Dear Editor,

we appreciate the efforts you and the reviewers have invested in our manuscript. Please find below point-by-point answers (marked blue) to the very helpful comments of the reviewers.

Sincerely,
on behalf of the authors,

Erik H. Saenger

Interactive comment on "Digital Carbonate Rock Physics" by Erik H. Saenger et al.

O. Lopez (Referee)
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Received and published: 31 March 2016

The overall quality of the paper is good but a number of minor revisions are needed to make it clearer and ensure good understanding. This paper presents a novel approach for estimating elastic properties of carbonate rocks combining laboratory measurements, imaging technique and simulations. The technique in itself is not new but the authors approach differs regarding the image segmentation to estimate unresolved porosity or unidentified phases and then the measured porosity is used for estimating rock properties.

Thank you for this general judgement.

As the editor mentioned, I strongly recommend adding some relevant references regarding previous major works regarding DRP and rock physics: Arns et al., (2002, Geophysics Vol.27), Derzhi et al., (2010, SPE 138586), Ringstad et al., (2013, EAGE) etc: : : The authors should compare their results with previous published works as per today DRP results regarding elastic properties are rarely matching experimental data and are often overestimating Vp and Vs.

We added the suggested and further relevant references.

Part of the author's technique is based on image segmentation into different classes. In Page 6, last paragraph should be improved for better understanding. What defines "global thresholds"? Why do you end up with 5 intermediate classes? Please make it more specific.

We modified the mentioned part of the text.

In page 8, first and second paragraph (line 10) should be clearer. It is difficult here to understand the Figure 7 description you made here. How do you end up with a minimum and maximum porosity for each subsample? You mentioned that you selected the "most relevant subsamples" on which criteria is based your choice (porosity only)? A table for both high and low resolution summarizing both calculated and experimental poro/perm will be maybe useful as it is difficult to understand why Figure 7 and 9 are so different.

Thank you for this comment. We modified the text of both paragraphs and also include an explanation how maximum and minimum porosity is defined with a reference to the corresponding discussion in section 5.2. Permeability results of high and low resolution samples should not be directly compared

as domain sizes are quite different (2.4 mm vs. 0.43 mm characteristic length of low and high resolution sample, respectively). We expect that the domain size of the high resolution sample is too small in order to be representative.

In page 10, Line 18 you highlight the scale issue which is well known from previous studies when comparing DRP and experimental study. Have you made any attempts to upscale DRP results to plug scale? Maybe citation of existing study could be necessary here to avoid misleading conclusions.

You are right, however, we do not have scans on the cm scale for our study. We include a reference to Ringstad et al. (2013).

Paragraph 5.1 you state that even with the highest resolution achievable you cannot resolve all the smallest pores which is true. But techniques exist to overrule these limitations as dry and wet imaging as described by Bhattad et al. (2014, SCA-2014-24) for example. This should be mentioned.

Thank you for your suggestion. We do not agree that the suggested technique will overrule these limitations. However, it is a possible technique to get useful approximations for practical use. Therefore we included the reference.

Paragraph 5.2 page 12, you write that "porosity values of carbonate using micro-XRCT will only provide estimates with relatively high uncertainty due to significant amount of unresolved pore feature in images". I do disagree with this statement; your approach based on single scale imaging is not suitable for proper porosity estimation. Numbers of published papers show the opposite (Lopez et al, 2012). You should be more specific here and mentioned that for carbonate having one image at a single resolution is not enough for porosity estimation.

Thank you very much for your comment. We agree that having one image at a single resolution is not enough for sufficient porosity estimation, but we also disagree about the suitability of the alternative approach presented by Lopez et al. (2012). In Lopez et al. (2012) it is assumed that "the Grey value is a function of porosity below the resolution of the image in the region of the grey value histogram between pore and solid voxels". Since the nature of this function is not further specified in Lopez et al. (2012) we resign any assumptions on such a relationship in this paper and split the grey values between pores and solid voxels into five intervals without any explicit porosity estimate (the only underlying assumption is that lighter grey values correspond to a lower porosity). We modified Section 3.2 according to this comment.

And this is what you work is supporting, that with a single image and doing some assumptions due to unresolved structures it is still possible to estimate some of the effective properties!

