Solid Earth

Discussions

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Interactive comment on "Classification and quantification of pore shapes in sandstone reservoir rocks with 3-D X-ray micro-computed tomography" *by* M. Schmitt et al.

M. Schmitt et al.

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Dear referee thanks a lot for your very detailed comments and suggestions, we believe that they aid to further improve the thoroughness and the quality of our paper. We are confident that your suggestions, specific issues and technical comments were suitably addressed as follows.

General comments and suggestions:

You are right, since this study is a preliminary part of an ongoing research project, we could "switch" from "research paper" towards a "method paper". Nevertheless, and as

far as we know by the literature research, there is no such study dealing with different types of sandstones in 3D geometries derived by CT, and the older approaches have been used for 2D investigations only. Hence, it seems legit to keep this submission as a research paper.

You also point out in your introduction that the equivalent diameters "have little meaning when the pores are very anisotropic". At this point we would prefer not to talk about "anisotropic pores", since anisotropy is per definition the directional dependence of a physical property or a process, not of a geometrical structure. Hence we should talk about regular or irregular shaped pores. We also only partially agree to your statement that "both findings are not surprising". For highly complex structures, i.e. highly irregular shaped pores, this seems to be the mandatory case. But for "classical" reservoir rocks, which are characterized as "homogeneous" and "isotropic" in terms of depository environment, mineralogy and physical properties (such as the S1 and S2 sandstones that we present in the paper), due to the good grain sorting and rounding also very regular shaped pores could be expected . As the results have shown, this assumption does not seem to be appropriatefor any kind of sandstone when a pore scale characterization is performed.

We appreciate the referee's insightful comments concerning the comparison of the pore shapes to (e.g.) measured / modeled permeability values. In fact we must say that at this point our discussion and conclusions need to be rephrased to clarify the next steps, since also R2 asked the same point. You are of course correct, this should and will be done - one of the next steps of the project will be to try to "decompose" measured petrophysical laboratory data (permeability, electrical conductivity, NMR T1 & T2 relaxation times, pore size distribution, capillary pressure, surface area and fractal dimension), which are integrated and averaged values over the investigated sample volume (not all, but most of them), into the different (shape & size) clusters and to characterize the individual influence on the on the pore scale towards the laboratory scale. We will furthermore rephrase and clarify this in our paper.

Specific issues:

To 1.) First we need to point out that we did not use any small pores, which are smaller than 2-3 times voxel resolution. They have been sieved out in order to address the segmentation uncertainty. What is left is – in terms of 3D image resolution – a bundle of small and disconnected pores that are correlated with structural features of the sample, e.g. like clayey agglomerations (clay booklets) or weathered minerals (feldspars) that cannot be resolved by even high resolution μ-CT. At this resolution suchpores might not have much "physical" relevance, but they might became essential on even smaller scales (e.g. trapping structures, gas storage). That's why we have taken them into account at this point. We can and will clarify this point in the manuscript.

To 2.) The smallest pore clusters have been removed intentionally to investigate the algorithm effect on the residual connected pore networks. The numbers 1,3,10 represent the weight graduation in the "Bin command" implemented in AVIZO software and they were arbitrarily chosen. We will shortly point this out, too.

To 3.) As we point out on page 3445 and 3446 (pages 5 & 6), only the thickness (l) descriptor was calculated using equivalent sphere diameter, being the length (L) and width (S) taken from the Feret caliper analysis. This is the reason why though the pore clusters (ganglia) tend to have cube-like shape (See Fig. 11) they will never be one hundred percent spherical.

To 4.) True, we could do this – but it would only save round about $\frac{3}{4}$ - 1 page.

Technical comments (addressed in order as named by Referee #1):

- Generally, agreed. Here we want to point what has been done in the past and what should be done today, since most classical characterization is done "only" in 2D (e.g. by thin sectioning).

- You are right. We will delete the second part of the sentence.

- In the cited literature, they assume "pore types" such as "small, medium and large

pores" as a qualitative classification.

- Pore network detachment = separation of the connected pore network into individual pores.

- Agreed. This sentence needs to be rephrased to get the meaning it was intended to have. It should be like "...are essential to optimize and to study influencing parameter variations on laboratory measurements systematically".

- Yes, we are aware about that. We introduced the phrase "pore ganglia" intentionally to refer to the disconnected state of both. We would like to keep this definition and may point this out more distinctively in our introduction.

- We will add a short descriptive sentence about the "marker extent parameter" and refer to section 3.2 afterwards.

- Agree. We will do that.

- Agree. BE02b is the internal sample name, S1 the equivalent for the manuscript. We will cross check the entire manuscript in addition for any mix nomenclatures.

- We mean the "error for the determination of the particle thickness", and will add that.

- Agree. We will do that.

- Agree. We will do that.

- Agree. We will re-organize the sample description a bit in order to prevent from mixing styles.

- "Better-ordered" means better sorted and classified grains. We will rephrase that.

- Agree. We will do that.

- Agree, total fail by MS Excel due to rounding. We will change to 0.2057!

- Agree. We will change that accordingly.

- We need to remove "interactive", since this belonged to an early version of the manuscript and that has been missed by internal review. We use / register a 2D SEM image from the centre of the investigated CT-sample into the 3D volume. We cross check the high contrast and "sharp edged" SEM image with the threshold values derived by the algorithm in order to get a highly reliable segmentation. We can add an point this out more clearly.

- Agree. We will change that.

- Agree. We will change that.

- Agree. We will change that.

- Agree. We will change that.

- Agree. We will change that.

- Correct, we need to change that into "Bin command" instead of "watershed command".

- Agree. We will change that.

- Agree. We will change that.

- After some discussion. . . we concluded to remove that phrase which (again) seems to be a leftover of an old version of the manuscript.

- We will change the symbols in the figures. In fact, one ratio should be indicated by full, the other ratio by empty symbols. In addition, we will add a sentence to the captions to improve how to read the figures.

- Agree. We will do that.

- Agree. We will do that.

- Agree. We will do that.

- Agree. We have measured permeabilities and will try to address this as mentioned above in the introduction of our comments.

- Actually, there are different approaches to evaluate the MICP data, either by relating to maximum inscribed sphere or by equivalent spheres. Here, the last has been used. We can add references if needed.

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