

Author's response to reviewers: Ref: SE-2019-55

Title: "Uncertainty in fault seal parameters: implications for CO₂ column height retention and storage capacity in geological CO₂ storage projects"

5 Our manuscript has benefited from two reviewers, Graham Yielding (R1) and Katriona Edlmann (R2). In the following we refer to the manuscript that was submitted for review as the "original manuscript", while the edited version which incorporates the reviewers comments is referred to as the "revised manuscript".

In the following the reviewer's comments are in *italic Arial* while our responses are in Times New Roman.

10

Author Comments to Reviewers for the open discussion.

To R1:

Dear Graham Yielding,

15 We are grateful for your detailed review of our manuscript. The constructive suggestions and industry insights have been very helpful and have improved and strengthened the revised manuscript. In response to the reviews received we have made a number of changes which are outlined in the "response to reviewers" which has been uploaded as supplementary information along with the revised manuscript. We hope that the revisions sufficiently address the points raised.

Kind regards,

20 Johannes Miocic

To R2:

Dear Katriona Edlmann,

25 We are grateful for your review of our manuscript. The constructive suggestions have been very helpful and have improved and strengthened the revised manuscript. In response to the reviews received we have made a number of changes which are outlined in the "response to reviewers" which has been uploaded as supplementary information along with the revised manuscript. We hope that the revisions sufficiently address the points raised.

Kind regards,

Johannes Miocic

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R1

In a number of places, starting at the top of p.4, the units of IFT are incorrectly stated as mN/cm instead of mN/m - the values quoted make it clear they should all be mN/m. This should be corrected throughout the ms.

35

This has been adjusted in the revised manuscript.

Also at the top of page 4, I do not recognise the stated depth variability for hydrocarbon IFT. Firstly, IFT is not "fairly constant" but varies with depth. Oil IFT is stated to decrease with depth but in my experience it increases with depth - see Figure 9 of Yielding et al 2010, uploaded - from 25 mN/m at the surface to about 40 mN/m at around 3km. For methane, IFT decreases from around 75 mN/m at the surface, also to around 40 mN/m at 3km (see same figure).

We thank the reviewer for their comment. In the original submission the depth variability of hydrocarbon IFT was based on data from Schowalter (1979) and Watts (1987) and, as in the literature recommended by the reviewer, the IFT data in these publications does change with depth. The "fairly constant" statement of the original publication was supposed to illustrate that the IFT values for hydrocarbons are fairly constant at reservoir depths (1000-2000 m depth) when compared to CO₂ at these depths. However, we realise that this statement is misleading and have changed the section in the revised manuscript as follows:

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"For HCs the wettability parameters IFT and θ vary with depth, particularly large changes occur between surface and conditions found at depths of 1000 m. IFT of oil increases from around 25 mN/m at very shallow conditions to around 40mN/m for conditions commonly found in reservoirs at 2.5 km depth (Yielding et al., 2010). For methane IFT is around 70 mN/m at surface conditions and decreases to 40 mN/m at subsurface conditions (Firoozabadi and Ramey, 1988; Watts, 1987). The contact angle for HCs is commonly reported as 0°(Vavra et al., 1992), simplifying equation 2 as the cosine of 0° is 1. However, for other fluids such as CO₂ the wettability parameters IFT and θ are even more pressure and temperature dependent."

55

On Figure 3, it is not clear to me how the CO₂ lines have been derived for the two 'empirical' equations (Bretan & Yielding). I can see that the oil and methane lines differ appropriately because of their different density contrast with water. On density difference alone however, the CO₂ line should be between oil and

gas, so I assume the CO₂ line is reduced because of either IFT or CA input or both (in a manner similar to eqn 10). However, both the Bretan & Yielding equations were based on agglomerated oil AND gas data (i.e. the data pool is blind to fluid type), so it is not clear to me what generic 'hydrocarbon' values for IFT & CA have been assumed, in order to compute the CO₂ values via eqn 10. I request that these hydrocarbon IFT and CA values be added to the caption (already contains the CO₂ values).

The reviewer is correct in pointing this out. We have added the IFT and CA values to the figure caption. Additionally, we have changed the assumed IFT values for both oil and methane in the calculation of the column heights to be in line with the values suggested by the reviewer in the previous comment (IFT for methane of 60 mN/m and 30 mN/m for oil). Note that for the resulting column heights the difference between methane and CO₂ columns is even larger.

