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Solid Earth
Discussions

Interactive comment on "Modifications to Kozeny–Carman model to enhance petrophysical relationships" *by* Amir M. S. Lala

Anonymous Referee #1

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Dear Dr. Rossetti,

Thank you for the opportunity to review "Modifications to Kozeny-Carman Model to Enhance Petrophyscial Relationships" by Amir Maher Sayed Lala.

The author presents a concise article that recasts the Kozeny-Carman relationship in two different ways: 1) to exploit pore size data and variable fluid path tortuosity and 2) to introduce polydisperse grain sizes into the Kozeny-Carman relationship. These models are then applied to porosity-permeability data of shaly sandstones to assess their suitability to modelling the data.

The article is generally clear and well written and the derivations of the modified Kozeny-Carman (KC) relationships are self-consistent (though there are a few errors, outlined in my major and minor comments below). However, the KC equations that

employ pore size data appear to be of little general use, since they have been empirically calibrated to fit the author's dataset, negating the purpose of the model. Further, the equations do not appear to do as good a job of fitting the author's data as the KC relationship that employs a percolation threshold (see below). In contract, the KC relationship proposed for a polydisperse grain size (the example used here is clay and sand) is more generally derived and, thus, can be applied to other datasets. However, this equation still requires the use of grain size data and so its novelty is reduced in light of the author's thesis, which purports to provide a KC relationship that is not dependent on grain size data.

Furthermore, the author discusses a dataset that has not been previously peer reviewed, without presenting it in the current manuscript. The scientific methods used to obtain these data and the data, themselves, must be provided.

The manuscript would be made clearer by a discussion that compares and contrasts the various models presented therein. I believe that this would be a useful addition to this manuscript to better highlight why the author recommends their use. I encourage the author to go further in their explanation of how these models should be applied. For example, what are the various pieces of information that are needed to adequately use the models presented: pore size, grain size, porosity?

In general, the manuscript requires significant modification to prepare it for publication. The author can do more to highlight why their work is relevant to the wider scientific community and why it is novel.

Below, I have included some major comments followed by more minor line comments. I hope that the author will find them constructive and helpful.

Major comments:

1) The derivation of the modified Kozeny-Carman relationship is confusing due to the author's choice of terminology, specifically, the use of a specific surface area term, S.

At first glance, this term does not appear to be consistent with the general definition of specific surface area in the broader literature. Specific surface area, S, is the surface area of a material per unit mass and has units of [m2/kg]. The S term used by the author in this manuscript is defined as the ratio of the total pore surface to the total sample volume (though I believe this should be total pore volume), with units of [1/m] and appears to be a reformulation of specific surface area. This term seems to be consistent with the hydraulic radius, rH, which is generally defined as the ratio between the cross-sectional area of a channel and the perimeter of the cross-section in contact with the flowing fluid. Is this true? Could the author please reference the source of this derivation of specific surface area to avoid confusion? Also, it would be useful for the general audience if the author could clarify the steps needed to transition from the more general form of specific surface area to the form used in the present manuscript.

2) In the section entitled Kozeny-Carman Equation with Pore Size, the author includes the pore diameter in the Kozeny-Carman relationship, as well as a variable tortuosity term. The author employs the work of Grathwhol (2001) and Berryman (1981) for this tortuosity term, but empirically modifies these expressions to fit their dataset. I find this problematic for several reasons. Firstly, the Kozeny-Carman relationship is, fundamentally, a model that tries to use physical parameters that can be measured in the laboratory (such as porosity, grain size/pore size, specific surface area) to predict permeability. Modifying such an expression empirically to force the relationship to fit a specific dataset is counter-productive and not of scientific interest. The parameters of the modified KC equations given in Eqs. 14 to 17 have no apparent physical meaning and, thus, do not provide meaningfully parameters to describe the measured samples, nor can these equations be applied to other datasets. Secondly, it is unclear to me why the formulation of these equations was deemed necessary by the author. As shown in Figures 1 and 2, the Kozeny-Carman relationship (modified for a percolation threshold) does a rather good job of describing the 'Rudies data' (Note: please see my comments below on the use and presentation of this data). By contrast, Eqs. 16 and 17 do a poor job of modelling this data. Further, there is no discussion of how the term Do was

chosen (or measured).

3) The author applies their derived equations to 'the Rudies data' (Line 116). There is no introduction given for the Rudies and Matullah Formations and the porosity-permeability data for these formations is not given in the manuscript. This is particularly egregious because these datasets are discussed at length in the 'Results and Discussion' section (with a cursory reference to the author's MSc thesis) but the data do not appear to have been previously published in a peer-reviewed article. If this is the case, a description of the formations and the datasets (in tabular form) should be given in this manuscript. Specifically: 1) what is the lithology and composition of the rock (this is not explicitly stated), 2) where do the samples come from, 3) why are these rocks of interest, and 4) how was porosity and permeability measured. Furthermore, where do the data for the Kharita member and Bahariya Formation (in Figure 6) come from and how were these data acquired. The source of these data must be referenced or, if the data are new, must be provided.

