

Authors response on comments from anonymous Referee #1:

The authors would like to thank both anonymous referees for their comments and suggestions. We would like to comment on them point by point as recommended. Additionally, we would like to give a general outlook on the changes, which are implemented in the revised version of the manuscript.

General comment:

The authors present a regional study on petrographic and petrophysical properties of a layered sandstone from Israel. The paper is not meant to improve process understanding but aims at characterizing the rock with a broad mix of methods (laboratory, imaging, simulation). Therefore, the value of this paper is supposedly to provide a reference for future studies working in this rock formation.

The paper is a bit lengthy, but easy to follow for its good language and clear structure. Some more data could potentially be outsourced into the appendix. I can only comment on the imaging and flow simulation parts of the paper, as I have very limited expertise in geology. Hopefully, this is covered by other reviewers.

Authors' statement:

The main goal of the paper is to provide a comprehensive and multi-methodological case study on these particular Lower Cretaceous sandstones from the north of Israel as a fundamental base for future works. This goal is now stated explicitly in the abstract and in the introduction.

The implemented multi-methodological multi-scale approach allows also a better process understanding. Nevertheless, since we recognize that this aim and scope of the paper has not been fully presented as intended in the initial version of the manuscript, we are going to slightly change the title of the manuscript, clarifying that this is a fundamental case study ("Multi-methodological Petrographic and Petrophysical Case Study of Lower Cretaceous Sandstones from Hatira Formation, northern Israel"). Additionally, we shortened and re-organized the manuscript as described in our responses to both reviewers, in order to make it more accessible and informative for the potential reader of Solid Earth.

Detailed remarks:

Comment on line 200-213: It is unclear to me how many simplifying assumptions are in the Katz & Thompson approach to derive l_c , l_{max} and eventually k from MIP data. For instance, is the ratio $1/89$ defined for a capillary bundle model with a specific shape of the cross section, or some percolation-type network model? Some more information should be provided here or later in the discussion section.

Response: We added the following information regarding the approach in the Appendix. Katz and Thompson (1986, 1987) developed a permeability model derived from the percolation theory (Ambegaokar et al., 1971). The model is applicable for systems characterized by a broad distribution of local conductances with only short-range correlations, like those that occur in sandstone with broad range of size distribution of pore spaces. The constant $\frac{1}{89}$ resulted from a trial solution.

Comment on line 229-240: What software was used for the non-local means filter, converging active contours, etc.? How were the parameters for each processing step determined? Manual by expert knowledge and then kept constant for all three samples? Software information is also missing for variogram analysis (line 262).

Response: The code for Image Processing was designed by Kirill Gerke (in Acknowledgements) and received personally from him. The parameters were determined manually for each sample by expert knowledge, in order to derive the best possible results for the variety of sample material. Variogram analysis was performed using 'Variogramfit' Matlab package. This information appears now in the Methods Sect.

Comment on line 303-305: Please explicitly state which software was used to determine tortuosity. Somewhere later in the text it was mentioned that Comsol was used (line 594).

Response: 1) particle tracing after Stokes flow simulation was implemented with Comsol Multiphysics software (specified initially in lines 292-293 of the manuscript); 2) shortest path simulation through the main pore network with Fast Marching Method (Sethian, 1996, former lines 303-305) was implemented with Matlab using Accurate Fast Marching plug-in. We have added the names and references to all of used softwares within the Methods section.

Comment on line 308-313: How where the number of grains determined? By watersheding on the distance map of the binarized grain images?

Response: Separation of grains from the sample cluster was performed using distance map followed by watershed algorithms (MorphoLibJ plugin, Legland et al., 2014). This clarification is inserted in the manuscript at the introduction of the image analysis.

Comment on line 350: Unclear, which method was used to capture Fig 3(i)

Response: Incident light microscopy has been used for Figure 3(i). We have added this information.

Comment on line 667: Meaning of the sentence unclear to me: "A possible : : :"

Response: This sentence is now re-phrased: A possible source for the clay (0.8 %) is pressure solution (Fig. 5d).

Comment on line 732: I'm well aware of the partial volume effect, but it is unclear how you can quantify surface roughness from the volume fraction of unresolved porosity. Please explain.

Response: We withdrew the discussed paragraph.

Comment on line 744-759: Since the number of grains N is always positive, the connectivity index should always have the same sign as the Euler characteristic. I wonder why all CI values are positive in Table 2 are positive. A well-connected pore network such as sample 3 should evoke a very negative Euler characteristic. Please explain in the text.

Response: The reviewer is correct regarding the sign of Euler characteristic. χ was modified to $|\chi|$ in Eq.7. Please see the additional detail in our response to the comment #6 of Referee 2.

Comment on line 911-912 and 930-931: So is it 84% or 50%? I guess one corresponds to S1 and the other to S3, but this needs to be made clear in the text.

Response: The reviewer is correct. 50% corresponds to S1 while 84% to S3. This has been clarified in the text.

Comment on "number of plugs per layer and method": How many replicates plugs per layer and method? Information appears sporadically at several occasions in the manuscript (e.g. line 391, 423, 476 Figure 10). Could you add this information at an appropriate location in one of the tables?

Response: This information has been added in Table 2.

Comment on Fig. 7(a) and Fig. 8: What's the information gain between Fig. 7(a) and Fig. 8? Maybe merge both figures into one.

Response: Figures 7a and 8 were merged to one figure. 7b was removed.