Related to the above point, the model results in D & G of Figs 5 & 6 report the effect of different ranges of wettability when using the Bretan & Yielding eqns. However, as you mention in the Discussion, these equations were derived as the maximum of observed data in oil & gas systems - so arguably they correspond to one extreme of the wettability range for hydrocarbons (least wettable). Depending on how that 'least wettable' state relates to your average CO₂ wettability, I'm not sure if you are right to look at an uncertainty range which is symmetric about your chosen mean for CO₂. Maybe I'm over-thinking this, perhaps you can offer a simple clarification.

We thank the reviewer for their comment. Indeed, when using the Bretan and Yielding equations the resulting column heights represent the maximum column heights instead of the average column heights resulting from the Sperrevik equations. Thus, where the Bretan and Yielding equations are used (Fig. 5&6 D&G) to analyse the influence of wettability uncertainty on column heights, we model around the least wettable state. Due to a lack of wettability data we use a normal distribution around this "least wettable" state. A more skewed distribution may better reflect the range in potential wettabilities, but this is not what we have considered here. We have clarified this in the revised manuscript by adding the following:

"Note that equations 5 to 7 result in maximum column heights (or minimal wettability) while equations 8-9 give an average column height."

And

95 *“IFTs of 38 mN/m and 34 mN/m, and CAs of 50° and 70° are used as mean wettability for the MCMO models of reservoir A and reservoir B, respectively, based on the IFT-depth and CA-depth relationships of Iglauer (2018). For models where the approaches by Bretan et al (2003) and Yielding (2010) are used, these correspond to the mean least wettability.”*

100 The comments by the reviewer offer some intriguing ideas about how future models could be developed by using the whole input data of individual sealing faults from the Yielding and Bretan algorithms. These could be used to calculate the distribution of buoyancy pressures for a given SGR and then translating the buoyancy pressures to wettability (or pore throat size) distributions. If this could be combined with other geological data, possibly new fault seal algorithms could be developed.

105 *In the MC modelling, do you assume that all input parameters are independent? Is this valid? (E.g. do IFT and CA co-vary for CO₂?).*

110 The reviewer points out an important point: theoretically IFT and CA should co-vary, with high IFT values corresponding with low CAs and the other way around. However, for the CO₂-brine-rock system the existing data in our opinion is too scattered to allow for a model in which they are dependent. We have added text to the revised manuscript to highlight this point as follows:

“The input parameters, which are all treated as independent, are derived from the published data described:...”

115 *In the MC modelling, I don't think it is mentioned in the text (or tables) what value of pore-throat size is used. A value of 100 nm is mentioned in the caption of Figure 7 but it isn't clear if this is used for all other models. This is an important point, because in general in oil industry fault seal analysis it is believed that the increasing seal capacity with increasing phyllosilicate content is overwhelmingly due to a corresponding decrease in pore-throat size. Pore-throat size is observed directly in microscope analysis and also back-calculated from Hg-air injection tests. Thus your results in Fig.7, showing smaller column height for phyllosilicate-rich fault rock than for qtz-rich fault rock because of changing CA, might be countered by the qtz-rich fault rock having larger pore throats. Maybe include this pore-throat variation in the input parameters to the Fig.7 models?*

120

125 We thank the reviewer for this comment, we have added the pore-throat size to the MC-modelling description in the revised manuscript. As suggested we have also added pore-size variations to the models 55-59 (two additional models) and these are shown in Figure 7. The new outcomes are discussed in the revised manuscript.

130

p1, line 15, should be "assess"

This has been changed accordingly in the revised manuscript.

p1, line 16, "As with" might be better than "In similarity to"

This has been changed accordingly in the revised manuscript.

135 *# p1, line 25, omit ";" after storage*

This has been changed accordingly in the revised manuscript.

p2, line 23, replace ";" by ":"

This has been changed accordingly in the revised manuscript.

p2, line 24, model not models

140 This has been changed accordingly in the revised manuscript.

p2, line 27, omit "potentially"

This has been changed accordingly in the revised manuscript.

p3, line 17, replace "lighter" by "less dense"

This has been changed accordingly in the revised manuscript.

145 *# p3, line 23, replace "transport" by "trapping" (transport is controlled by permeability)*

This has been changed accordingly in the revised manuscript.

p4, line 9, replace "higher" by "greater"

This has been changed accordingly in the revised manuscript.

p4, line 22, replace "catalases" by "cataclasites"

150 This has been changed accordingly in the revised manuscript.

p4, line 24, insert "typically" before "> 1km"

This has been changed accordingly in the revised manuscript.

p4, line 26, replace "with" by "in"

This has been changed accordingly in the revised manuscript.

