Questions and comments for papers #1 to # 15: DPE 190 papers

Paper#1 Lenormand, R., Toubol, E., Zarcone, C.: "Numerical models and experiments on immiscible displacements in porous media," J. Fluid Mech. 189 (1988), 165.

1. One of the best papers I have read but would like to report a clear printing mistake in page 173 in figure 7 – the saturation value in the viscous fingering region should be 0.28 not 0.54.

Correct.

Paper# 2 Lenormand, R., Zarcone, C., Sarr, A.: "Mechanisms of the displacement of fluid by another in a network of capillary ducts," J. Fluid Mech. 135 (1983), 337-353.

1. In page 342 for Imbibition I1 at (a), referring to figure 5a, it says that when Pw increases in the wetting fluid, the Pc decreases (I understand this one)….Then it says as the curvature decreases the difference between the pressures at each side of the interface increases which involves rapid displacement. I do not understand this! How could the pressure difference at each side of the interface increase when the curvature decreases? The opposite should happen, shouldn't it? I could understand the sudden and rapid displacement perhaps because of instability when the meniscus does not touch the walls anymore!

I think this is an error in the article. With reference to Fig. 5a, the decrease in curvature at stage 1 is gradual and stable. Stage 2 is unstable, and an abrupt transition from 2 to 3 with an *increase* in curvature (corresponding to increased pressure difference) occurs. At stage 3, the curvature continues to increase until the entry pressure (or entry curvature) of duct 4 is reached and invasion into duct 4 occurs. The steps 2 through 4 occur rapidly, despite the quasi-static assumption. This is supported by the illustration in Fig. 5b of the phase pressure gradients which develops because the pressure difference across the interface at stage 2-4 is larger than the imposed pressure difference $P_{nw}^0 - P_w^0$, which causes wetting phase to imbibe spontaneously.

Paper#3 Lenormand, R., Zarcone, C.: "Role of roughness and edges during imbibition in square capillaries," SPE 13264, presented at the 1984 SPE Annual Technical Conference and Exhibition, Sep. 16-19.

1. In equation (1) on page 4, they define μ as the wetting fluid viscosity while in paper#1 you would understand they refer to μ as the non-wetting fluid viscosity. This should be clear if we examine figure #1 and equation (6) in paper#1. The capillary number equation was later introduced in paper#7 on page 37 with no clear indications of the parameters being for the wetting or the non-wetting fluid. Do we have an agreed reference for the various parameters in the capillary number equation? In addition, equation (1) in this paper#3, they refer to the total

flow rate. Well is it the invading or the defending fluid? Also, I thought the denominator should be the cross-sectional area not the length!

The viscosity included in the capillary number should be the viscosity of the invading fluid. In paper #1 this is the viscosity of the non-wetting phase (drainage), and in paper #3 it is the viscosity of the wetting phase (imbibition). The total flow rate is calculated as the sum of the flow rates for all fluids flowing through a particular cross-section of the entire model. If only one fluid is allowed to flow across the inlet boundary, the total flow rate is equal to the flow rate of the invading fluid. The total flow rate is used in the capillary number. The denominator of the capillary number includes the cross-sectional area (not the length). In paper #3, Eq. (1), this is given by Lx, and in paper #1, Eq. (6), it is given by Σ.

Paper#5 Jerauld, G.R., Salter, S.J.: "The effect of pore-structure on hysteresis in relative permeability and capillary pressure: Pore-level modelling," Transport in Porous Media 5 (1990), 103-151.

1. The equation on page 117 (to me) suggests that SI would occur in tight rocks at a higher Pc level than in permeable rocks. What do you think?

Yes, it is expected that Pc level in low-permeable rock is higher. The equation on page 117 refers to the snap-off mechanism in Fig.5 (a), and because pore cross-sections in low-permeable rock would be narrow, smaller values of R_1 would be expected. If the value of R_2 remains large (essentially the curvature of the grains in the figure), the critical snap-off pressure could be high and favoured compared to the other imbibition mechanisms. The mechanism in Fig. 5(a) displaces non-wetting phase from the throat which subsequently could become trapped in adjoining pore bodies in spontaneous imbibition.

Paper#6: Øren, P.-E., Bakke, S.: "Reconstruction of Berea sandstone and pore-scale modelling of wettability effects," J. Pet. Sci. Eng. 39 (2003), 177-199.

1. Well written paper indeed!
2. In page 13 in the upper paragraph to the right they say that some of the pores that are oil wet during water flooding (θ_a>90°) will be water wet during secondary drainage (θ_r<90°) due to contact angel hysterisis. Well! Although their argument is correct but should not be mentioned in this way. I prefer what was mentioned in Masalmeh's paper in the DPE180 course where Shehadeh Masalmeh said the pores would behave like water wet in the secondary drainage. The pores cannot be water wet because simply they cannot imbibe water spontaneously - they remain oil wetted. What do you think?

I agree. It is be better to say that the pores *behave* oil-wet during imbibition and *behave* water-wet during secondary drainage if $\theta_a > \pi/2$ and $\theta_r < \pi/2$. Also, **keep in mind that the triangle tubes are idealised models of the pore throats in the rock, and the contact angle hysteresis model is a largely simplified characterisation of pore scale wettability. In reality, each pore could have rough pore walls and heterogeneous**

wetting states (i.e., the pore wall boundary could be composed of several small segments of different contact angles). It has been demonstrated by Long et al. (http://www.ux.uis.no/~s-skj/KapHystFukt.06/Papers/long05.pdf) that such systems exhibit contact angle hysteresis, where the advancing and receding contact angles are effective contact angles which are different from the contact angles measured on flat, smooth and uniform surfaces. In such systems it is possible to observe oil-wet and water-wet behaviour during imbibition and secondary drainage, respectively. **The heterogeneous wetting state which developed in primary drainage depends among other things on the disjoining pressure isotherm, which could be different for different rock mineral – fluid systems, and pore wall curvature.** The contact angle hysteresis model used by Øren and Bakke is probably used to describe such heterogeneous wetting cases, where pore wall surfaces are composed of many small oil-wet and water-wet segments.

3. In page 14, under equation 30, they talk about correlating Wettability to pore size, and I find their statements very important in obtaining a rather profound understanding of Wettability in real reservoir rocks. They say that the saturation function fs1 depends on the distribution of oil wet pores. Then they say if the oil wet pores are randomly distributed, Wettability is uncorrelated with pore size. Whereas, if Wettability is correlated with pore size, fs1 depends on the pore size distribution. Here, I have several questions:

    a) Shouldn't Wettability always be linked/correlated to pore size? Pore size affects the entry pressure of oil. It also affects the curvature of oil/water meniscus which is in turn a direct influence on water film thickness and hence Wettability alteration.

I believe yes. However, the functional dependency/correlation could be very complex. In some cases I guess it could be reasonable to consider a random distribution of oil-wet pores.

Consider a scenario where wettability alteration is assumed to begin when thick water films collapse at a capillary pressure determined from the Augmented Young-Laplace Equation which takes into account pore wall curvature. Depending on pore size, pore wall curvature and critical disjoining pressure values (which includes all effects of mineralogy and fluid chemistry), it is possible to develop pore-scale models for primary drainage which gives MWL behaviour (large pores become oil-wet first) and MWS behaviour (small pores become oil-wet first). The MWL case is probably most common, but the MWS case can be obtained if one assumes non-convex pore shapes where pore wall segments curve more and more concavely as pore size decreases. **In reality, there may not be a correlation between pore wall curvature and pore size, and in such cases it is more difficult to say anything about the distribution of oil-wet pores based on such a simple analysis.** A heterogeneous distribution of minerals will further complicate the analysis.

Arne Skauge's group distinguish between the MWL, MWS and FW (random distribution of oil-wet pores) cases, and experimental data suggested that all these cases exist in different reservoirs. More information on this can be found in the following paper:

A. Skauge, K. Spildo, L. Høiland, B. Vik: "Theoretical and experimental evidence of different wettability classes", Journal of Petroleum Science and Engineering 57 (2007), 321-333.

Thanks! Did you want to say above (in red) MWS can be obtained with convex pore shapes? If I recall correctly the MWS would be obtained for convex pore shapes. Please advise.

Above, I am defining convex/non-convex shapes from a reference point outside of the pore. From this reference, circles, triangles and squares are convex shapes, and star-shapes with curved walls formed by the space between touching circles (e.g. grains) are non-convex shapes. In the paper by Skauge (reference above) it is explained on page 324 how a star-shape can give MWS and how circles can give MWL behaviour. In the literature, different authors have different reference points (outside or inside of the pores) which leads to different definitions of what shapes are convex and non-convex, which makes it confusing to follow.

Wow! This is very important point to note. Thanks! Indeed it can make a lot of confusion.

b) If the oil wet pores are randomly distributed does this mean Wettability is uniform throughout the rock?

Not necessarily. Network models are constructed on mm length scales, and for network models representing different parts of the entire rock, one may consider different fractions of randomly distributed oil-wet pore surfaces.
Also, **uniform wettability in network modelling would refer to cases where the contact angles are the same in all pores.**

c) In my opinion, fs1 should always be dependant on the pore size distribution because Wettability should always be a function of pore size distribution. If we say random distribution of oil wet pores this may not mean (as they say in the paper) Wettability is uncorrelated with pore size, rather it may well mean that the pore sizes are themselves randomly distributed in the porous medium which gave rise to random distribution of the oil wet pores. This is the way I understand the porous medium, and I hope you could help me understand better. Thanks.

I agree with the first part, but for a given pore-size distribution, the fraction of oil-wet pores can be distributed in all kinds of ways. One possibility is random distribution, without paying attention to the pore size. Another possibility is to specify wettability (contact angle) as a function of pore size, i.e., wettability is correlated with pore size (as in the MWS and MWL cases discussed above).

I see, thanks!

4. I wonder where the ΔS4 equation derivation has gone in page 14! Is there a printing mistake in equation 36 where ΔS3 there should have been ΔS4?

From my point of view, there is no printing mistake in Eq. (36). The equality sign holds if the imbibition and secondary drainage curves form a closed hysteresis loop, implying there is no trapping of water during secondary drainage, as discussed in the text above Eq. (36).
Based on the definition of I_o and Figure 8, $\Delta S_4 = S_3 - S_1$.

I see. We could also write $\Delta S4 = \Delta S_1 + \Delta S_2 - \Delta S_3$, right? Hence, we could express ΔS_4 in terms of equations 29, 31 and 34. Now, I realise why they did not need to derive a separate expression for ΔS_4 in the paper.

Yes, this is how I understand it.

5. I guess a_4 would be equal to one in the discussions in page 14 under equation 35, right?

I cannot see a4 occurring any place in the paper.

Yes, you are right but I assumed we can talk about trapping term as a_4 in the positive sec dr curve similar to what was done in a_1 and a_2. Since it was assumed no water trapping in sec dr then it would follow a4 is equal to one. By the way can we call a_3 as water trapping term in the negative sec dr. curve? If so why isn't it assumed to be one?

I agree regarding a4. No water trapping in positive part of secondary drainage (SD) could imply a4 = 1. Permanent water trapping in positive part of SD would imply that the main hysteresis loop is not closed, and a4 < 1. In Eq. (34), S_{ow2} seems to represent the volume fraction of all oil-wet pores of type α_2, i.e., $\theta_a > 90^\circ$ and $\theta_r > 90^\circ$. If not all of these pores are invaded by oil during the negative part of the SD curve, a_3 must be less than one. This can occur if for example some of the water filled oil-wet pores ($\theta_r > 90^\circ$) are not accessible for oil invasion before oil has invaded some neighbouring water-wet pores ($\theta_r < 90^\circ$) first during the positive part of the SD curve. Also, if permanent water trapping occurs during negative SD, the main hysteresis loop will not be closed, and if this is the case, a3 should also be less than one. If a3 = 1, all oil-wet pores of type α_2 are invaded by oil during negative SD (see also further comments below).
My understanding is that a3 < 1 can occur if a fraction of water filled oil-wet pores ($\theta_r > 90^\circ$) is not invaded at negative SD, because these pores could first be accessible and invaded at positive SD. If a3 < 1 and a4 = 1, the hysteresis loop should be closed, and if a3 < 1 or a3 = 1 and a4 < 1 it is not closed.

I have got confused when you say water invasion in negative SD. SD involves oil invasion. Please clarify. Sorry about this – I mixed water and oil. See the corrected comment above.
Additionally, if a3<1 (as being assumed in the paper) then there is water trapped during oil invasion in the negative SD. The remaining water does not have to be trapped: The remaining fraction of waterfilled oil-wet pores could be invaded at positive SD. Further, oil could become trapped in oil-wet pores during forced imbibition, and the saturation contribution to this would be $S_{or,ow1}$. The part of this trapped oil saturation after FI that are located in oil-wet pores of type α_2, i.e., $\theta_a > 90^\circ$ and $\theta_r > 90^\circ$, is given by $S_{or,ow2}$. So, if $S_{w,ow2}^{imb} = 0$, it is still possible with $a_3 < 1$ as long as $S_{or,ow2} > 0$. To clarify: Eqs. (34)-(35) states that the saturation change in negative SD is given by: $\Delta S_3 = S_{ow2} a_3 = S_{ow2} - S_{or,ow2} - S_{w,ow2}^{imb}$. This is equal to the volume fraction in oil-wet pores of type α_2 minus the trapped oil saturation after FI in pores of type α_2 minus the water saturation in the pore fraction of type α_2 after negative SD.