Thank you, we will point this out in more detail.

Paragraph 5.4, for the Vp and Vs it would have been nice to have the value at infinite resolution as described by Arns et al.(2002) in their Figure 4c.

Arns et al (2002) derive their "infinite resolutions" considering models with 120^3, 160^3, and 240^3 elements. Our approach is based on a model size of 400^3 and is already benchmarked in Andrä et al (2013). From our point of view the suggested approximation used in Arns et al (2002) and originally derived by Roberts and Garboczi (2000) should not be used in our case.

In summary, this paper demonstrates a new way of estimating elastic properties of carbonates containing micritic phases based on micro-XCRT and experimental nano-indentation. This is an elegant way to define moduli that are often not well known for non-pure minerals and use them for elastic properties determination.

Thanks for this evaluation.

Interactive comment on "Digital Carbonate Rock Physics" by Erik H. Saenger et al.

Anonymous Referee #2

The approach of using X-ray micro-CT in carbonates to obtain P- and V-waves is relative new and the idea of selecting distinct threshold values for the frontier between pores and solid matrix is interesting and relevant for the 3D image analysis, presenting a potential addition to the literature dealing with carbonate and digital rock physics (DRP).

Thank you for this general evaluation.

However, the paper is somewhat written in a confuse way and should be clarified and sharpened throughout the text to improve the understandability of the results.

We have considered all relevant comments to address this issue.

The authors should concentrate more in explaining and discussing their results, instead a big part of the paper deals with the results of other authors with named Tables/Figures (See e.g.: Page 3, line 15; Page 7, line 4; Page 13, line 5) which is inappropriate since results from the literature that the authors refer to should be included in the text of somehow in the paper' structure.

Page 3, line 15: Modified
Page 7, line 4: Modified
Page 13, line 5: Not modified; we regard this accuracy statement with an appropriate reference as adequate.

There is previous work by all co-authors. However, we are confident that the manuscript would not benefit from including much more details from these studies. Instead, the interested reader is invited to consult the references provided in the text. We hope we found a good compromise with the modifications listed above.

Even though it is important, there was a lack of connection between the literature and the authors' own results: what was new from the author's paper compared with the previous literature?

The whole manuscript has been revised carefully with respect to this comment. Not only the abstract should now be much clearer.

The discussion and conclusions were not very clear; for both sections there is a need of pointing out and correlating the values and results from the tables and figures in the text.

From our point of view the discussion and conclusions are now clearer. We considered all comments by the reviewer.

In addition, the theory of P- and S-wave velocities applied to the carbonate samples characterization should be elucidated. I suggest a previous definition, its importance related to DRP and the analyzed samples; provide a more detailed discussion between the experimental results and its practical applications.

We included an explanation how we derive P- and S-wave velocities by our numerical simulations in section 4.1.2. based on a standard workflow already applied in Saenger et al (2004,2011,2016).

Several comments have been incorporated along the text
(see below). Please take them into account (but not limited to) as much as possible.

Thanks for all the suggestions.

The title is too general; The Authors should rename the work to better show the focus
of their studies.

Thank you very much for this comment. However, in particular after considering all statements from the reviewers we are even more certain that the title is appropriate.

Abstract:
The abstract could be less general also some results (values) should be listed.

Modified

Page 1, line 15: Please list numbers to the resolutions;

Now we specify the range.

line 17: Mention briefly the properties complemented with nano-indentation;

We include a short statement.

line 20: By "intermediate phases" do you mean "intermediate threshold values for distinct phases"?

Thanks for the hint. Changed.

Lines 21-22: This structure is very confusing. To clarify I suggest the authors giving names to the technique/method used in the laboratory to measure porosity, to the predicted effective properties and to the technique used to acquire the experimental data;

We modified the corresponding sentence.

line 23: Specify that "some sub-samples" actually refers to the distinct smaller regions of interest (ROIs) selected from the acquired CT-datasets. I would also replace "in our case" to "analyzed rocks".

Modified.

Text: Page 2,

lines 7-9: When performing 3D images analysis a helpful tool to investigate
and verify the representative elementary volume (REV) of subsamples is
using autocorrelation function. Did the authors investigate REV of their samples somehow?