155 *# p5, line 24, insert "and" before "clay bed"*

This has been changed accordingly in the revised manuscript.

p5, line 24, replace "do" by "does"

This has been changed accordingly in the revised manuscript.

p6, line 15, replace "sometimes" by "typically"

160 This has been changed accordingly in the revised manuscript.

p7, line 30 onwards... I was confused by FRC (fault rock composition) and FRCC (fault rock clay content). I think you only need one of these, make it consistent in text and tables.

This has been changed accordingly in the revised manuscript.

p9, line 3, insert "(see Figure 7)" after "57"

165 This has been changed accordingly in the revised manuscript.

p9, line 26, insert "(and hence smaller pore-throat radius)" after "content" (see earlier comment)

This has been changed accordingly in the revised manuscript.

Fig.4 - label the contours explicitly, to save the reader having to work out their values.

This has been changed accordingly in the revised manuscript.

170 # Table 1, last column. I assume the heading should be a lower case sigma, for standard deviation? The upper case symbol confused me for a while.

This has been changed accordingly in the revised manuscript.

175 **R2**

In your abstract your statement of your findings around line 15 is not as clearly stated as it could be – especially with regards to your finding that fault composition is important and it may be worth moving your sentence beginning “In contrast to hydrocarbon systems higher phyllosilicate. . .”. To before your sentence beginning “However, the wettability of the carbon dioxide system is highly sensitive. ..” only to make your point that increasing phyllosilicate may not result in the expected increase in fault sealing for a co2 system.

We have changed the abstract in the revised manuscript accordingly to the reviewer’s suggestions.

185

Page2 line 2 – while I agree that faults are ubiquitous in sedimentary basins, I would like to see a little more information provided on their extent, distribution, and scale, especially the impact of faults that are below seismic resolution.

190 We thank the reviewer for their comment and have added additional information on the impact of subseismic-resolution faults to the revised manuscript as follows:

“The scale and distribution of faults depends on the type of sedimentary basin and its geological history. In particular faults that are below the resolution of seismic surveys cannot be avoided (Maerten et al., 2006; Le Gallo, 2016).”

195 Page 2 line 10 – faults can provide fault parallel flow for impermeable units as well? fig 1c if you are suggesting flow through the fracture – if you are not suggesting fault flow then you may need to reword this section and figure.

200 The reviewer points out an important point. Indeed faults can provide fault parallel flow through fracture networks within otherwise impermeable units. We have updated the text describing this in the revised manuscript as follows: “... (iii) faults can provide fault parallel flow through fracture networks in otherwise impermeable rocks linking separate permeable units...”.

Additionally, we have adjusted the caption of figure 1.

205 Page 2 line 28 – “will accumulate underneath the flow barrier until breakthrough occurs due to the increase in pressure within the reservoir” what do you mean by breakthrough? Capillary, spill point, induced fracturing? Can you be clearer with this statement?

We have changed the statement in the revised manuscript accordingly as follows:

“The fluid will accumulate underneath the flow barrier until capillary breakthrough or, less frequently, induced fracturing occurs due to the increase in pressure within the reservoir.”

210

Page 3 line 30 – perhaps add the word seal before “rocks.”

This has been changed accordingly in the revised manuscript.

215 Page 4 line 6 – you slightly contradict your statements at the end of page 3 where you infer fine grained rocks have small pore as page 4 states “Due to the heterogeneous nature of rocks the size of pores within the sealing rock (fault rock or cap rock) varies”

We have changed this sentence in the revised manuscript to highlight that the pore sizes within caprocks generally only varies slightly. In the revised manuscript it reads as follows:

“Due to the heterogeneous nature of rocks the size of pores within the sealing rock (fault rock or cap rock) varies to a certain degree and thus two capillary pressures can be defined.”

220

Page 8 line 5 – I would remind the reader that equations 5 to 9 are the three different author approaches (Bretan etc) as that is how you refer to them in the subsequent modelling discussions.

This has been changed accordingly in the revised manuscript as follows:

225

“Capillary threshold pressures for fault seals are calculated by using equations 5 to 9 (the algorithms by Bretan et al. (2003), Yielding (2010) and Sperrevik et al. (2002)), these are then converted to the CO₂-brine system using equation 10, and subsequently column heights are calculated assuming a pore-throat size of 100 nm (eq. 3).”

230

Discussion section – I would like to see a clearer statement of your findings and a more logical layout to your discussion as it is quite challenging to pick out the pertinent information rather than saying it “strongly influences” – I would like to see clearer statement of what the influence such as increasing phyllosilicate fault rocks results in one third less CO₂ stored - to make your findings clear from the start of this section which can then lead onto the detailed discussion of the reasons why.

235

We thank the reviewer for their suggestion and have reworked the discussion section in the revised manuscript.