At the end of Forced water imbibition (at Sor) we have oil wet pores (mostly invaded by water) and water wet pores (where oil is trapped). Correct. We have water wet pores (where oil is trapped) because a1<1. Correct.
During negative SD (spontaneous oil imbibition), water will be trapped in the oil wet pores and hence a3<1. Water could be trapped temporarily in negative SD, and be displaced at positive SD to give a closed hysteresis loop. See also comment above and below. This will always yield open loop which they do not want to have because this will complicate the calculations, right. It will at least introduce more parameters (e.g., a new endpoint saturation after completed SD). This is why they assume little entrapment and hence a4=1? I guess so, but also due to experimental evidence. Eq. (36) (with equality sign) implies no water trapping during complete SD (positive and negative).
When they say $S_{w,ow2}^{imb}$ is the vol frac of oil wet pores filled with water at the end of spont oil invasion, this means they assume that spont. oil invasion did not occur in all oil wet pores, why? Am I missing something here?
My interpretation is that all waterfilled oil-wet pores may not be accessible for oil invasion in negative SD due to the spatial contact angle distribution within the network. The oil-wet pores satisfying $\theta_a > 90^\circ$ are divided into fractions satisfying $\theta_r > 90^\circ$ and $\theta_r < 90^\circ$. It would be possible to distribute these fractions within the network such that not all oil-wet pores are accessible for oil invasion in negative SD.

That was a very condensed discussion and I very much appreciate it. Thanks! The world becomes more complex when we start looking into its tiny details!

6. I very much appreciate the new understanding I got from this paper which is: We can quantify wettability effects on macroscopic properties but there is long way for predictive modeling of the wettability effects.

Good. One of the main challenges in pore network modelling is to characterise accurately the wettability of the rock in question, because it depends on so many factors

(mineralogy, fluid chemistry, temperature, pore geometry, etc.). Therefore, **network models will have an inherent requirement for calibration or history matching to determine the contact angles based on available measured capillary pressure and relative permeability curves.**

7. In page 20, the first paragraph on top, they conclude that contact angle hysteresis is probably the main cause for the discrepancy observed between simulated and experimental oil recoveries. Would this be contradicting with the conclusion from Masalmeh's SCA2001 paper (DPE 180 course) where experimental data suggested that the contact angle hysteresis had significant effect on Pc hysteresis but hardly affected Kr hysteresis?

In page 20, I think the main point is that **increased θ_a results in more pores with the configuration shown in Figure 7 (b) with thick oil layers (films) in the pore corners, sandwiched by water in the pore centre and corners. It is argued that existence of such configurations throughout the rock will make the oil more continuous and permit displacement of oil by "film drainage", which may result in increased oil recovery and smaller residual oil saturation.** In the SCA2001 paper, the focus seems to be on the effect of contact angle hysteresis on Pc and Kr hysteresis, and not so much the effect of wettability on residual saturations.

8. In page 20, they conclude that the computed oil recoveries are fairly insensitive to the way the oil wet pores are distributed. To me this could be visualized if almost all oil invaded pores are flooded with water but oil recoveries may not be the same with different oil wet pore distributions after, say, the first PV injection, right?

This seems right. Trapping of oil may occur in water-wet pores during imbibition by snap-off events, whereas in forced imbibition, piston-like displacements occur more frequently, resulting in much smaller amount of trapped oil in oil-wet pores. On the other hand, clusters of oil-filled oil-wet pores could become isolated during imbibition.

Paper#7: Valvatne, P.: "Predictive pore-scale modelling of multiphase flow," PhD dissertation, Imperial College London, 2004.

1. In page 31 at the top, I see that the famous Laplace equation lacks an item to account for pore shape in addition to the representation of pore size by "r". This understanding was later confirmed by equation 3.7 in page 41. Can we say that the pore shape term was needed here because the Laplace equation was not used in its general form of the principle radii of curvature? Therefore, we can say that the "G" factor was introduced as an empirical parameter to account for the principle radii of curvature. We can further say that the principle radii of curvature not only consider the pore size of the medium but also the shape of the pore. Can we say this? Is this the true understanding here?

No, this is not the understanding here. The capillary entry pressures presented here are derived from the Mayer & Stowe – Princen method, and the expressions are the exact analytical solutions of the Laplace equation in triangular pores. There are several papers later in the syllabus which deals with the Mayer & Stowe - Princen method more thoroughly, and a brief introduction for equilateral triangle tubes with uniform wettability can be found here (Section 2.4.1): http://www.ux.uis.no/~helland/Part1.pdf .

To account for all kinds of irregular triangles, the solutions can be expressed exact in terms of the dimensionless shape factor G = A^2/P. Hence, G is not empirical. The shape factor G was introduced by Mason and Morrow (1991) and the analysis was further extended by Øren and Bakke (1998) to allow for nonzero contact angles and imbibition.

- G. Mason, N.R. Morrow: "Capillary behaviour of a perfectly wetting liquid in irregular triangular tubes", Journal of Colloid and Interface Science (1991) 141, 262-274.
- P.E. Øren, S. Bakke and O.J. Arntzen: "Extending predictive capabilities to network models", SPE Journal 3 (1998), p. 324-336.

Thanks for the explanation above. Apparently, it is not only the pore size which controls the Pc behaviour. Pore shape and contact angles are also part of the story. So, do you think the principle radii of curvature in the general form of Laplace equation account for both the pore size and shape? The Laplace equation with a single "r" can only account for pore size not the pore shape, right? The single "r" equation was derived for tube model. However, the general Laplace equation with both principle radii of curvature is already taking the pore shape into account. Please advise?

Generally, the principle radii in the Laplace equation do not contain information on pore size and shape at all. The point is that we need to solve this equation for the unknown principle radii of the oil-water interfaces in the particular pore geometry in question. To do so, we need to know the boundary condition (given in terms of the contact angle). The solution would depend strongly on pore size, pore shape and contact angle.

However, to obtain solutions for tubes with constant (e.g., triangular) cross-sections the Mayer-Stowe & Princen (MS-P) method is widely used. Consider an "oil sausage" invading a water filled tube with triangular cross-section. The capillary pressure across the interface is given by the Laplace equation, $P_c = \sigma_{ow}\left(\frac{1}{r_1}+\frac{1}{r_2}\right)$, where r_1 and r_2 are the principle radii of curvature. It is distinguished between the front interface moving in the direction along the tube length (which is referred to as the Main Terminal Meniscus (MTM)) and the interface that can be seen behind the front in a cross-sectional view which separates water in the corner from oil in the centre portion. The latter interface is called Arc Meniscus (AM). Sufficiently far behind the MTM, one of the principal curvatures of the AM is zero, i.e., $r_2 = \infty$, and the Laplace formula for the AM is given by $P_c = \sigma_{ow}/r$, $r = r_1$. Since we assume that

mean curvature and capillary pressure is constant everywhere along the interface, the mean curvature of the AM and MTM is equal. Hence, as you walk along the interface, mean curvature is constant, but the individual principal radii of curvature can change provided capillary pressure remains constant. Far behind the MTM, one of the radii of curvatures of the AM becomes zero, which is utilised in the MS-P method.

The MS-P method considers a virtual displacement dx of the MTM in the direction along the tube length. The curvature at the MTM is difficult to calculate because it is 3-dimensional, generally non-spherical and given by both r_1 and r_2. Therefore, we choose to calculate the curvature at the AM instead, which is equivalent, and given by a cylinder with circular cross-section and radius r. The crux of the MS-P method is that we can consider the cross-sectional fluid geometry **before** and **after** the virtual displacement without paying attention to the actual shape of the MTM. The work required to move the MTM a distance dx must be balanced by a change in interfacial free energy,
$W = \Delta F$.
The work for the assumed displacement (oil displacing water with water left in the corners behind the MTM) is given by:
$W = p_o dV_o + p_w dV_w = P_c A_{eff} dx,$
where $dV_o = A_{eff} dx, dV_w = -dV_o$ and $P_c = p_o - p_w = \sigma_{ow} / r$.
The change in interfacial free energy is
$\Delta F = \sigma_{os} dA_{os} + \sigma_{ws} dA_{ws} + \sigma_{ow} dA_{ow} = \sigma_{ow}(L_{ow} + L_{os} \cos\theta) dx,$
where $dA_{os} = L_{os} dx, dA_{ws} = -dA_{os}, dA_{ow} = L_{ow} dx$ and $\sigma_{os} - \sigma_{ws} = \sigma_{ow} \cos\theta$.
By solving $W = \Delta F$ for r, $P_c = \sigma_{ow} / r$ can be calculated, and the obtained value for capillary pressure is valid everywhere across the interface, including positions at the AM and the MTM. In tubes with triangular cross-sections, the obtained expression for r is a function of certain parameters that characterise the cross-sectional angular shape (normally the half angles of the corners & the radius of the maximal inscribed circle of the pore), and the contact angle. As discussed previously, the solution can also be expressed in terms of the dimensionless shape factor G = A/P^2, where P is perimeter and A is area of the pore cross-section.

Wonderful and simple explanation. Thanks indeed!

2. In page 32 at the second line, if it were me, I would replace the phrase “oil flooding” by “oil invasion” as, I think, would represent the physical phenomenon in better words. Am I correct here? Oil flooding may lead to thinking that the oil entering is done with high viscous forces.

I agree. However, many publications exist in the literature where “flood” is used interchangeably with “invasion”.

3. In page 32 in the first paragraph, I have come across with the statement that some wettability alteration might still occur in pores where the water film remains un-

ruptured as a result of asphaltene diffusing across the water film. The PhD thesis referred to Kaminsky and Radke work which I had read previously in DPE 180 course and I do not recall at all that they (Kaminsky and Radke) concluded wettability might change with this phenomenon. On the contrary, they explicitly say that wettability cannot be altered without the water film rupture. Where is the truth here? Appreciate your valuable comment. The thesis concludes that the rock may end up with three different wetting populations: strongly oil wet where the water films collapsed, strongly water wet where oil did not invade (e.g. crevices) and intermediate wet where the water films remain. The thesis also refers to the published work by Shehadeh Masalmeh in the J. of Pet. Sc. and Eng. in 2003 where the thesis say that such type of wetting condition (intermediate wet) had indeed been observed experimentally! I have not had the chance to get access to the Journal to confirm this. Could you help in getting that Journal or if you could let me know how I could have access to such journals as a UIS student. Such Journals are not available in the OnePetro downloads. Thanks

The statement by Valvatne regarding diffusion of asphaltenes across water films cannot be correct. As you say, it is opposite of the conclusion by Kaminsky and Radke. However, still one may distinguish between different wetting cases as in the thesis, but the underlying explanation is wrong. **The intermediate wet case could be due to cases where the water film has collapsed but without much adsorption of asphaltenes.**

4. In page 33 in the second paragraph the thesis says (in the last sentence in that paragraph) that Kovescek et al. were able to reproduce the experimental trends observed by Salathiel. Well! I find this statement very misleading! I have read both work by Kovescek et al. (which is actually the Radke work in DPE 180 course) and the work by Salathiel 1973 as it was again in DPE 180. The work presented by Salatheil is all focused on oil recovery by PV of water floods whereas Kovescek et al. work is all Pc curves. Indeed both works are dealing with oil films but there is nothing related to reproducing experimental trends, is there? What do you think?

Kovscek was one of the first to present a model for wettability alteration in angular pore geometries which can account for the proposed mechanism (oil drainage through films/layers in imbibition) that Salathiel thought was responsible for the behaviour in his experiments.

Absolutely right but my comment is that they should not say “reproduce experimental trends” as this will lead to confusion where people may think that Kovseck at el made simulation of the experimental data and were able to match the experimental data seen in Salathiel work!

Ok – I can see your point.

5. There is a very nice argument in page 34 in the second paragraph. Dixit et al. suggested that if “experimental Pc’s were available for different flooding cycles, it should be possible to determine whether oil wet pores were correlated to pore size or just randomly distributed”. I guess they are talking about scanning Pc

curves all starting on the primary dr. with different Sw to see how the imbibition curve will act. What do you think? This can directly give us a link between different oil wet fraction to pore size. This actually could be done easily in the lab and it will be a fantastic research area.

I think the works referred to here (Dixit et al.) are the first papers that introduced the MWS, MWL and FW wettability classes described above. They derive analytical expressions for USBM and Amott-Harvey wetting indices for all the 3 wettability classes using a bundle of tubes model. I think "flooding cycle" refers to the main hysteresis loop, which is required for calculation of these indices. Then I guess the idea was to compare the analytically derived relationships with the experimental data and try to determine which wettability class the measurements belonged to.

6. In the second line at the top of page 41, there is the gravity effect that could be included in the Pc equation. I just want to confirm that the eqn would then be as follows: Pc = 2*IFT*cos(angle)/r – delta density*g*h. Is this how the inclusion of the gravity term would be?

This is correct.

7. In equation 3.8 in page 42, there must be a sign mistake for the term $Los\sigma_{ws}$. The (+) should be (-). What do you think?

Yes. The equation, which is the basis for the Mayer & Stowe - Princen method, should read: $P_c A_{eff} dx = (L_{ow}\sigma_{ow} + L_{os}\sigma_{os} - L_{os}\sigma_{ws})dx$.

8. In page 43, just above section 3.3, the thesis says that the wetting phase (water) remain in the corners which ensures the water stays connected throughout primary oil invasion. This idea was later emphasized in page 50 in the second paragraph under section 3.3.5 where the thesis says during primary drainage trapping is unimportant as water in the corners ensured global connectivity. I like these statements because they are in line with our findings in the paper that we will hopefully publish together with Ove Bjorn Wilson. In that paper that we will write we will show how the RI values were not affected by the rate of invasion and that trapping was unimportant. Although there might be some differences at the early invasion stage but because of the global water connectivity the late RI values and hence Sw distribution may be the same in slow and fact primary drainage. What do you think?

Sounds interesting.

This is also seen in the thesis in page 31 which is suggested in the work by Oren et al. 1998 and Patzek 2001.

9. In page 45 in figure 3.7(a), it is confusing if this state applies to spontaneous imbibition (SI) and forced imbibition (FI) or only for FI. In other words, can we talk about hinging contact angle in SI? Could you clarify this for me, please? I would say it is obvious figure 3.7(b) considers FI.