We modified the corresponding text and added some relevant references. However, from our point of view a detailed study of this aspect is out of the scope of this paper. Also, an auto-correlation function by its own will not allow verifying the REV.

The related literature can help: Haussener, S.; Coray, P.; Lipiȯnski, W.; Wyss, P.;
Steinfeld, A. Tomography-based heat and mass transfer characterization of reticulate

porous ceramics for high-temperature processing. ASME J. Heat Transf. 2010, 132, 023305:1–023305:9.
Petrasch, J.; Wyss, P.; Stämpfli, R.; Steinfeld, A. Tomographybased multiscale analyses of the 3D geometrical morphology of reticulated porous ceramics. J. Am. Ceram. Soc. 2008, 91, 2659–2665.
Haussener , S.; Steinfeld, A. Effective Heat and Mass Transport Properties of Anisotropic Porous Ceria for Solar Ther mochemical Fuel Generation. Materials 2012, 5, 192–209.
Costanza-Robinson, M.S.; Estabrook, B.D.; Fouhey, D.F. Resentative elementary volume estimation for porosity, moisture saturation, and air-water interfacial areas in unsaturated porous media: Data quality implications. Water Resources Research, 47, WO7513:1–WO7513:12.
Bear,J. Dynamics of Fluids in Porous Media, General Publishing Company LTD, 1972. pp. 19–21.

Page 2,
lines 11-13: In which type of material/rock? This statement can be invalid e.g., when analyzing other rock types such as shale;

We added two references with discussed examples and slightly modified our wording.

line 16: “3D rock models”? Maybe, “3D rock pore networks”.

We modified the wording to “3D rock pore structure models”

Line 19: It is not the porosity which is smaller, but the pore sizes;

You are right. Thanks for the hint.

lines 20-22: Once more authors draw a statement which is in fact strongly depending on the material/rock type and acquired voxel resolution. Please add rock type and resolution range to correct sentence;

We added a rock type as example and a corresponding reference.

line 28: rephrase sentence.

Rephrased.

Page 3,
line 2: Take out “as well”;

Ok.

lines 3-6: How did the authors managed to improve “digital rock images themselves and/or the computational workflow”? Describe it succinctly relating e.g., image enhancement with image acquisition parameters, voxel resolutions, pre- and post-processing;

This is the introduction of the paper. We describe our findings in detail in the following parts of the paper.

line 4: Correct the verb form;

Corrected.

line 6: name the “suggested techniques”;

This is the introduction of the paper. We describe our findings in detail in the following parts of the paper.

line 7: Complementary in which aspects? Authors should use
this structure to point out in more details the importance of their work and in which
aspects it is novel and relevant compared with the former cited studies.

Thank you for your suggestion. In contrast to other studies our digital rock physics study is complemented with a very detailed experimental characterization (section 2). Our suggested segmentation technique (section 3) is used to estimate effective mechanical as well as effective transport properties (section 4). Among others, we observe for mono-mineralic (calcite) carbonates a two-phase trend which can be regarded as an upper bound for velocities at all scales (see discussion in section 5) due to the observed self-similarity of those rocks (Jouini et al. 2015).

Pages 3-4, lines 26-7 and Tabs. 1 and 2: Remember using S.I. standard units and note
that the numerical value precedes unit and a space is always used (except for degree,
minute, and second for plane angle) to separate them.

Corrected. Thanks for the hint.

Subtitles are too short and should be improved making a least description of each
subsection.

After carefully considering your suggestion we find our subtitles to be clear with respect to the contents. Also, from our point of view subtitles should not be too long.

Page 4, line 14: “(RMS values)” should be moved to right after “1.4 _m”.

Corrected.

Page 5, lines 12-16: Authors mentioned Poisson’s ratio but do not say which values
they used for their calculation?

Now we include an additional sentence: “This local Poisson’s ratio cannot though be measured experimentally and we have taken here a constant value of 0.3”.

Fig. 1 is under explained, e.g., the blue and green
areas mentioned in the caption should be clarified in the text;

From our point of view the explanation in the caption is sufficient.

