Authors response to Interactive comment on "Archie's Law – A reappraisal"
By P. W. J. Glover

Referee #2 – Harald Milsch
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This document is a response to the comments made by Referee 2.

Referee's comment: In this paper the author investigates differences in derived cementation exponent m when applying, both, Archie's first law (1942) (EQ1) and a modified form introduced by Winsauer et al. (1952) (EQ2) that includes one additional adjustable parameter a. For this purpose, an analysis of a large dataset comprising formation factor and porosity for over 3500 core plugs from 11 sandstone and carbonate reservoirs was performed. Furthermore, a sensitivity analysis was conducted to determine the impact of measurement errors in porosity, pore fluid conductivity, and temperature on the precision of m from EQ2 with respect to EQ1. The author reaches a number of conclusions summarized in Section 7 of the manuscript where, in my perception, the most important are that (1) EQ2 should perform better than EQ1 and (2) that this may be explained by one or more of the measurement errors stated before. The author carried out an important task with implications for fundamental rock physics and industrial applications alike. The paper may definitely be suitable for publication in Solid Earth (SE). However, there are a number of major issues outlined in the following that I encourage the author to address before the paper can be recommended for publication.
Author's response: the author thanks the referee for his kind words and understanding.
Changes to manuscript: No explicit changes required.

General comments:

(1) **Referee's comment:** Equations: Despite being - potentially - theoretically founded (e.g. Glover, 2009) it should be noted that EQ1 is an empirical correlation, as is EQ2. It is therefore unclear what defines "quality" of EQ1 with respect to EQ2. If this were the R2-parameter of the respective data fit - clearly - the difference is marginal (Fig. 3). Also, when fitting the data with EQ2 (i.e., the a-parameter) this affects m with respect to EQ1 (Fig. 3). In my opinion this is a mathematical result and not some indication of physical quality. What is significantly more important than the negligible differences in the two fits are the (natural)

variations in, both, F and m with respect to porosity (or vice versa) within each of the reservoir types (Fig. 1). This should be discussed not least with regards to errors in reserves calculations.
Author's response: I have a slightly different view from Harald on this point. To me it is very clear what defines the quality of Equation 1. It is the fact that this equation is dimensionally correct whereas Equation 2 is not, and therefore cannot be accurate. This is a fundamental position that has nothing to do with any differences in fits, whether they be negligible or not. It is clear to me however that modifications could be made to the paper to make the position clearer, which I have done.
Changes to manuscript: I have introduced a sentence describing how Equation 1 was initially empirical, with recognised theoretical special cases. I have given a reference to a wide discussion on the matter. I have also mentioned that Equation 1 has now been shown to derive from other mixing models (The Lichteneker-Rother equation). It may not yet have a completely theoretical pedigree, but is at least dimensionally correct. I have also inserted a sentence explicitly stating that it is the paradox that exists in the limit value of 100% porosity which leads to Equation to not being considered to be as high a 'quality' as Equation 1. These are points of mathematical quality. The whole point of the paper is that despite the differences in their mathematical quality the small differences in their physical quality can lead to significant differences in derived cementation exponent, with the better fits occurring, paradoxically, for the model which theoretically ought not to be the best model.

(2) **Referee's comment:** Data: As already stated by Anonymous Reviewer #1 it is very unfortunate that the reservoirs are geologically unattributable, not least in connection with potential sources of surface conduction. It even is unclear if the data has been acquired by the author himself or collected from some external source. In any case it should be explained how the measurements have been performed. This also applies to the fluid exchange experiment introduced in Section 5.2. The experimental procedures require more emphasis and the results remain rather qualitative. I had expected some information on changes in fluid chemistry, for example. Not least, the choice of Boise sandstone is unfortunate as it contains ca. 40 % of non-quartz components including ca. 10 % of clay minerals. As a result, the outcome of the experiment is likely to be very different when, as in the data, clean sandstones are concerned.
Author's comment: as mentioned by the other reviewer, this is an unfortunate situation. The original paper indicated that there were no variable surface conduction sources, and I agree that it was not clear from the original paper who did the measurements. I have clarified that

in the revised paper. I also needed to clarify some of the experimental procedures. The choice of the Boise sandstone however, is not erroneous. Our samples have 80 to 90% quartz and 10 to 20% felspar and mica, with very little clay. We also have explicit measurements of surface conduction, in a paper that is already published, and have cited it in this paper.