Fig. 3.7(a) can occur for SI and FI, and Fig.3.7(b) only for FI. After primary drainage with a small contact angle θ_{pd} and wettability alteration and/or contact angle hysteresis,

i.e., $\theta_{pd} < \theta_a$, the interface in the corner will hinge according to the decrease in capillary pressure during imbibition while the contact lines are pinned at the positions separating oil-solid pore wall segments and water-solid corner segments. The hinging angle of the interface θ_h then changes gradually from θ_{pd} toward θ_a as capillary pressure is decreased. For (weakly) water-wet conditions this will occur until an invasion event occurs, or, until $\theta_h = \theta_a$ at which the interface starts to move with θ_a toward the pore centre during additional decreases of capillary pressure. This movement is only possible for positive capillary pressures (SI). For cases $\theta_a > 90^\circ$, the interface will always hinge until an invasion event occurs. One possibility is shown in Fig.7 (b), which can only occur at negative capillary pressures (FI). What will happen depends on how sharp the corners are, the maximum capillary pressure obtained at the end of primary drainage, and the input contact angles. If several imbibition and drainage processes have been carried out, the correct displacement event in the pore depends on the history.

More on this topic can be found here:
http://www.ux.uis.no/~helland/SPE89428PA.pdf

10. As a continuation to the previous question, during SI the meniscus slowly turns from being convex to the water side to being flat between oil and water at Pc=zero, right? If this is so, how could we view contact angles at Pc=zero?

This is correct. However, the advancing contact angle cannot be viewed in the pore cross-section in these cases. The angle you would see in the corner is the aforementioned hinging angle θ_h, but this angle is a function of capillary pressure and is certainly different from θ_a. At zero capillary pressure, the value of θ_h depends on the corner shape.

Does this mean that at Pc=zero, we may not have flat (oil-water) meniscus as the interface curvature does not only depend on the Pc value but also on the corner shape and contact angle? In other words, is it possible we have convex meniscus towards the middle of the pore (the oil side) during SI?

No. At Pc = 0, the AM interface must be flat, at positive Pc's the AM must bulge toward the corner, and for negative Pc's the AM must bulge toward the centre of the pore.
The hinging contact angle in a water filled corner with half angle α is calculated as

$$\theta_h = \arccos\left(\frac{P_c b \sin\alpha}{\sigma_{ow}}\right) - \alpha$$, where b is the distance from corner to contact line.

For Pc=0, this reduces to $\theta_h = \pi/2 - \alpha$. Hence, it depends on the corner angle.

Excellent explanations. I find it difficult to convince people about the effect of pore geometry on the SI behaviour. I say that the SI curve is a function of both Wettability and pore geometry but I find "some" experts in the oil industry say that the SI is only a function of Wettability and has nothing to do with pore geometry. Would the comments above be a good reasoning for the clear influence of pore geometry on the behaviour of SI? What do you think?

In my opinion SI depends on both pore shape and wettability even for similar porosity and permeability conditions. However, the effect may be minor or negligible if there is no

wettability alteration during primary drainage. If you have two models with different pore geometry and run it to the same (Sw,Pc) point (if possible) after primary drainage, and consider the pore walls in contact with oil to develop the same wetting conditions in the two models, then the SI part of the Pc curves will generally be different. This is because the fractions of pore wall area with altered- and water-wet conditions generally will be different in the models (due to different geometry), and because the pore shapes are different also, the entry pressures in the pores in the two models will be different. The endpoint after primary drainage is very important too. As discussed below, it is possible to have SI in pores where $\theta_a > 90^\circ$, whereas in other pores where $\theta_a > 90^\circ$, the invasion would be forced (the normal case). Based on my own experience, the pore shape geometry has larger impact on the FI (forced imb) process. This is because the pore shapes have large impact on whether oil layers could form or not, which strongly affects the capillary behaviour. To my knowledge, a thorough and systematic study on these issues based on realistic models is lacking in the literature.

I cannot see how the angle could be larger than 90 deg during SI where the meniscus is either bulged to the water side or flat!

The advancing contact angle θ_a is defined on the pore wall surface which was in contact with oil during primary drainage. SI can occur in mixed-wet angular pores even if $\theta_a > 90^\circ$. The contact angle θ_a is formed between the invading MTM (Main Terminal Meniscus) and the pore wall with altered wettability. In the cross-sectional view, we cannot observe the MTM or the contact angle θ_a. In the pore cross-section, only the AM (Arc Meniscus) and its hinging angle θ_h are visible. The hinging angle is located on the point separating the surfaces of different wettability, and the AM is a circular arc. The hinging angle is smaller than θ_a in a mixed-wet pore (it varies continuously with capillary pressure), and for SI to occur, θ_h must take a value that makes the AM curve toward the corner. Under these circumstances the shape of the MTM will be quite complex (since the curvature of the invading MTM and AM are equal during invasion). Obviously, SI in circular pores would not be possible for $\theta_a > 90^\circ$ since the pore boundaries have uniform wettability (if the same model for wettability alteration is applied). This demonstrates that different pore geometries can lead to wettability conditions that give different capillary behaviour.

11. Related to the above, it is confusing how it says in page 46 just under eqn 3.23 that advancing contact angle could be larger than 90 degrees for SI? Does this mean that the surface is still water wet but due to contact angle hysteresis θ_a can be >90?

This is an effect of the pore corners being water-wet. If $\theta_a > 90^\circ$, it is possible to have positive entry pressure in imbibition for piston-like invasion if the displacement is affected by the water-wet corners. The critical value of the advancing contact angle θ_a for a transition from spontaneous to forced piston-like invasion is given in Eq.(3.24). It is derived from Eq. (3.8). As maximum capillary pressure obtained in primary drainage

approaches infinity, the amount of water in the corners decreases toward zero, and the critical contact angle decreases toward 90 degrees. See also Figure 3 in the article below:

http://www.ux.uis.no/~helland/SPE89428PA.pdf

Can we call the piston like invasion in the positive entry pressure during imbibition as retraction? Yes, I believe so.
What is meant exactly by the critical contact angle? Is it the change in contact angle as we go from positive imbibition to negative imbibition?
Yes. The critical contact angle $\theta_{a,crit}$ is the value of the advancing contact angle θ_a which gives the pore an entry pressure equal to zero in imbibition ($P_{c,entry} = 0$). If $\theta_a > \theta_{a,crit}$, then $P_{c,entry} < 0$, and if $\theta_a < \theta_{a,crit}$, then $P_{c,entry} > 0$. Note also that these issues were not taken into account in the analysis discussed previously in Paper #6 by Oren and Bakke.

12. In page 46 just above section 3.3.2 why θ_r is replaced by π- θ_a and not simply by θ_a for forced imbibition?

This is because the entry pressure for the displacement is derived based on the Mayer & Stowe - Princen method for forced imbibition. It is assumed that water invades bulk oil, which results in oil layer formation in the corners. This is different from SI where oil layers do not form. The equivalent to Eq. (3.8) is for FI given by
$-P_c A_{eff}\, dx = (L_{ow}\sigma_{ow} - L_{os}\sigma_{os} + L_{os}\sigma_{ws})dx.$
Solving the equation for Pc should prove the validity of replacing θ_r with $\pi - \theta_a$ in Eq. (3.7).

13. In page 47, in figure 3.8, it is confusing which event is more favorable, 3.8(a) or 3.8(b). The way I understood this is that 3.8(a) is similar to piston-like displacement so it is more favorable (at higher SI Pc values) as mentioned in the last paragraph in page 47. However, at the beginning of this section in page 46, it infers that 3.8(b) would be more favorable because the radius of curvature is higher and thus lower Pc (towards the end of SI process). Therefore, different SI processes will take place as we lower down the Pc from the max. value to zero. Is this correct?

I agree that this part is confusing. Apparently, this is an empirical procedure suggested based on the experimental data from Lenormand. According to a private communication with K. S. Sorbie and M. I. J. van Dijke, the procedure is questionable due to a missing physical basis.

The event in Fig. 3.8 (a) is more favourable than the event in Fig. 3.8 (b). In spontaneous imbibition, the favourable entry capillary pressure is the one associated with smallest radius of curvature, i.e., the highest entry pressure is favoured.

Fantastic! I find your statement in line with what is mentioned in Jerauld and Salter (1990, Transport in Porous Media) paper#5 in DPE190 in page 118.

The statement in page 46 refers to the maximal radius of curvature that can be achieved in the *pore body*, and when this radius is large (occurring when many adjacent *throats* are oil-filled) a small capillary pressure is required for water invasion into the pore body (see Fig.3.8 (b)). The approximate form in Eq. (3.25) is suggested to model such cases.

14. In page 48 in the first line, what is meant by weights?

I think weights refer to the empirical A_i parameters in Eq. (3.25).

15. In page 48, in the first paragraph, we can conclude that scanning SI curves starting on the PD curve will experience different displacement mechanisms. What do you think? If this is so, then it will indeed be interesting to investigate which displacement is more favorable on the different scanning curves. Would, for example, the snap off mechanism be more favored on the scanning curves starting at the high Sw values or would be more favored on the boundary curve?

It is probably true that different displacements will take place in imbibition processes starting from different S_wi, even if wettability alteration takes place during primary drainage or not.

I would expect snap off to occur most frequently in imbibition processes from high Swi under water-wet conditions. At high Swi, there is a lot of water in the corners of the pore throats after primary drainage, and if no water is available for piston-like invasion into a throat, the capillary pressure does not have to decrease much in order for snap off to be favoured in the throat during SI.

In a bundle of triangular tubes model (which is simplistic in that snap-off is topologically impossible), different configuration changes occur during the scanning imbibition processes from different Swi:

http://www.ux.uis.no/~helland/SPE89428PA.pdf

16. I wonder how valid the stated assumption is located at the first line of section 3.4 in page 51, where it is assumed that no further wettability alteration would take place during imbibition? Could you refer me to published work which opposes this assumption, please? Thanks.

Svein: Kan du kommentere her?

17. Above equation 3.41 in page 53, it should say momentum conservation rather than mass conservation, shouldn't it?

No, it is mass conservation, or even more precisely, conservation of volumes of each fluid phase *p*. Eq. (3.41) states that the amount of fluid flowing into a pore body from adjacent pore throats is equal to the amount of fluid flowing out of the pore body to adjacent pore throats.

18. In Masalmeh's paper in DPE180 course, it was said that contact angle hysteresis hardly affected Kr curves. In this thesis in page 65 in the first three lines of the paragraph similar thing was mentioned but with more details. Here it says that

small changes in the distribution of contact angles did not significantly affect the Kr results as long as the contact angles were less than 90 degrees.

Masalmeh considered effects of contact angle hysteresis on the difference between imbibition and secondary drainage curves. In page 65, Valvatne considers the effect of changing the advancing contact angle θ_a slightly. It is concluded that it did not have any impact on the imbibition relative permeability curve. Hence, the two works should not be compared.

Of course, Valvatne would have observed a change in shape on the relative permeability curves if $\theta_a > 90^\circ$ was considered. However, the experiments (Fig. 4.3 and 4.4) which are compared with the computations are performed in water-wet rock.

19. I think there must be a typing mistake in page 66 in the 5th line from top. It should be primary gas flooding instead of primary oil flooding. This is important because it refers to data predictions in the corresponding plot 4.4(a). What do you think?

I agree.

20. In page 75 in the paragraph under table 5.1, it quotes Jackson et al. statement that the distribution of contact angles is similar regardless of Swi. I find this statement in total disagreement with what we learnt from the disjoining pressure theory about the critical Pc which influences the water film thickness and hence wettability alteration. Surely the critical Pc would also affect the contact angle. Hence, I do not imagine that the contact angle distribution of the invaded pores to be the same as we increase the oil saturation in the pore system with high primary drainage Pc. Appreciate your comment, please.

I agree with your reasoning above which sounds physically reasonable. I am not sure what is stated in the Jackson et al. paper. In Valvatnes thesis, I think the main objective here is to examine if it is reasonable to assume a hypothesis where wettability is independent of Swi in order to reproduce the experiments by the model. Later, on page 80, it is indeed stated:

"Our initial hypothesis that the distribution of contact angles in pores contacted by oil is independent of initial water saturation and brine salinity (in terms of Ca2+ content) does not seem to be entirely supported when comparing predicted and experimental trends in Amott wettability indices. In Table 5.2 the wettability indices corresponding to Figure 3.1 are tabulated. These experiments were all preformed with high salinity brines. It is clear that as initial water saturation increases the rock exhibits more water-wet behaviour than what can be attributed to just increasing Swi."

The above quote from the thesis should be in line with what you say above and what you have learnt on disjoining pressure theory.

Thank You

21. In the paragraph in page 82, I just want to clarify that the assumption of the largest oil-filled pores becoming oil wet is not contradicting the pore level scenario suggested by Kovscek et al. as long as the shape of the meniscus is not

convex. In real life, I would imagine some of the big pores may become oil wet and some of them remain water wet as we do not expect all the pores to be of the same shape. Some of the pores would have concave curvature and others may have convex curvature.

Good point. This is what I tried to explain in the beginning in connection with the paper by Øren and Bakke. Following Kovsceks scenario, you could end up with MWS, MWL behaviour (or something in between) depending on the pore geometry.

22. Under section 5.1.1 in page 88, it says that Sw values in the transition zone could be much higher than Swi in the oil zone due to capillary rise effects. Is it really due to this effect or is it, more correctly and physically, due to capillary pressure decreasing as we go down in the reservoir. I like to see this as oil losing pressure as it invades the reservoir downward. The way I understand primary invasion (charging of reservoir with oil) in the reservoir is from top at the cup rock towards the FWL downward. So, as the oil invades from top to bottom it loses its pressure and that is why oil can invade both large and small pores at the top while it only invades the largest pores at the bottom. Please clarify.

