluminum plate) in all images. We will this information to the updated draft.

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.

Frequency refers to the number of counts for each size classes normalized by the number of total counts. We'll show cumulative pore size distributions in the updated draft that allow for a better comparison between samples and will provide detailed information in the figure caption.

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