Explain also which is the relation/implication between the “blue and green”
areas and the nano-indentation results.

See section 5.1: Discussion on these results

Clarify the real mean/relevance of Fig. 1 to the paper context as well.

The “real meaning of Fig. 1” is elaborated in section 5.1. At this point of view we present the laboratory characterization by nano-indentation only.

Page 6,
line 1: Inform the source-to-sample distances in Table 3 and change “pixel size”
to “voxel size” adding the cubic unit to the values as well;

Modified.

line9: “illuminate”?

Modified to “remove”.

Line 12:
I suggest the authors take out Fig. 2 and only present this sequence in the text itself,
since this workflow is relatively simple and brings no novel information to the paper;

Thank you for your suggestion. We understand that for a reader well versed in this métier this figure may appear somewhat simple, however we prefer to not take out Figure 2. The simplified visualization of the workflow applied in this study is supposed to (1) inform the quick reader about the applied method (in contrast to a rather long description in the text) and (2) to break down the full workflow into simple single steps to make the method more accessible to readers from other disciplines.

Line 13: Give the voxel size of selected ROI.

This part of the text describes the general workflow; the voxel size for the given ROI can be found in other parts of the manuscript.

Fig. 4: Authors should describe the dark green areas, which are overlapping volumes between neighbors subsample ROIs, to improve understanding of their procedure;

Now added in the caption of Figure 4.

line 18: Keep a standard on typing: “subvolumes” or
“sub-volumes”, “subsamples” or “sub-samples”;

Modified to “subvolumes” in the whole manuscript.

Lines 20-22: “appropriate dimensions and kernel window sizes” which were?

We modified the sentence and now give the details of the used algorithm.

Page 7,
line 7: Were the same Carb-A and Carb-B samples investigated by Vialle et
al., 2013?

Yes, this is stated in the Abstract and in section 2.1:
“Both samples have been characterized in the laboratory in detail in Vialle et al. (2013).”

In positive case, I suggest the authors to add the values of Hg porosity and
compare it succinctly to the He porosity (shown in Table 1) and distinct CT porosities
obtained from the thresholds levels of micritic phases. This will give an idea of the
optimal threshold value which is surely related to the effective rock properties moreover
discussed in the work.

This is a good and attractive idea to use the Hg-porosity versus radius size data given by MICP to constrain the optimal pore size threshold from the CT data. However, it is challenging as MICP gives a pore throat (or access) size and not the actual pore size invaded by mercury. Hence the two “pore size” versus porosity data express different relationships that are difficult to correlate.

Line 14-15: give the used values for pressure bound condition
and dynamic viscosity of fluid;

In our numerical simulation delta p is -5.8 e-4 Pa/m an μ is 1.2 Pa s. The corresponding text passage has been modified.

line 19: what does the form “RSG” stand for? Note that

Corrected.

Fig. 6 was not commented in the manuscript text. If Fig. 6 isn’t that relevant to the paper’ findings it should otherwise be taken out.

In paragraph 3.2 we reference Figure 6. Figure 6 is important to illustrate the different segmentation classes.

Page 8,
line 6: What do you mean by “most relevant subsamples”? Give the criteria to judge a subsample relevant;

Corrected. Please also refer to our answer to this point to reviewer #1 (Oliver Lopez).

also do the authors mean by “numerical investigation” in this structure the P-and S-waves velocities? Please clarify! Because if one looks to the numerical investigation of permeability (Figs. 7 and 9) it is possible to see that simulations were performed in all 8 subsamples, while P-and S-waves velocity simulations are given only for one subsample (give the subsample names in the legend) of each carbonate (Figs. 8 and 10).

Corrected. We have improved the Figures according to this comment.

In Fig 7,
add “simulated” after “Intrinsic permeability” and
in the graphic axis (in Fig. 9 as well).

Modified.

Comparing the results of permeability simulations for the high resolution (Fig. 7) and the low ones (Fig. 9) one can see that only the minimum and maximum threshold values were depicted in Fig. 7. Please elucidate the reasons for that.

Now we explain the selection in Section 4.1.1.

Lines 11-18: Authors made a good observation and should justify this result better.

The paragraph is part of a results section. Justification or evaluation of results and observations should be part of the discussion.