Your explanation is correct from my point of view. However, in the transition zone both imbibition (capillary rise effects) and drainage processes may take place locally, which must be modelled by scanning curves in the reservoir simulators.

23. In the same paragraph, it says Kr curves are needed for estimating reserves. Well, although this is partly correct but I always thought Pc curves would be more important for the reserve estimation. Please clarify.

Relative permeability curves are needed for all conventional reservoir simulators to estimate reserves. In some cases, capillary pressure effects can be neglected due to the large length scales which will make the capillary diffusion term in the saturation equation small. On the other hand, relative permeability depends on capillary pressure, and hence capillary effects are incorporated in relative permeability.

24. In page 89 in the last paragraph, a choice of threshold throat radius of 0.1 micro-m was considered in a carbonate sample to determine a realistic connate water saturation. What is the basis for such a selection? Please note that the choice was changed to 1 micro-m in page 99 under section 5.3 for a sandstone sample. Are these selections based on something?

It seems that these selected values are based on “educated guesses” since relevant experimental data are missing. These threshold radii are set to ensure that micro-throats in clay with smaller radii than the threshold radii are not invaded by oil during primary drainage, and hence such pore throats remain water filled and water wet. This in turn would lead to more realistic connate water saturations after primary drainage.

25. In figure 5.11 at page 92, it is clear there is a wrong shift of symbols in indicating Exp. Kro with predicted values.

Correct.

1. In page 25 the 2nd paragraph, what is meant by swelling?

My guess is that it refers to increase of film thickness if the advancing fluid, which moves by piston-like motion for example, is also connected through films of this fluid that are coating parts of the solid surface in front of the invading meniscus.

2. Equation 4.7 in page 41 should more correctly be called Laplace equation rather than Young-Laplace, right?

I agree with your view. However, in the literature both appellations appear frequently.

3. In page 46, eqn 4.21 and the last paragraph are discussing the effect of primary drainage contact angle θ_1 on AM's. I would like to relate this discussion to the effectiveness of core cleaning before carrying out Pc measurement. It has been reported that ineffective cleaning of the core may lead to unrepresentative Pc curve which is shifted to the left at the intermediate Pc levels. Could this Pc behaviour be understood on the basis of the discussion in page 46 where θ_1 (for ineffectively cleaned core) could be larger than $\pi/2-\alpha$ at the start of primary drainage?

Correct. This is also true for larger and larger contact angles that still satisfies $\theta_1 < \pi/2-\alpha$: The Pc curves will be shifted to the left (because the amount of water in corners at entry pressure decreases with increased contact angle), and the Pc level decreases too.

4. But then we could we visualize the absence of AM's. Is this physically possible? Does this mean that no water will be left in the corners and crevices of the invaded pores? Yes, except for any thin water films not accounted for by the model.

The absence of AMs at $\theta_1 \geq \pi/2-\alpha$ represents the most favourable displacement based on minimisation on free energy in this case, and hence it makes physical sense. In this case, oil displaces all the water in one displacement, and hence no AMs are left after invasion. The simple entry pressure expression given by Eqs.(4.21)-(4.22) is consistent with this displacement. However, there could be thin films of molecular thickness coating the corners, but such details are not incorporated in network models. Further, the contact angle has to be larger than 60° in an equilateral triangular pore if oil should displace all of the water. Experiments have been performed on glass capillaries (by Mason and Morrow (I do not have the exact reference here)) that agree with the entry pressure values calculated by this MS-P theory.

The sign in the equation in the first line in the paragraph above should be >, right?

Generally, there are two different entry pressure expressions for the cases $\theta_1 < \pi/2-\alpha$ and $\theta_1 > \pi/2-\alpha$. When $\theta_1 = \pi/2-\alpha$, both expressions give the same result, so it doesn't matter.

It is minimum free energy because at that time less work needs to be done on the porous system, right?

Correct. The smallest work required for a displacement to take place gives the most favourable entry pressure and the most favourable displacement. Generally, several displacements could be possible from a geometrical view. In these cases it is important to analyse all possibilities and choose the most favourable displacement that requires least work to be performed. In practice, the entry pressures for all the possible configuration changes are calculated and the displacement corresponding to the most favourable entry pressure is chosen (i.e., the smallest entry pressure during drainage and the largest entry pressure during imbibition).

5. On page 48, the paragraph, is it discussing SI or FI?

SI.

6. Generally speaking, do we have advancing contact angle specific for SI and another one specific for FI?

Each pore is equipped with one advancing contact angle for water invasion. Based on the existing fluid configuration and the value of the advancing contact angle, the entry pressure for all possible events are calculated for water invasion. The set of possible events are different for different value ranges of the advancing contact angle. The chosen event in water invasion is the one associated with the most favourable (i.e., highest) entry pressure. The results from the simulation would indicate the SI and FI parts of the water invasion.

7. If so, then surely the advancing contact angle in SI is lower than that of FI, right?

If you are aiming at dividing the range of advancing contact angle values belonging to SI and FI, then you would get:

$$\begin{aligned} \theta_1 \le \theta_a < \theta_{a,crit} \quad & \text{(SI)} \\ \theta_{a,crit} < \theta_a \le 180^\circ \quad & \text{(FI)} \end{aligned},$$

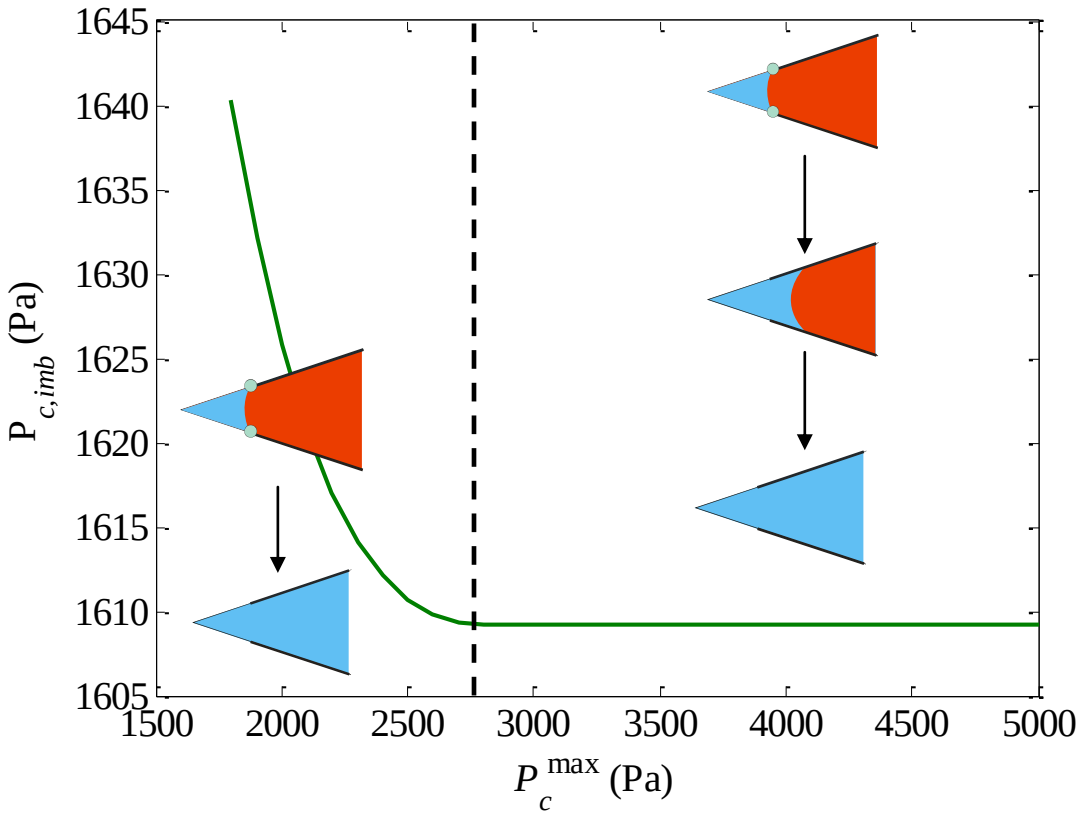
where θ_1 is the receding contact angle during primary drainage.

8. In SI, does the MTM also pin before wetting fluid invasion. I would say that the curvature here is always towards the water phase because of positive Pc value.

This is correct, but it can still pin while curvature decreases at positive capillary pressure. The MTM will be pinned at the entrance of the tube until θ_a is reached and invasion will occur. The AMs are pinned while curvature decreases, until (1) MTM invasion occurs at some positive capillary pressure, or, (2) the AMs reach θ_a, at which the AMs are not pinned anymore and instead start to move slightly toward the centre of the pore at positive and decreasing capillary pressure in a stable displacement until MTM invasion or snap-off occurs at a positive capillary pressure. The favourable scenario – (1) or (2) – depends on θ_a and the maximally obtained capillary pressure at the end of primary drainage.

Excellent! So, the higher the max Pc is, the more favourable the MTM is, right?

I am not sure what you mean here. If you are referring to the cases (1) and (2) described in the paragraph above, then case (1) is more likely to occur for small Pc,max and case (2) occurs for large Pc,max. Regarding the MTM entry pressure values in the two cases, case (1) gives higher entry pressures than in case (2). In fact, the entry pressures in case (2) do not vary with Pc,max because the AMs are not pinned anymore (the hinging angle of the AM has reached the advancing contact angle value). Instead the AMs have started to move toward the pore centre with advancing contact angle before MTM invasion. I attach a figure below explaining the favourable displacements and entry pressure variation in the two cases, using equilateral triangle as pore geometry and $\theta_{pd} = 0^{\circ}, \theta_a = 30^{\circ}$.



9. A related question to item#3 above: On page 54 under "Snap-off Mechanism" in the first paragraph, it confirms that no water will be left in the corners if PD was established with contact angle larger than π/2-α. If this happens (and I would like to understand how) then, I would say that not only the PD curve will be affected but also the imbibition mechanism and hence the imbibition Pc curve itself. Please confirm.

   This is correct. See also comment to item#3. There will not be any pinned AMs in imbibition if there is no water in the corners. Therefore, MTM invasion during

imbibition will also be modelled by Eqs.(4.21)-(4.22) with θ_1 replaced by θ_a, rather than Eqs. (4.32)-(4.48), which is used to model cases with water in the corners. Further, if wettability alteration occurs in drainage, the oil-wet area will cover the entire pore walls if $\theta_1 \geq \pi/2 - \alpha$. This differs from the case with water present in the corners, as the corners then will be water-wet. Hence, a more oil-wet scenario could be modelled if no water is left in the corners after drainage. However, if $\theta_1 < \pi/2 - \alpha$, the capillary pressure at the end of drainage must increase to infinity to remove all the water from the corners.

10. The 2nd paragraph also assumes that oil would be trapped in the middle of the pore, right?

    It is assumed that AMs move from the corners toward the centre, and the amount of water in the corners grows. Eventually, the AMs meet, and snap-off occurs, and water invades the entire cross-section and cuts off the oil. Then 2 scenarios are possible:

    - Oil could be displaced out of the tube if the oil is still continuous, leaving only water inside the tube (as mentioned in the text)
    - Oil could become trapped if it has become isolated. The trapped oil would then be located partly inside the tube, but mostly inside the neighbouring pore body.

    I think this paragraph is aiming at describing the same event which is covered in Paper#5 by Jerauld and Salter in Fig 5a.

11. Whether there is contact angle hysteresis or not (in water wet pores), oil will always be trapped in the middle of the pore, right?

    No. See comment to item #10. Both piston-like invasion and snap-off could be responsible for removal of oil occupied in pores (= throat = tube). However, in snap-off, oil will be cut off, and if the cut-off results in formation of discontinuous oil, then this oil is trapped.

    Thanks but what about the snap off process itself (whether it leads to trapped oil or not)? Can it occur in SI in WW pores/throats whether there is contact angle hysteresis or not? What do you think?

    Snap-off occurs more likely in SI than FI. It happens more likely for water-wet conditions, and it can occur if contact angle hysteresis is assumed also. Pages 54-57 in Piri's thesis give the expressions for these events. Snap-off could lead to small trapped oil compartments or droplets inside throats, but I am not sure how detailed Piri's model (and other network models) accounts for this. Several trapped oil compartments could appear in a throat if snap-off occurs at several cross-sections in the throat.

    The main point is that when snap-off in a throat occurs, water invades the cross-section and divides the oil in two different parts. This process can eventually make the oil isolated and discontinuous, but it depends on the configurations in the surrounding pore throats and pore bodies.

Another process by which oil could be trapped in a throat is if the throat has been bypassed during water invasion, i.e., water invasion did not occur in the throat, and the oil becomes trapped because oil-water interfaces has formed at both ends of the throat due to the bypassed water. In my opinion this is a different process, but in the literature this is sometimes also referred to as snap-off.

12. I assume if the advancing contact angle is very high then snap-off may occur in FI not SI, right?
    Correct. If the contact angle is very high, then all events occur at negative capillary pressure, except for the hinging of AMs which occur at both positive and negative capillary pressure.
13. I do not understand this statement: "When it occurs at a positive threshold capillary pressure ($\theta^a \leq \pi/2-\alpha_1$), it is an imbibition event, while it is forced (drainage) when the threshold capillary pressure is negative ($\theta^a > \pi/2-\alpha_1$)".

    I can understand it if θ^a refers to the hinging angle of the AMs in the corners. If $\theta^a < \pi/2-\alpha$, then the AM is pinned and curvature decreases at positive capillary pressure (i.e., SI). If $\theta^a = \pi/2-\alpha$, the capillary pressure is zero since the pinned AM is flat. If $\theta^a > \pi/2-\alpha$, the pinned AM has started to bulge toward the centre of the pore, and curvature and capillary pressure decreases and becomes more and more negative (i.e., FI).

