

Interactive comment on “On the path to the digital rock physics of gas hydrate bearing sediments – processing of in-situ synchrotron-tomography data” by Kathleen Sell et al.

Anonymous Referee #3

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This manuscript details the processing of x-ray scans of hydrate-bearing sands to derive high-resolution 3-D CT representations of the pore-scale geometry, which can then subsequently be used in numerical models to better characterize the geophysical properties of these materials. This manuscript has the potential to be of great value in terms of improving 3-D imaging of rock samples as well as improving our understanding of hydrate-soil interaction at the pore-scale. However, there are a number of clarifications that I feel need to be addressed to ensure the significance of this paper.

The main clarification I feel is required is related to Section 3. Further details are required to describe the underlying constitutive model that is considered in the numerical model. As noted by the other referee there are a number of rock physics models that

C1

have been developed to describe the interaction of the hydrate and soil grains, which give rise to significant differences in wave velocity at the reasonably low hydrate saturations used in your modeling. The wave propagation would depend not only on the respective elastic moduli of the components in the system but how these components interact. Details of the modeling, as in size of model, mesh size, how the discrete nature of particles and hydrate are modeled is lacking. These issues need to be addressed for this aspect of the paper to be worthwhile.

In addition there appears to be some ambiguity to exactly what phases are present in the samples tested. Your terminology of free-gas system and gas-enriched system does not clearly define the percentage of phases. Were both systems ‘excess gas’? or are you inferring the ‘gas enriched’ is ‘excess water’? Given the low velocity contrast between the two methods are they forming hydrate at the same location (relative to water) and therefore the minor variations in velocity are related to just reforming characteristics.

A final ambiguity I feel that needs to be addressed is the link between the title of this manuscript and the overall thrust of the paper. The title suggest that the focus of the paper is on the processing of the synchrotron data, however the conclusions focus nearly entirely on the success of the numerical modeling in determining seismic response.

I have attached an annotated manuscript highlighting some suggested grammatical corrections and areas where the grammar should be improved/revise.

Please also note the supplement to this comment:
<http://www.solid-earth-discuss.net/se-2016-54/se-2016-54-RC3-supplement.pdf>

Interactive comment on Solid Earth Discuss., doi:10.5194/se-2016-54, 2016.

C2