Another interesting find when comparing Figs. 7 and 9 is the variation on the permeability results between subsamples: for the low resolution results less variation in the permeability is observed compared to the higher resolution, indicating less anisotropy of the subsamples and more material representativity. It is an important find in your study, you have it in numbers and you should highlight it!

As mention in the answer to the question of the first reviewer, we could not directly compare results shown in Fig. 7 and Fig. 9. The permeability results in Fig. 7 and Fig. 9 do not give any information

about anisotropy. We perform the Stokes flow simulations only in one direction (z-direction, see Fig. 3), which we choose identical to the direction of wave propagation. We have modified section 4.1.1 to clarify this point.

Observe as well
how the subsamples of Carb-B (high resolution) showed to be heterogeneous; even
though as the authors describe "it shows a much lower variation between the extreme
values", the subsamples have extreme variation in the permeability values compared
with subsamples of Carb-A. Which would be the probable causes for these results?

As mentioned also in the discussion, the high resolution samples are probably too small to be representative. The results in permeability calculations for different subvolumes, Fig. 7 left/right, show that fact. In addition, we can also observe that a variation in extreme values of porosity (for Carb-B high resolution, Fig. 7, right) doesn't seem to affect the permeability results. This could also be a result of a non-representative, i.e. too small subvolume/sample (e.g. a disconnected pore dominating the porosity value of the subvolume).

Line 18: This statement is half wrong!

Thanks for the comment. We fully agree because one statement could be misunderstood.
The high resolution samples don't seem to be representative as already mentioned above. Therefore large disconnected pores could dominate the porosity changes without contributing to the effective permeability.

Line 20: Here the "micritic phases" term is given
without a clear explanation that they actually are the distinct phases identified from the
threshold' classes of 3D images (as described in section 3.2). Please clarify it also
linking it to the Fig. 5.

We modified the corresponding text. Thanks for this hint.

The same is happening in section 4.2.1 when a new term "six
possible domains" is introduced.

We have modified the text to clarify this point.

Page 9,
line 12: In fact the threshold values are being varied what implies in the porosity
change!

Indeed the porosity changes. We want to calculate the permeability for the porosity variation of each subsample.

Lines 18-19: Make sure to inform that these results are shown in Fig. 11;

The sentence has been modified.

Lines 22-23: Rephrase structure;

Rephrased.

lines 25-26: Rephrase the position of "(Figure 10)" in the structure.

Rephrased.

Page 10,

lines 1-4: I disagree that only Carb-B showed slightly difference, which can also be seen in the P-wave results of Carb-A, on which data "a blue dashed-dotted line" should be fitted as well.

Ok; agreed. Now we include also "a blue dashed-dotted line" for Carb-A.

IMPORTANT: Note that if P-waves are represented with the blue color in Figs 8 and 10, captions must be corrected.

All captions are corrected according to this comment.

Lines 13-16: The performed procedure and described results are very interesting for a better discussion;

We added some sentences to explain the procedure in a better way.

lines 21-23: Make a link to it commenting the finds from Carb-B (Fig.7) and discussing in a practical manner how the present work overcomes this problem.

We modified the corresponding lines: "In general, a multi-scale approach as suggested by Ringstad et al. (2013) should be used for upscaling the results to the plug scale. However, our studies on Carb-A and Carb-B will suggest workflows which should be applied in practice for as many samples as possible for improving the statistical significance."

Page 11:
Section 5.1: Although the idea of correlating estimated elastic properties of carbonates based on distinct micritic phases identified from the threshold' levels in micro-XCRT images, with experimental nano-indentation experiments sounds very attractive, the authors failed in their expectations described in the lines 22-24.

We are not exactly sure why the reviewer points to some expectations we described in the lines 22-24. From our point of view we demonstrate and describe clearly that the results from indentation experiments are difficult to use in a direct way. However, as explained in the text, the results motivate us to use effective medium approaches at the image scale.

For a rock/material having a defined amount of pores and solid matrix, one can expect an image threshold with at least two peaks: one in the darker gray levels regions (related to the pores) and another in the brighter regions (linked to the matrix);

This is true. However, as shown in Figure 5, bottom row, right hand side, there are also examples where the peak corresponding to pores will not show up as clear as for other examples.

however If the analyzed material has also a certain amount of heavy phases (e.g. iron) then another additional peak in the threshold can be observed.