14. On page 67, in the 2nd paragraph, it says that trapped fluid will have different pressure than the absolute pressure of the same phase. This is understood but is it correct that the trapped fluid will remain at the same pressure at which it was isolated?
    No. The pressure in the trapped fluid will increase according to an increase in pressure in the other surrounding continuous fluid. Therefore, the capillary pressure across the interface of the trapped fluid will be constant during further increments in pressure of the invading fluid and remain at the capillary pressure value at which it became trapped.

15. I am not sure if we discussed this before but would like to confirm my thinking that entry pressure is a function of pore size, shape and contact angle for a given fluid pair.
    Correct.

16. Would like to confirm that contact angle is a function of rock surface chemical properties (mineralogy) as well as physical properties (roughness), in addition to fluid pair. Can we also say that the contact angle is a function of pore geometry?
    Contact angle is a function of mineralogy and fluid pair. Whether it is a function of roughness of pore walls, would depend on how much details you would like to include in your description. One possibility would be to work with an effective contact angle which is assumed to account for all effects of the roughness. On the other hand, you could choose to model accurately all the corners and grooves which constitute the rough pore walls, and in all these corners rather use the

contact angle in the model which is defined on the flat surface of the same material. In the latter approach the detailed roughness must be dealt with in the modelling of the fluid flow processes.
In my view, and based on my comment above, the contact angle is not a function of the pore geometry as long as the "roughness" and "pore geometry" concepts are kept apart; "roughness" are features observed at a smaller level than the "main pore geometry". If you choose the former approach with an effective contact angle, then it is rather the surface roughness in a given pore geometry that is taken into account.

I see your excellent point on roughness but how come the contact angle is not a function of the pore geometry and we write this: $\theta^a = \pi/2 - \alpha$. Isn't alpha here a pore characteristics parameter?
This expression is not meant to be interpreted as an equation. It is an expression used in the construction of IF-ELSE or SWITCH-CASE statements in programming. The value of θ^a is tested against the value on the right-hand side. If it is true, then a set of statements are executed. If it is false, then other statements are carried out instead. θ^a is an input parameter in the model and has nothing to do with the pore characteristics parameter alpha.
In addition, I recall from Hirasaki's Wettability papers (in DPE 180 course) that the contact angle was somehow linked to pore radius and water film thickness which are basically pore geometry parameters or at least directly affected by the pore geometry. What do you think?
In the theory presented by Hirasaki, which include accurate modelling of wetting films, the contact angle is not a required parameter. However, as demonstrated by Hirasaki, the contact angle may be considered as a function of the *critical* (equilibrium) film thickness. The critical film thickness depends on the disjoining pressure isotherm whose shape depends on the mineralogy and the fluid composition. It is not clear to me how the critical film thickness will vary with different pore shapes and pore-wall curvature. The works I have seen on mathematically rigorous Augmented Young Laplace modelling, considers polygonal geometries with flat side walls only. In network modelling one adopts a simpler view and do not consider the accurate description of these films, and instead a contact angle is used. Further, wetting layers, i.e., films whose thickness has increased significantly due to the corner geometry, is taken into account in these network models by describing the film-layer transition region with a contact angle. This may be a good approximation because layers are thicker and hence the disjoining pressure component can be neglected in the Laplace equation. It would be interesting to explore if a drainage process in a pore geometry which is modelled by incorporating an accurate film description (without contact angles) can also be described accurately by the simpler contact angle representation adopted in network models. This is a main assumption for most network models.

17. Figure 7.1 in page 120: Which fluid pair is presented and predicted?
This is not made clear in the thesis. My guess, based on the paper by Oak (SPE 20183), is that Figure 7.1 includes experimental data for 12 two-phase relative

permeability curves (6 curves measured in two samples of 800mD and 1000mD). This is:

- kro(Sw) and krw(Sw) for oil drainage in an oil-water system
- krg(Sw) and krw(Sw) for gas drainage in a gas-water system
- krg(So) and kro(So) for gas drainage in a gas-oil system

These data are then predicted by the 2-phase version of the model. Since primary drainage is assumed and the following imbibition involves oil and water, it seems that the model assumes an oil and water system in the predictions of the data.

18. Why the behaviour and explanations for fig 7.19 are different from those for fig 7.22. Both scenarios are secondary and tertiary gas injection. Am I missing something? For fig. 7.19, the 2nd gas injection gave rise to higher Kro at low So dur to layer conductance. In fig 7.22, tertiary gas injection gave rise to higher Kro at low So due to double displacement which increased O/W Pc. Why do we have diff explanations for the same scenario (if it is really the same scenario)? The only difference I may think of is that oil initially is well connected for the tertiary fluid displacement in fig.7.19 and that oil is trapped in the case of fig. 7.22. Please advise. Thanks

These simulations represent water-wet conditions. The initial oil saturation S_{oi} before gas is injected is much larger in Fig 7.19 than Fig 7.22. For large S_{oi}, oil layers (between water in corner and gas in the middle of the pores) more likely form than for small S_{oi}, because the amount of water in the corner is smaller. Oil layers make the oil more continuous. For small S_{oi} after water imbibition, more of the oil is trapped, and some of this trapped oil is mobilized in gas invasion by double displacements: gas displaces oil (trapped) and the oil displaces continuous water in one displacement. Oil mobilisation by double displacements is more important mechanism for small S_{oi} than large S_{oi}, because less oil layers exist, and because more of the oil is trapped.

This is good explanation.

19. On page 143, under “Two-Phase” title, I seem to disagree with what is said about the lower Krw in the oil wet system than in water wet system at low Sw. Opposite to that I would say that in water wet systems Krw is reduced by the oil trapping effect. In oil wet systems, I expect to see higher Krw due to the ability of water to invade large oil wet pores at low Sw (high So).

I agree with your reasoning. I cannot find a reason why the oil-wet krw should be lower than the water-wet krw. However, the advancing contact angles are distributed in these cases, which, in turn complicates the analysis of the results and makes it difficult to examine the invasion order of the pores.

Paper#9: Helland, J.O., Skjæveland, S.M.: "Physically-based capillary pressure correlation for mixed-wet reservoirs from a bundle-of-tubes model," paper SPE89428 presented at the SPE/DOE 14th Symposium on

Improved Oil Recovery, Tulsa, OK, Apr. 17-21, 2004. Revised paper accepted for publication in SPE Journal.

1. I would like to start with a general question here: If no change has occurred in wettability and in contact angle between primary drainage and imbibition, would there be still fluid entrapment? I want to confirm that the causes of hysteresis which are contact angle hysteresis, wettability change and fluid trapping are not "necessarily" the causes of each other.

   Contact angle hysteresis (A) and wettability alteration (B) have their origins at the micro scale (or even smaller scales), whereas trapping (C) occurs as a result of the pore space geometry in the model. (A) and (B) affects the degree of trapping, but the amount of trapping does not, to my knowledge, change the amount of (A) and (B).

   In order for trapping to occur, a 3D model is required. Snap-off is the main responsible mechanism for trapping, and snap-off can only occur if piston-like invasion is topologically impossible, since the capillary pressure for piston-like invasion is always favoured compared to the capillary pressure associated with snap-off events. A pore can only be invaded by a Main Terminal Meniscus (MTM) if the invading phase is available at one of the ends of the pore. Snap-off is the only possible event to occur in a pore if no MTM is available for invasion. In the model presented in this paper (a bundle of capillary tubes), all tubes are available for piston-like invasion, and hence snap-off never occurs, implying that the model is too simple to allow for trapping. The only origin for hysteresis is then to assume contact angle hysteresis and wettability alteration. To study trapping, a 3D model would have to be used, for example by varying the cross-sectional shape of the pores in the length direction.

2. Can configuration D ever be achieved in real rocks?

   This is an illustration of a wettability-altered pore that has become invaded by water during imbibition, which should occur very frequently in real rocks. The question would rather be how thick the oil film (along the bold thick lines), if present, should be. In the model, these effects are incorporated in the contact angle used, without affecting the saturation. I remember we tried to assume thick oil films formed as convex oil lenses along these flat altered surfaces. However, in terms of minimisation of free energy, it was not favourable.

3. The first paragraph under eqn 11 is discussing SI for configuration B and the start of FI for C, right?

   In the 1st imbibition, configuration B can only form during SI. However, if configuration C forms, it can exist at both SI and FI. Eq. (11) is valid for configuration C for $P_c>0$ and $P_c<0$. Note that, in Figure 1, we do not distinguish between different configurations for FI and SI: At positive P_c, configuration C occurs with the interfaces curving towards the corners (as in Figure 2). At negative P_c, configuration C occurs in its form presented in Figure 1. The same is the case for the other configurations. For example, contact angle hysteresis between imbibition and secondary drainage in configuration E could cause the

interfaces (AMs) to curve the other way when Pc > 0 at an early stage of secondary drainage.

4. In the 2nd paragraph under eqn 11: If configuration B is attained (in SI, where eqn 6 is satisfied), it goes from B to D during the same SI, right? Correct. Here, however, the SI does not occur with snap-off but rather with piston displacement of the MTM. How is that? Appreciate your explanation!
   This is because the MTM is always accessible from the end of the tube in these simple capillary tube bundle models. Then MTM invasion will always occur since the snap-off occurs at a less favourable (smaller – during imbibition) capillary pressure. See also comment under item #1.

   Yes, thanks, now this is clear to me and of course from point#1 above.
5. Eqn 12 applies for the SI displacement of MTM where eqn 6 is satisfied, right?
   Yes, provided configuration B is obtained before invasion occurs. If, in SI, the pore still retains configuration C and it turns out due to contact angle hysteresis that the entry pressure for displacement C to D is most favourable, then displacement C to D will occur instead.

6. Sorry, I could not fully understand fig. 3 in page 173. I did understand, however, that smaller tubes exhibit more water wet nature. I also understood that capillary entry pressure increases with smaller pores. What I do not understand is the minima in the curves in that figure.
   The expressions for entry pressures given by Eq. (12), (22) and (9) are given by analytical solutions which behave as $P_c \propto 1/R$. This is because the contact angle of MTM and AM are equal. This is not the case in Figure 3. Here the contact angle of the MTM is the advancing contact angle given as input, but the hinging angle θ_h is unknown and is a function of capillary pressure as given by Eq.(11). Hence, the entry pressure does not behave as $P_c \propto 1/R$. There is a more complicated behaviour in these cases, and it is not possible to derive explicit expressions. Instead Eqn. 13-17 are solved numerically by iterations. Under certain conditions, minima are observed in the plot of entry pressure vs pore size. The displacement from C to D depends on reversal point after drainage, which is demonstrated by the different curves in the Figure 3.

7. Talking about oil invasion during SD from the first reversal pt on PD (in page 174 under Secondary drainage), that point may not be reached due to oil trapping which may occur during spont. re-drainage. How do we deal with this? Do we simply assume no oil trapping?
   As mentioned previously, there is no trapping in this model.

8. From figure 5, do we understand that after a given Pc^{min} (at the end of imbibition), SD Pc entry pressure decreases with pore size? Hence, smaller pores are invaded with a lower Pc than the larger ones due to the effect of oil layers. In here, smaller pores behave more like oil wet pores than the larger pores. In the same time, as the Pc^{min} decreases ???? Please help

Figure 3 demonstrated that for a displacement C to D, the role of the water in the corners was to increase the entry pressure toward more water-wet behaviour, even though the advancing contact angle would suggest oil-wet conditions. The reason for this is that the AMs in the corners obviously would act as a strongly water-wet surface, separating water in the corners from oil in the middle of the pore. One could imagine that the MTM invades with a zero contact angle on this surface in the corner, and with an advancing contact angle indicating oil-wet conditions along the side walls.

In Figure 5, however, the situation is opposite, describing displacement from E to C during SD. In this case, the displacement is affected by the oil in layers. Hence, the innermost AMs in the layers act as perfect oil-wet surfaces, and oil (by MTM invasion) could be imagined to invade onto this surface in the corners (the AMs) with a contact angle of 180 degrees. This tends to decrease the entry pressure toward more oil-wet conditions, although the receding contact angle defined along the side walls would indicate water-wet conditions. The Pc^min value determines the position of these innermost AMs after FI. As Pc^min decreases, the position of these AMs are located closer to the corners, and the "oil-wet surface" has less impact because it becomes smaller. Because the position of these corner AMs is constant for all pore sizes, the effect of the corner configuration is larger in the smaller pores.

Note that in the displacement E to C described by Equations (39)-(43), the outermost and innermost AMs in the corners both hinge. Hence, the hinging angle of the affected AMs is not equal to the receding contact angle, and therefore the entry capillary pressure does not behave as $P_c \sim 1/R$ and no explicit expression can be derived. Instead the behaviour is more complicated, and the Equations (39)-(43) are solved numerically. The solution plotted against pore size may exhibit maxima under certain conditions and the entry capillary pressure could be positive or negative. The displacement from E to C during SD depends on reversal point after FI, and this is demonstrated by the different curves in the Figure 5.

Isn't true that as Pcmin value decreases (Maximum Pcmin would mean Sor, end of imbibition), oil lens is larger and hence AMs are located away from corner?

Correct. I was referring to decreasing Pc,min as Pc,min becomes more and more negative, with its smallest value at the end of imbibition. It is OK to use your view instead by considering the absolute value.

A quick comment on the hinging of the outermost and innermost AMs from SPE24880 by Kovscek et al on page 7 (169), they say the free oil meniscus (innermost) is unpinned and grows at zero degree contact angle while the w/o interface remains pinned at the contact line of the step change in wettability.

This zero degree contact angle should be 180 when the angle is measured through the water phase. The innermost AMs are not pinned because no contact angle hysteresis between imbibition and secondary drainage is assumed. They also

consider snap-off in their modelling. This should not be possible in a bundle of tubes model, and is probably considered because the various favourable piston-like displacements from the MS-P method (layer- and bulk-phase displacements, see SPE89428-PA for example) had not yet been derived for mixed-wet pores at the time Kovscek's paper was presented. These displacements are favoured compared to the geometrical layer collapse and snap-off events.