4) The article is currently very poorly referenced, given the extensive body of literature that has used and modified the Kozeny-Carman relationship. A conspicuous omission is the work of Costa, 2006 (GRL), which reformulates the Kozeny-Carman relationship in terms of a fractal pore space geometry. I believe this article would be of particular pertinence to the present manuscript. I have included other references that may be of use in my minor comments below.

Line comments:

Line 39: Q in Eq. 1 is the volumetric flow rate, not the volumetric flux. Volumetric flux is the volumetric flow rate divided by the cross-sectional area (i.e. Q/A) and is denoted by q. It has units of [m/s]. Please correct this in the rest of the manuscript; I have found the same mistake in lines 58, 61, and Eq. 2.

Line 47: Darcy's Law also assumes laminar fluid flow, which may be difficult to maintain in some geological materials. In many cases, fluid flow is not laminar and permeability

requires a correction for the Forchheimer and/or Klinkenberg effects.

I would further rephrase this statement to say that permeability is a fundamental rock property and remains constant, so long as the sample microstructure is unchanged – this is the reason that permeability is independent of the fluid type and the pressure conditions. Saying that permeability does not depend on fluid viscosity or pressure difference is technically correct but leads to some confusion in the text since Eq. 1 requires both these terms – should either of these terms change in Eq. 1, k inevitably changes as well. It is important to highlight the caveat that there are cases in which a high pressure difference may give rise to turbulent flow and the measured permeability is lower than the true permeability – i.e. the Forchheimer effect. Further, water and gas pore fluids interact differently with clays, yielding different values of k.

Line 48: I would avoid specifying which units are 'most commonly used' since this is not consistent across fields of study. Stick with SI units.

Line 51: Same as Line 48. Also, if the author would like to keep this sentence then please, at least, specify the 'industry'.

Line 53: Cite Kozeny, 1927; Carman, 1937; Guéguen and Palciauskas, 1994; Bernabé et al., 2010.

Line 61: Does the author mean volumetric flow rate [m3/s] (as seen in Eq. 1) instead of volume flux [m/s] (Eq. 2)?

Line 74: How is specific surface measured and why is this too difficult to do? Please specify the difficulties of measuring S in rocks to justify this statement.

Line 75: How can grain size be determined? Please give examples.

Line 80-81: "This operation is inconsistent with the KC formalism but it is useful." Please justify this comment.

Line 82: Please specify that these grains are spherical.

Line 82: How are the grains packed? This will influence the surface area and porosity.

Line 87: Provide a reference for this statement.

Line 116: What is the Rudies data? Up to this point there has been no discussion of it and no given reference. Further, which 'respective theoretical curves' is the author referring to?

Lines 119 to 124: Please provide references for this entire paragraph.

Line 134: Is the author referring to Eq. 9 here? Please specify the 'KC', which is being referred to. Please also reference a figure that supports this statement. Is ÏŢ=0.3 for the Rudies formation?

Lines 137 to 144: These modifications to Eqs. 10 and 11 must be justified. The author is applying an empirical correction based on a single dataset, essentially forcing a fit with no theoretical justification. These equations are, thus, only applicable to the dataset which was used to calibrate them. What is their wider application and what can we learn from them? Further, how are Do and D determined?

Lines 155-157: How are Eqs. 16 and 17 'classic clay free trends'? These are equations that have been empirically modified in the previous section of the manuscript.

Line 206: Mavko and Nur, 1997 suggest this in their article, please provide a citation.

Lines 209-211: Please remove the text in the parentheses – it is not related to the thesis of the manuscript.

Lines 214-215: What does the author mean by this statement? If the data is well defined by the sample porosity and the grain size, is there any need for the author's derivation of a KC relationship that takes into account pore diameter?

Line 223: I would argue that Eq. 17 does a very poor job of describing the Rudies data, compared to Eq. 6. The author should discuss why they think Eq. 17 does a better job of describing the data than the more general Eq. 6.

Lines 225-229: The pore size version of the KC relationship presented here appears to do a poor job of describing the Rudies data (Eq. 17, in particular) compared to the grain size derived relationship. While Eq. 16 appears to do a good job of describing the Rudies data at low porosity, it overestimates permeability at high porosity. Further, I would argue that determining grain size using image analysis is just as simple as determining pore size from image analysis.

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