In this paper we only consider mono-mineralic carbonates (see Section 2.1).

Whereas (as the authors described very well) it is difficult to see the moduli peaks of pores in the nano-indentation experiment results, naturally because the values are very low.

Ok, thanks.

The relation from the micro-XRCT images and nano-indentation experiments using the number of threshold peaks seems somehow inappropriate.

In the manuscript we clearly state that: "Therefore the direct translation of moduli derived fom nano-

indentation remains also to be difficult". As mentioned above, the results from nano-indentation will suggest that the use of effective medium approaches on the considered image scales is appropriate.

Page 12,
lines 5-8: Include figures numbers (low and/or high resolutions) of your work to improve reading and understanding;

Now included. Thanks for the hint.

line 16: nane the technique used to the measured porosity or add "as shown in Table 1".

Modified.

In line 18: specify "full sample".

Specified.

Lines 22-24: Authors should be careful and add in this statement, that this observation is for their specific case (Carb-A and Carb-B) within the investigated resolutions which is based on the single image scales. Nowadays the use of multi-scale approaches to investigate porosity and DRP of heterogeneous rocks such as carbonates became widely common and has proving to be reliable.

We disagree with this statement. As stated in the paper we conclude that the porosity values of carbonates using micro-XRCT-data will only provide estimates with a relatively high uncertainty due to the significant amount of unresolved pore features in the images. The reviewer is kindly asked to give a reference where porosity has been determined successfully based on micro-XRCT-data **and** the procedure has been described in detail.

Page 13,
lines 2-3: name the tables/and figures from were readers can see these results;

Thanks for the comment. We have added the respective figure references in the text.

line 9: change to "experimental measurement".

Modified

Line 12: name the porous materials;

Changed to "porous rocks"

lines 15-16: How "statistically significant" (also given the Summary) samples should be? Try to base it on your results with the proposed approach using multi micritic phases and subsamples (ROIs).

The number of samples necessary to be "statistically significant" is chosen depending on the domain size. In case of the numerical simulation we have chosen 8 samples for the permeability calculations. We have clarified this point at the end of section 5.3.

Page 14,
section 5.5: the statement that "any significant anisotropy for permeability" was found in the analyzed samples is in disagreement with some of the paper' results (see e.g. Fig. 7). Elucidate the anisotropy changing from the higher to the lower

There are no anisotropy findings in Figure 7; our results on anisotropy are displayed in Figure 11. As explained in section 5.5 only a few samples are out of this trend which is displayed in Figure 7. This moderate anisotropy is regarded as not significant.

Concerning to the Summary:
Summary is in general written in a confuse way making
it hard to follow the author's thoughts. The summary should be rewritten in a more focused and brief way. Again, the authors provide their conclusions without backing them up with the quantified values that they base their assumptions on, making the work appear somewhat subjective. They tend to loose themselves in generalizations such as "the porosity of the rock samples is the most relevant parameter"; certainly the authors do not mean that for any purpose in the world including rocks porosity is the most relevant parameter, as an example for structures that need to be sharpened and detailed.

The Summary has been revised substantially according to this helpful comment.

Several references are missing, i.a.:

Page 2,
lines: 10-11,

Fusseis et al., 2014

14-15,

Andrä et al. 2013b

15-18,

Osorno et al., 2015, Saenger et al., 2016

22-24;

Andrä et al. 2013b

Page4, line 22;

Fischer-Cripps (2004), Lebedev et al. (2014)

Page 5, lines 3,

Fischer-Cripps (2004)

Page5, line 7;

Fischer-Cripps (2004).

Page 6, line 8 (reference the model used in the reconstruction);

We have given in the text the name of the software we used but have slightly modified the sentence to make it clearer.

Page 6, line 20;

From our point of view the reference "FEI Visualization Sciences Group" is sufficient.

Page 7, lines 7, 10;

The reference was given in line 9 “Osorno et al., 2015”

Page 13, line 27.

No reference required. We modified the sentence accordingly.