9. I did not understand the link mentioned in page 176 in **Fig. 6** paragraph between imb Pc of contact angle 100 deg and figure 3. It is understood that the effect of water filled corners is less with big pores but what is the relation to the seen imb Pc behaviour with 100 deg contact angle?
   The non-monotonic Pc vs R behaviour in Figure 3 typically occurs when the advancing contact angle is around 100-120 deg for the input parameters ($P_c^{\max}, \theta_{pd}, \theta_a$ and PSD) used in this study. A plot of Pc vs. R as shown in Figure 3 gives the order at which the pore sizes are invaded during imbibition. If one considers the curve in Figure 3 for Pc^max = 25 kPa, it is clear that invasion would start from the smaller pores first, followed by a capillary pressure range where invasion occurs simultaneously from both the large and small pores, and finally the intermediate sized pore are invaded. The last invaded pore size corresponds to the value of R where the minimum on the Pc-R curve is located. Because of this invasion order of the pore sizes, the resulting Pc-Sw curve is rather flat at this weakly oil-wet case.
   For oil-wet conditions without water in the corners (or in a model with circular tube cross-sections) water invasion would start in the largest pores and continue in successively smaller pores. Then the capillary pressure curve would "curve" more towards decreased capillary pressure values at high water saturations, than what is observed in the example in Figure 6.

10. How come imb Pc in figure 6 for the 100 deg contact angle is horizontal? Where is the contribution of smaller pores on imb?
    See comment to item #9.

    Yes, thanks!

11. I am interested to know what would happen to the PD curve in fig. 6 if we assume contact angle of higher than zero. Would it be shifted to the left (lower Sw)? Such a situation may happen with insufficient cleaning in reservoir rocks!
    Yes. It will be shifted to the left and down to a lower capillary pressure level.

12. The standard curve fitting method mentioned in the last paragraph in page 178 is the nonlinear least square method, right? It could be solved by an independent software package or by solver in excel for example. Please advise.
    Correct. It could be solved in Excel. However we chose to use the lsqcurvefit function in Matlab.

13. Under "correlation" section in page 178, what is meant by uniform pore size distribution? Is it, for example, cylindrical tube model? What does it physically represent? Can we have 100% uniform PSD in reservoir rocks?
    Uniform pore size distribution means that each pore size is represented by an equal number pores, or alternatively, each pore size occurs with equal probability. In this work, PSD is not related to pore shape. In practice, PSD is obtained from mercury injection capillary pressure (MICP) curves by assuming the data can be reproduced by a bundle of cylindrical tubes with circular cross-sections. One could instead choose to derive PSD from MICP assuming a bundle of tubes with triangular cross-sections. This would give a different PSD.
    100% uniform PSD in reservoir rocks is not realistic. The reason for choosing the simplistic PSD's presented in Figure 11, is that we were interested in deriving explicit analytical correlations for capillary pressure curves and be able to analyse qualitative trends. This turned out to be very difficult for bell-shaped distributions, which are more realistic.

14. From eqn 48, the uniform PSD would correspond to the probability density function f being equal to $1/R_{max}$. How do we understand this in a physical sense?
    The pore size density function $f(R)$ describes the relative likelihood for the occurrence of each pore pore size $R \in (0, R_{\max}]$. In a uniform PSD, the relative likelihood is equal for all R.

Paper#10 van Dijke, M.I.J., Lago, M., Sorbie, K.S., Araujo, M.: "Free energy balance for three fluid phases in a capillary of arbitrarily shaped cross-section: capillary entry pressures and layers of the intermediate-wetting phase," J. Coll. Int. Sci. 277 (2004), 184-201.

1. In page 186 under section 3, in the first paragraph, it says the crux of the MS-P method lies in comparing the effective radius of curvature at the MTM with the radius of the AM's. Does this mean to prove that both radii are equal as mentioned in the first paragraph on page 187 above section 3.1?
   The curvatures of the MTM and AM are equal:
   $\frac{1}{R_1^{MTM}} + \frac{1}{R_2^{MTM}} = \frac{1}{R_1^{AM}} + \frac{1}{R_2^{AM}}$.
   However, sufficiently far behind the MTM, one of the AM radii are infinite, $R_2^{AM} = \infty$, and hence $\frac{1}{R_1^{MTM}} + \frac{1}{R_2^{MTM}} = \frac{1}{R_1^{AM}}$. This is because the curvature is equal and constant everywhere on the interface.

2. In the paragraph under section 5 on page 192, we understand by "spreading oil in a water wet pore" intermediate phase, right?
   Correct. Oil is in this case the intermediate-wetting phase. Water is wetting phase and gas is non-wetting phase.

3. In Table 1 on page 192, case 1 refers to all contact angles of zero value. Is this a physical situation? I wonder how would configuration 3 in figure 1 look like if all the three angles are zero!
   It is not that unrealistic. It would occur when $C_{s,o} = 0, \sigma_{gw} = 30, \sigma_{ow} = 20, \sigma_{go} = 10$ mN/m, representing a perfectly spreading oil. Configuration 3 would not change much, except that the contact angles would be zero. Note that in three-phase systems, the contact angles depend on the interfacial tensions as described by Eq. (7). Hence, we cannot just choose the contact angles independently of each other. We must ensure that Eq. (7) is satisfied.

4. On page 192, under eqn 37, we understand that beyond a certain AM radius, snap off is favoured. It is shown in this work that these certain AM radii are never reached, because there are always entry radii for MTM invasion that are more favourable. Hence snap-off is not favoured compared to invasion by MTM. One must have a network of tubes in order to model snap-off (see previous comments on this subject). Below that radius it is less likely to have snap-off. Eq. (37) describes the radii for snap-off. If snap-off was supposed to happen, the radii must increase to those given in Eq. (37). This is also in line with water amount in filled corners. What is meant by "when the corresponding AM's are absent" when r goes to infinity, then no snap off? What is that physical situation they are referring to?
   There are two possible ways by which wetting phase can be removed from the corners during drainage:
   - The contact angle is large and does not satisfy Eq. (30). In this case all wetting phase is displaced from the cross-section by MTM invasion, and the invading phase occupies the entire cross-section.
   - The contact angle is small and satisfies Eq. (30). In this case wetting phase is left in the corners after MTM invasion, and the invading non-wetting phase occupies only the middle part of the cross-section. To remove the wetting phase, the capillary pressure must increase toward infinity (which of course is non-physical and not possible in practice), which causes the AMs to move further and further into the corners as capillary pressure increases.

   In both cases, wetting phase is absent in the cross-section, and therefore snap-off would not occur because there is no wetting phase in the corners of the cross-section that can grow (swell) and cut off the non-wetting phase in the middle part of the pore as capillary pressure is decreased.

Paper#11 van Dijke, M.I.J., Sorbie, K.S., McDougall, S.R.: Saturation-dependencies of three-phase relative permeabilities in mixed-wet and fractionally wet systems," Advances in Water Resources 24 (2001), 365-384.

1. What is the actual three phase fluid distribution in a porous medium? Before I read the first 4 lines on page 366, I used to think that the 3-p distribution would

mean all the three phases are distributed in each and every pore (of course except the tiny water filled pores) such that (for WW system) water is in contact with the rock, gas in the middle of the pore and oil is the intermediate phase between gas and water. However, the pore scale view mentioned in those lines suggest that the view of the 3-p distribution is meant to have the relevant fluids distributed among three different pore sizes (i.e. gas in the big pores, oil in the intermediate size pores and water in the smallest pores which were never invaded by oil in the first place). How do we look at the fluid distribution? Is it both what I have described? Yes, it could be both, but it would depend on the model considered. In this paper a simple model is used where it is not possible to have more than one phase present in each pore. However, still the model gives valuable insights to the 3-phase problem. If you have a model with angular pores, it is possible to have 1, 2 or 3 phases present in the same pore.

2. On page 367, at the left column before the last paragraph, it says: When gradually increasing the pressure of the invading phase, the entry pressures (2) are met pore by pore and the pores are invaded accordingly. This order is determined by the pressure of the phase that is already present in a pore Pj, which may be different from pore to pore, and the capillary entry pressure Pc;ij. How does it say Pj may be different from pore to pore? I understand that both wetting and intermediate phase pressures should stay constant throughout the gas invasion process. This is my understanding too. The only way I can understand this is that the pressure is different from pore to pore if they are occupied with different phases. The phase pressures should be the same in all pores that are occupied with the same phase. On the other hand, if viscous pressure drops are taken into account during invasion, the phase pressures in the different pores could be different at the same cross-section. But this is not accounted for in this paper, so I find the statement a bit strange.

3. With reference to the last 4 lines on page 369, I assume that type I and type II phase occupancies are more likely to occur in reservoir rocks than type III. I say this because it was mentioned earlier in the paper (end of page 368 and start of page 369) that water would be wetting to gas (i.e. type III) in strongly oil wet pores only if $\sigma_{go} > \sigma_{ow}$, which is something unlikely to happen, right? However, when I consider figure 2 I see the opposite. In figure 2, the b figure cannot occur if $\sigma_{go} < \sigma_{ow}$ which is the case in the reservoir. I agree that $\sigma_{go} < \sigma_{ow}$ is the most likely case in the reservoir. However, the wetting preferences (I) Water (wetting) – Oil (intermediate-wetting) – Gas (non-wetting) and (II) O – G – W (strongly oil-wet) and (III) O – W – G (weakly oil-wet) can all occur even if $\sigma_{go} < \sigma_{ow}$. Looking at Figure 1 (a) there is a small interval of $\cos\theta_{ow}$ satisfying $\cos\theta_{ow}^{*} \le \cos\theta_{ow} < 0$ where $\cos\theta_{gw}$ is positive. This is the oil-wet conditions at which case III can occur. For strongly oil-wet conditions, i.e., when $-1 \le \cos\theta_{ow} < \cos\theta_{ow}^{*}$ and $\cos\theta_{gw} < 0$, case II occurs. The point here is that type III cannot occur in the entire negative interval of $\cos\theta_{ow}$: that would require

the $\cos\theta_{gw}$ line had to end up at a positive value to the left, which would only be possible for the unlikely condition $\sigma_{go} > \sigma_{ow}$. Hence, Figure 2(a)-(b) should both be possible.

4. In figure 2(a), this would occur in the reservoir for secondary gas flood. Figure 2 (b) would happen for tertiary gas flood. What do you think? That seems reasonable. However, in this simple model without hysteresis it would be possible to perform invasion of the different phases in different order and still end up with the same occupancies as shown in Figure 2(a) and (b).

5. On page 371 above figure 3(a), we can use Krw+Kro+Krg=1 to calculate the intermediate Kr from the wetting and the non-wetting Kr's. How is that? For example, if I conduct drainage G/W Kr experiment where Krw decreases from 100% Sw and Krg increases, I could use this equation to calculate Kro as the intermediate phase in WW system? Please advise. This Kr relation is a consequence of the simple model, where (1) only one phase can occupy a single pore, (2) the pore wall boundary conditions are no-slip for all phases, and most importantly, (3) no trapping can occur (there is no pore interconnectivity, meaning that all pores are always available for invasion). In realistic experiments, this relation should not be used to calculate the 3rd relative permeability.

6. In the correlation given just above figure 3(b), I imagine Kro(Sw,Sg) would drop, Krw^{ow}(Sw) would also drop and Krg^{go}(Sg) would increase, right? This would depend on which displacements and invasion processes that are considered (i.e., the saturation paths). The scenario you describe above is just one of many possibilities. In that case (referring to Figs. 3 and 2a), oil must displace water and gas must displace a larger amount of oil than displaced by the water. Another case could be a gas invasion (Krg increasing) where Krw is constant, since gas displaces oil only (Kro decreasing). In this case the oil-water capillary pressure is constant, while Pgo and Pgw are increased. However, if a gas-water boundary has formed, because all oil in water-wet pores are displaced, this correlation is not valid anymore because Type II occur instead (see comment to item 7 also).

7. Do equations 20 and 21 mean that the drop of Krw^{ow} in 2-p flow would equal to the drop of Krw in 3-p flow during gas injection? Does this also apply for the increase in the gas rel perm? In 2-p flow, oil pushes water in drainage whereas in 3-p flow, gas pushes oil and gas also pushes water. Would oil push water as well? Equations 20 and 21 apply to Figs. 2a and 3. Oil could displace water in the 2-phase oil-water case, and in order to have a Krw drop during gas invasion following Eq. (20), oil must continue displacing water. This could occur by assuming an increase in the oil-water capillary pressure during gas invasion. If gas displaces oil only, Krw would be constant and equal to the fixed Krw^{ow} value before gas invaded. (However, a variation in Krw following Eq. 20 could then be modeled by simulating gas invasion from many oil-water saturations.) As long as the water is bounded by oil only, and the situation is given by Figure 2a, you are correct regarding Eqs (20) and (21). The increase of Krg is equal to an increase in

Krg^{go}(Sg). However, if gas has displaced all the oil in the water-wet pores, and gas continues invading water-filled water-wet pores and oil-filled oil-wet pores, the situation would be similar to Figure 4c. Now a gas-water boundary has formed, and in this case the equivalent 2-phase system for determining the 3-phase Krw would be the gas-water system (i.e., Krw(Sw) = Krw^{gw}(Sw), see Eq. (25)). The Krg has become a function of two saturations, and hence Equations (20) and (21) is not valid anymore.

Paper#12 Helland, J.O., Skjæveland, S.M.: "Three-phase mixed-wet capillary pressure curves from a bundle-of-triangular-tubes model," paper presented at the 8th International Symposium on Reservoir Wettability, Houston, TX, May 16-18, 2004. Revised paper accepted for publication in J. Pet. Sci. Eng.

1. How R_{ch} is defined in equation 7? This is a scale parameter in the Weibull distribution. The distribution is more spread out if this parameter is large.