Changes to manuscript: Initially, please refer to those changes that I have made response to Referee 1. Secondly, the revised paper now explicitly states that the samples are from relatively clean clastic reservoirs exhibiting a low degree of surface conduction. Thirdly changes made in response to Referee 1, making clear that the data was measured by an oil service company(s). The typical experimental procedure required by the reviewer has been inserted into the text. The fluid exchange experiment is described in another paper, and that paper has now been cited. The Boise sandstone samples used in the measurements had an 80% quartz content, with the remainder being made up of feldspar and mica and very little clay. Consequently our samples are a good match to the relatively clean sandstone data presented in this paper.

(3) **Referee's comment:** Experimental Errors: With what has been stated before it is impossible to judge whether the experimental errors in the data, as claimed by the author, are real or simply presumed. This is crucial as otherwise the reasoning that EQ2 works better than EQ1 is unsubstantiated. Furthermore and despite the sensitivity analysis being nicely conducted, how the errors in the measurements had "miraculously" been compensated for by the a-parameter remains absolutely unclear.

Author's comment: There are two points here. First, that Eq 2 works better on the data in this paper than Eq 1 is shown by the statistical analysis and does not need to be substantiated by finding a cause. The point of the paper is to try to understand why. This paper examines a number of possibilities and concludes that systematic errors are the likeliest cause, which needs clarifying.

Second, the assertion that "the errors in the measurements had "miraculously" been compensated for by the a-parameter" is, in retrospect, not justified in the original manuscript. It might reasonably be said that I pulled it out of a hat. In fact, it arises directly from the equations, so perhaps an applied mathematician would be better than seeing it than the average reader of the journal. Consequently, I have added a four justification of this with a new figure.

Changes to manuscript: First, material has been added to clarify that an analysis of the data bears out what generations of petrophysicists have felt.

Second, the assertion that “the errors in the measurements had “miraculously” been compensated for by the a-parameter” has been significantly strengthened in the paper. I have added over 1100 words and a new figure to explain exactly why and how the *a*-parameter works as a compensation the systematic errors. I thank Harald for pushing me into making this explicit, because in doing so I now understand it so much better.

(4) Specific comments:

Referee’s comment: - Section 5.3: Fluid conductivity below say 100C changes by ca. 2.3 % per 1C! This is also what Fig. 7 shows. From Fig. 7 it is implied that the error in fluid conductivity for 25C with respect to 20C is ca. 12 % and not 20 % as stated in the text.
Author’s comment: My error.
Changes to manuscript: Now corrected in the MS.

Referee’s comment: - The manuscript contains significant amounts of repetitive or redundant statements, in particular with respect to what is initially stated in the abstract and ultimately concluded in Section 7. This should be improved.
Author’s comment: While it is the job of the abstract to repeat important results in the body of the text, I recognise that there is some unnecessary repetition.
Changes to manuscript: Some repetitive material has been removed from the MS.

Referee’s comment: - Section 3 should be included in Section 6 and the latter should be more logically organized with respect to the principal findings outlined in the previous sections.
Author’s comment: It makes no sense to incorporate Section 3 in Section 6 because the material in Section 3 relates to the initial analysis of the data and its implications for reserves calculations, while that in Section 6 relates to the reasons for a discrepancy between the two forms of Archie’s equations. I feel that they are more elegantly and logically placed as they stand. Furthermore, Section 3 acts as an introduction to sections 4 and 5. Making the changes suggested would upset the whole logic of the paper.
Changes to manuscript: No change made.

Referee’s comment: - Text in Sections 6 and 7 should not be presented as a list of statements.
Author’s comment: I feel that this is a matter of style. I prefer the more concise format. I would like to have the editor’s view on this. In the meantime I have changed the format to a more prosaic one.

Changes to manuscript: Format changed.

Referee's comment: - The author should choose between "resistivity" and "conductivity", e.g., in Fig. 6 and its caption.
Author's comment: While I agree with the general comment and have made modifications, some of the data is better displayed in terms of resistivity and some in terms of conductivity. Nevertheless, each independent point made by the paper now is discussed solely in terms of resistivity or in terms of conductivity.
Changes to manuscript: Figure 6 has been corrected to read in conductivity.