2. Consider eqn 8. If $\alpha=\pi/6$ and $\theta_{pd}= \pi/3$, this will give $\cos(\pi/2) =0$ and this will make b_{pd} zero! Is this possible? It is assumed that θ_{pd} satisfies Eq. (3), i.e., $\theta_{pd} < \pi/3$. However, in view of the fluid configurations, an increase in θ_{pd} above $\theta_{pd} = \pi/3$ corresponds to the cases when the oil displaces all the water in the pore in one displacement. Obviously, in these cases $b_{pd}=0$.

3. Eq 8 suggests that if α decreases, b_{pd} increases implying more water wetness. Here, we could say that lower α may mean higher degree of roughness. Can we say this is consistent and in line with what was mentioned in J. Long et al. (Thermodynamic modeling of contact angles on rough, heterogeneous surfaces) which was Journal#1 in DPE180 on page 179 at the paragraph above section 4? In that paragraph it says increasing surface roughness promotes more water wetness. What do you think? Your observations are correct, but I am not sure if I agree completely with the statement "lower α may mean higher degree of roughness". In our paper we considered pores containing only 3 corners, which are main features of the pores. There could be a significant amount of water in these corners during the invasion processes, and I would prefer to model the corners explicitly instead of "hiding them in the roughness term". Long et al. derived contact angles by assuming certain roughness patterns along the solid surface. In my opinion the surface roughness is a smaller-scale feature than the main pore corners. The roughness/contact angle models of Long et al may be applied along the side walls in the triangular pores rather than their corners. It is also correct that pores containing many narrow corners would give more water-wet behaviour, than other pores of the same size which have only a few wide corners. Whether all small pore corners are modeled explicitly, or a surface roughness model (Long et al) is assumed, both approaches would give more water-wet behaviour than if the rough pore walls were replaced by a smooth pore wall boundary.

4. Does eq 8 also apply in SD? If so, would it be possible to decrease b if θ in SD is higher than θ_{pd}? We know that as θ increases, cos(θ) decreases and hence b decreases! θ_{owr} in SD is defined on the pore wall segments that oil contacted during primary drainage. The water-filled corners are still water-wet, and θ_{pd} remain valid there. We allow $\theta_{owr} \geq \theta_{pd}$, and the positions of any new AMs appearing in SD are calculated by Eq. (11). If the position is smaller than b_{pd}, the AM is pinned at b_{pd}, while the hinging contact angle θ_h decreases toward θ_{pd} as Pcow increases. This is calculated by Eq. 10. If the position is larger than b_{pd}, the AM moves toward b_{pd} during further increase of Pcow. When $\theta_h = \theta_{pd}$, we are back at the end of primary drainage, i.e., (Swi,Pc,max). A further increase of Pc would proceed as a continuation of the primary drainage Pc curve, and then the distance b_{pd} is further reduced, and this reduction is modeled by Eq. (8) using θ_{pd}, not θ_{owr}.

5. The second line under eq 8, which says there is no phase entrapment due to the bundle of tubes model may infer that, in real rocks, entrapment can occur in primary drainage. Swi is also said to be given by this entrapment. Well, I see this may be confusing (at least to me!). In primary drainage, phase entrapment is minimal and unimportant due the high water connectivity throughout the porous network because of water films and content on the surface walls and in pore crevices. Swi is viewed as water content residing in the smallest pores which were not invaded by the applied Pc level. Can we call that amount of water at Swi as trapped, I guess not. I understand that there might be minor amount of water trapped during primary drainage due to end traps. Appreciate your view on this. Thanks I agree with your view. To my defense, this statement was made based on comments from the reviewer. Trapped water can occur in network models, because water-filled pore throats can be bypassed during oil invasion. However, it can be imagined that this is an artifact of assuming idealized pore geometry which does not support correct water connectivity. Further, small amounts of water can be trapped because of water film collapse, which reduces the water connectivity.

6. The condition mentioned at the start of page 104 may mean that no water will flow during gas injection as the gas pressure is not allowed to exceed to levels which will eventually invade virgin water filled pores, right? Correct. This may also mean that Sw value is constant/fixed during gas injection, right? Could be, but not in general. The imbibition before gas invasion makes it possible to displace water from previously oil-invaded pores during gas invasion.

7. If this condition is true then we should consider this when we design 3-p Pc experiments by not allowing the gas to flow into water filled pores which were never invaded by oil in the first place, right? Instead of performing experiments that mimic the model, I would suggest developing models that mimic the experiments. However, the simulations presented in the paper were not in conflict with this assumption, because the level of Pcmax was much higher than the Pc,ow

at which imbibition was terminated, and the 3-phase capillary entry pressures required to invade all the pores that have been invaded by oil previously were also significantly smaller than the entry pressures required for invasion into the water-filled water-wet pores. The assumption could represent a minor restriction if we had chosen to let gas invade the model at the end of primary drainage.

8. I could not follow what was said in the middle paragraph on page 105 about the configurations of F, G, K, L and Q which was said can only occur if the wetting sequence of the three phases changes with the direction of the displacement. I also could not link between this statement and what was mentioned later on in the same paragraph. Appreciate if you could give the essence of this paragraph. Many thanks as always. This paragraph outlines what could happen if the contact angle hysteresis is large. In a two-phase oil-water simulation, configuration E could exist at the end of imbibition, and displacement E→F could occur in secondary drainage. If configuration D has been formed at the end of imbibition, displacement D → B could occur in secondary drainage, and in a subsequent imbibition B → G could occur. For these sequence of displacements to happen, the contact angle hysteresis must be large and satisfy $\theta_{owr} < \pi/2 - \alpha, \theta_{owa} > \pi/2 + \alpha$. Both the water and oil invasion events given above will be forced. A similar reasoning applies to the configurations K and L: The configurations can only occur if the gas-water contact angle hysteresis is large: $\theta_{gwr} < \pi/2 - \alpha, \theta_{gwa} > \pi/2 + \alpha$. But that would require a hysteresis in the oil-water contact angles too, considering the way these contact angles are calculated as explained in the last paragraph in Section 3. Hence, in gas invasion the fluids behave as in a water-wet system, whereas in water invasion, the fluids behave as in an oil-wet system. A similar behaviour (contact angle hysteresis) must be assumed if configuration Q should occur.

   Finally, I would like to point out that to describe contact angle hysteresis in 3-phase flow is an unresolved problem. We need to specify 3 advancing and 3 receding contact angles, and the contact angles should also satisfy the Bartell-Osterhof equation: $\sigma_{gw}\cos\theta_{gw} = \sigma_{go}\cos\theta_{go} + \sigma_{ow}\cos\theta_{ow}$. But this is impossible for all the possible combinations of the receding and advancing contact angles. In our paper we adopted a similar approach as in Piri's thesis, but it implies that the above equation would not be satisified for some displacement sequences.

9. I just want to confirm that the geometric conditions mentioned on page 107 at the third line from the bottom of the page are eq 12 and θ_h, right? The geometric conditions it is referred to are (i) Eq. (12) and (ii) the layer-collapse capillary pressure representing the capillary pressure at which the AMs surrounding the layer meet. The latter condition is given by equations (26), (27), (54) and (55) for the displacements described in Section 5.1 and 5.2.

   I would like to comment that after this paper was published we discovered that layers would be displaced in a piston-like displacement rather than collapse when

the surrounding AMs meet. The piston-like displacement occurs at a more favourable capillary pressure than the capillary pressure represented by the layer-collapse geometrical condition. The piston-like layer displacements are also derived from the MS-P method, and in 2-phase flow, the displacement is described in SPE89428-PA, Figure 4. For 3-phase flow and uniform wetting conditions, layer displacements are described in Paper#10 (van Dijke et al, J. Coll. Int. Sci. 277 (2004), 184-201.). For 3-phase flow at mixed-wet conditions, layer displacements are described in the following paper:

- van Dijke, M.I.J., M. Piri, J.O. Helland, K.S. Sorbie, M.J. Blunt, and S.M. Skjæveland: *"Criteria for three-fluid configurations including layers in a pore with nonuniform wettability"*, Water Resour. Res. 43, 2007, W12S05, doi:10.1029/2006WR005761.

A consequence of this finding is that the configurations occurring in the numerical examples may be slightly different during the simulations. This could also have a minor impact on the resulting capillary pressure curves and their saturation dependencies.

10. Could you please confirm to me the difference in the geometric conditions applied between van Dijke et al. (2004) and Piri and Blunt (2004) from one side, and Helland and Skjaeveland (2004a) from the other side, as mentioned on page 108 at the second paragraph from the top of the page? This paper (paper#12) considers the same conditions as Piri and Blunt (2004), which are the geometric conditions (i) and (ii) from item #9 above, and condition (iii) that we calculate the entry pressure for all geometrically possible displacements (i.e., (i) and (ii) are satisfied) and pick the displacement candidate with the most favourable entry pressure. This would be correct, except that we missed the aforementioned layer displacements as a possibility in the set of geometrically possible displacements. van Dijke et al (2004) accounted for this, but the paper did not consider mixed-wet conditions. Helland and Skjaeveland (2004a) is SPE89428 before revision, and this conference paper only considered (i) and (ii) and did not find the most favourable entry pressure based on a comparison between different possible displacements. However, in the revision, SPE89428-PA, which is part of the syllabus, we managed to include (iii) and the layer displacements so everything is OK in the published article.

11. Table 2 suggests that configuration B can lead to G upon water invasion. Is this a typo? No gas in B, where shall the gas come into G? This is not a typo. There is no gas in B and no gas in G.

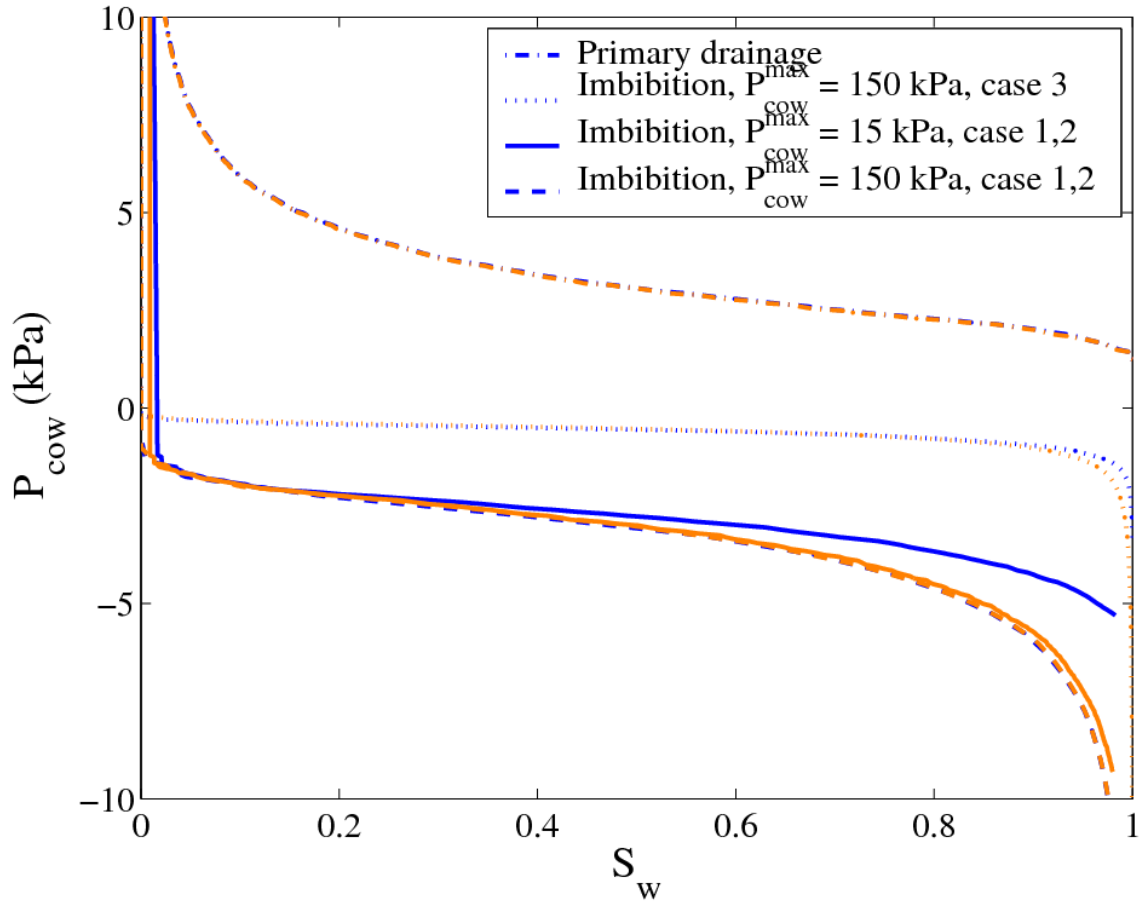
12. Just to confirm reference of displacement from the contact angle representation in table 3: θ_{gwa} would indicate water invasion process, right? Similarly, θ_{goa} would mean oil invasion. Correct, θ_{gwa} for water displacing gas, and θ_{goa} for oil displacing gas.

13. In fig. 6a, why Pc_{gw} curves are on the negative side while the process is drainage G/W? This is an effect of the complex behaviour of the 3-phase entry pressures at mixed-wet conditions in angular pores – they could depend on many parameters.

Even though $\theta_{gwr} < \pi/2, \theta_{gor} < \pi/2$, Pcgw is negative for displacements E→I at these conditions because of Pcgw's strong dependency on Pcow (which is negative) and the oil-water configuration at the end of imbibition. Gas displaces the oil layers and the water in the middle of the pore in one displacement. Obviously, if the pore had uniform wettability and did only contain water (as in circular pores), the behaviour would be more like the curve to the left, representing D → I.

14. At the start of section 6.2 on page 116, it infers that it is possible to have two different pore geometries and yet have same primary drainage curve. How can this is be true? This can be done as explained in the paragraph containing Eqs. (68)-(69). Note that this is exactly what we do in MICP (mercury injection capillary pressure) measurements to obtain a pore-size distribution. MICP is measured, and a bundle of tubes model with circular tube cross-sections is assumed to match the data, from which a pore-size distribution is calculated. If you use this model to simulate primary drainage, you would obtain an exact copy of the MICP data.

15. The legend on figure 7 does not indicate the different models involved (i.e. cylindrical vs triangular tubes). How can we follow the curves? The results from the two models are indicated by different colour only. There are 4 curves for each model. The PD curves of the two models are identical, whereas the imbibition curves from the two models are different. However, they become closer as Pc,ow^max is increased. I attach Figure 7 below in colour, where circular tube model results are shown in orange and triangular tube model results are shown in blue. Note the difference between the solid blue (triangular tube) and solid orange (circular tube) curves representing imbibition from Pcow^max = 15 kPa.



16. Does the Pc ^{max} of 15 kPa curve in figure 7 correspond to both models or only to the triangular one? See item 15.

17. From the last line on page 116 (A-D-C-D-E), it appears that larger pores tend to have configuration E during imbibition more than configuration D. Also it seems that smaller pores cannot attain configuration E. Why? I recall the change of C to E is a function of advancing contact angle as well as water content in the corner. Entry pressure for displacement C→D is a function of the water content in the corner. Entry pressure for displacement C→E does not depend on the water in the corner, but in the smaller pores the water content could be large making C→E geometrically impossible. I also recall configuration B always yields D. What I do not follow is why shall C in small pores does not attain E during imbibition and hence we could perhaps write (A-D-E-C-D-E)? Because we did not account for the layer displacements in this paper, the correct configuration distribution during imbibition would be A-C-C-E-E (if layers form) and at higher water saturations when the layers have been displaced it could be A-D-C-D-D. This will also affect the 3-phase simulations presented later in the paper, to some degree. Since the new layer displacements do not depend on pore size, configuration D and E cannot co-exist at the same capillary pressure if the model are composed of pores with equal shape (different pore sizes though) and equal contact angles in all pores. Hence, layer displacement E→D occurs at the same capillary pressure in all pores. If configuration E occurs, the layer displacement E→D occurs before

the remaining oil-filled pores are invaded by water in a displacement from C→D. The invasion order of the pore sizes for displacement C→E is from big to small pores always, but as explained in SPE89428-PA the invasion order for displacements C → D may be non-monotonic. Small pores can attain configuration E if Pcow^max is very big. If so, configuration E still exists in the large pores.

18. Configuration C in the above point#18, seems to represent the pore size at the minimum point in figure 4, right? This is correct. As explained previously the invasion order of the pore sizes for displacement C→D could be non-monotonic, and depends strongly on Pcow^max. Hence, at some point during imbibition, the configuration distribution in the model could be A-D-D-C-C, A-D-C-C-D, A-C-C-D-D for increasing Pcow^max.

19. Can we tell which process will occur first in imbibition – sequential order of invasion? For instance, if at Swi we have A, B & C configurations, which configuration will first be invaded by water, and what configuration will result? Again, how would this be affected by the start of imbibition, middle and end of imbibition? This should have to do with different pore sizes available during the process of imbibition. *We normally say: big oil-wet and small water-wet pores will first be invaded (assuming bigger pores are oil wet and smallest invaded pores are water wet). Then as imbibition proceeds, smaller pores will be invaded assuming all of the same wettability state.* Since in this paper the assumption is that we do not have water-wet invaded pores then all our pores here are oil wet. The only water wet pores are the non-invaded ones. Can we really tell which process could occur first? I see this might not be possible as these configurations are linked with advancing contact angle conditions such as $\theta <$ or $> \pi/2+\alpha$. Unless we are able to assign this condition to the different pore sizes, then we cannot tell which configuration will be invaded first, right? Please advise. After primary drainage we have configuration A and C (but with the AMs curving toward the corner). Configuration B is already subjected to SI at water-wet conditions, and is not present at the end of primary drainage. If we had uniformly-wet pores we would be able to tell what would happen, based on the value of the advancing contact angle. The same is true in a model with circular tubes. However, the effect of the water in corners of mixed-wet pores makes it challenging to guess what will happen. This is mainly because we in many cases cannot be sure whether displacements C→D or C→E will occur without performing the simulations. A further complication arises because there are no explicit expressions for the C→D and D→E entry pressures. They must be calculated numerically by iterations. Additionally, the results will be affected by the non-monotonic invasion order for the C→D displacements which depends on Pcow^max. It will be really hard to tell the invasion order without performing the simulations. Entry pressure for E→D also depend on Pcow^max. For water-wet conditions, it is slightly easier, because invasion in imbibition is from smaller to larger pore sizes. However in these cases there are also some minor details that are hard to foresee (see Figure in comment to item #8 for paper #8).

Finally, a comment to the "big oil-wet and small water-wet pore" view: The non-monotonic invasion order of pore sizes for displacement C→D in the model with mixed-wet triangular pores, may be viewed as an effect of having larger pores behaving oil-wet while smaller pores behave weakly oil-wet or even water-wet (if entry pressure is positive). This is because the fraction of pore wall area turning oil-wet is bigger in the large pores than in the smaller pores. Or equivalently, in the smaller pores, the fraction of water-wet pore wall area in the corners is larger. Hence, this model may to some degree explain the "big oil-wet and small water-wet pore" view even though the advancing contact angles specified in each pore size is the same. The extent of water-wet capillary behaviour in the smaller pores depends on the value of Pcow^max and hence the initial water saturation (Swi), which in turn can explain the relation between Swi and wettability. Further, the extent of this effect would depend on the pore shapes adopted in the model.

20. I wonder if there is a clearer figure 7 than the one here. I do not seem to be able to see black and grey curves. I cannot differentiate between imbibition curves either. See item 15.

21. During 3-p Pc gas injection experiments, shouldn't Pcow be constant throughout oil and water production? I am referring to the 7 lines at the left column just above figure 9 on page 118. Correct, but I am not sure what you are referring to. The text is related to Figure 8a, and it is explained that the saturation paths are "oil/water iso-capillary pressure curves", meaning that Pcow are constant along each gas invasion path. These paths follow constant water saturations in most of the saturation domain, implying that Pcow is a function of only Sw here, except at small oil saturations where the saturation paths turn and Pcow start to depend on two saturations. This does not mean that Pcow changes along the paths.

22. Why the displacements E to I and N occur at entry pressures increasing with pore size? Reference to the middle of the right column on page 118. This is an effect of the behaviour of the 3-phase entry pressures at mixed-wet conditions and their dependency on Pcow and oil-water configuration at the end of imbibition. See also comment to item #13.

23. Figure 4 says that if Pc^{max} is reduced, water invades larger R during imbibition. No, when Pc^max is reduced the entry pressures becomes higher (less negative). The trend in Figure 4 is also that pores change from being invaded in order of decreasing pore size at very large Pc^max, to become invaded in order of increasing pore size for very small Pc^max during imbibition. Why does the middle of the right paragraph on page 121 say opposite to this? The text you probably are referring to is: "If Pcow^max is reduced, water may only invade the smaller triangular tubes during imbibition, by Fig 4." I admit it is slightly misleading to refer to Fig. 4 in this case because it involves CASE 1 or 2 parameters($\theta_{owa} = 180^\circ$), not CASE 3 parameters($\theta_{owa} = 100^\circ$) that the text on page 121 is dealing with. However, I meant to refer to the non-monotonic

invasion order effect which would also be visible for CASE 3 parameters. The text considers what would happen for an even smaller Pcow^max. Referring to Fig. 4 with Pc,max = 3.0kPa, for $\theta_{owa} = 180^{\circ}$ (CASE 1 parameters) it can be seen that the displacement C→D will occur first into the smaller pores (A-D-C-C), and at higher water saturations larger pores may also become invaded (A-D-C-D). At the same saturations with CASE 3 parameters (less oil-wet conditions) the distribution may look like this instead: A-D-C-C and A-D-D-C. Hence, the desired water saturation before gas invasion begins may be obtained in this last case without having water occupied in the biggest pores. This would give different gas invasion results than for larger Pc^max, where the situation could be A-D-C-D or A-C-C-D.

24. The conclusion (on page 122, at the start of the page) that more than one Pc depends on two saturations due to contact angle hysteresis seems to be a new concept in this area which was not identified before by researchers. Is this correct? The finding is a consequence of this model. However, there are two main concerns: (i) the problem of describing 3-phase contact angles when accounting for contact angle hysteresis (see item #8) – there is no guarantee that the adopted model is sufficient, and (ii) the layer displacements which were discovered after the manuscript was published. Nevertheless, the results indicating a major impact of Pcow^max on the saturation-dependencies of the 3-phase mixed-wet capillary pressures are reliable. We have performed 3-phase calculations in mixed-wet pores again by implementing the layer displacements and neglecting contact angle hysteresis, and found similar results.

25. At the 4th line on page 125 there seems to be a printing mistake: the word “water” should be replaced by “gas”. Is this correct? Correct.

26. In the middle paragraph on the left column in page 127, configuration C is assumed to still be available for water invasion after the first water and gas invasions. If this is true then the second water flood must attain higher injection pressures than the first water imbibition to allow for the displacements of C which did not occur in the first water flood, right? This is correct. Pcow decreases during imbibition. In the following gas invasion, Pcow is constant and equal to the value at the end of imbibition. The second water flood is performed with a constant Pcgo, obtained from the end of gas invasion, while Pcow start to decrease further from the value reached before gas invasion, allowing water invasion into pores holding configuration C which were not invaded by water invasion in the first imbibition.

27. Just above “Summary and Conclusions” section on page 127, could you please explain what is meant by “…since water always starts to invade the smallest pores”. Also appreciate to clarify the essence of equation 70. Thanks in advance. This phrase refers to water-wet conditions. The most interesting results from the model regarding saturation-dependencies and behaviour of three-phase capillary pressure are found for the cases when $\theta_{owa} > \pi / 2$, i.e., the contact angle

represents oil-wet conditions and the pores are mixed-wet. For water-wet conditions, invasion occurs from small to large pore sizes, whereas the invasion order is non-monotonic for mixed-wet conditions. The differences in results between the two models (circular tube vs triangular tube) can also be bigger for the mixed-wet conditions.

Eq. 70 is an expression for the critical value of the advancing oil-water contact angle separating the cases when C→D is forced or spontaneous. The entry pressure is zero if $\theta_{owa}^{crit} = \pi / 2$. If $\theta_{owa}^{crit} > \pi / 2$, the displacement is forced, and if $\theta_{owa}^{crit} < \pi / 2$, the displacement is spontaneous. In all cases, $\theta_{owa} > \pi / 2$. Since b_{pd} is a function of Pcow^max, Eq. 70 represents a measure for a given pore size with radius R on how large (or small) the input value of $\theta_{owa} > \pi / 2$ can be and still SI (or FI) will result for a given Pcow^max. The idea was then to describe the conditions at which the different saturation-dependencies appeared for the 3-phase capillary pressures based on θ_{owa}^{crit} and other parameters. However, it turned out to be difficult and we have not looked more into it afterwards.

Paper#13 Lindquist, W.B.: "The geometry of primary drainage,"J. Coll. Int. Sci. (2005). Article in press.

1. The last paragraph on the first page says that all the arc menisci shown in figure 1 would have identical radii of curvature. Well, this is correct only if all relevant surfaces have identical physical and chemical properties and also all pore throats have the same size, right? No, it is correct. Independent of physical and chemical properties of surfaces and sizes of pore throats, the capillary pressure is the same everywhere in equilibrium, and therefore the radii of curvature of all arc menisci must be the same.

2. Eq 4 presents length of meniscus for one of the shown 3 menisci in figure 1, right? If so why would we then have the summation term? The equation adds up the total length of all 3 arc menisci. Hence, the summation term is required.

3. Talking about simple physics governing Pc phenomenon, I would like to confirm the following with you, please. Consider two glass capillaries immersed in separate water containers. Water is the same in both containers. Both capillaries are made of same glass material. Capillary 1 has smaller radius than 2. Water will therefore increase to higher height in 1 than in 2. Correct. Would the volume and mass of the wetting fluid in both capillaries be identical (although at different heights)? Well, as you know, the equation for capillary rise is given by $h = \frac{2\sigma\cos\theta}{\Delta\rho g r}$. For two capillaries, 1 and 2, we can write

$h_1 r_1 = h_2 r_2 = \frac{2\sigma \cos\theta}{\Delta\rho g} = const.$ Multiplying both sides with 2π shows that the pore wall areas covered by water are equal in the two tubes provided the contact angles are equal. This implies that volume of water and mass of water cannot be equal in the two tubes since the radii are different. The water column weights would be different also. Fluid lifting in the capillary tube is solely controlled by the balance between adhesion forces and gravity (weight of fluid) which are similar in both capillaries. In both capillaries we would have different Pc (higher in 1), different contact angle (lower in 1) and surely different fluid meniscus curvature (higher in 1). Above, you say that the tubes are made of the same glass material, and that the water is the same in both tubes. This should give equal contact angles in both tubes. Another point of interest is the speed of fluid rise. Which tube will encounter faster fluid rise, the smaller or the bigger one? Is it possible that the bigger radius tube have faster fluid rise due to higher permeability? Appreciate your comments. Based on the Washburn Equation, the fluid will rise faster in the tube with largest radius, but it will rise to a lower height than in the smaller tube. If the contact angle is reduced, the speed of fluid rise should also decrease.

4. Considering the same setup as above but with identical Pc applied in both capillaries, I wonder how this would be viewed. Two tubes with different contact angles and pore radii can give equal Pc. This would give equal heights of the water columns in both tubes:

$$P_{c,1} = \frac{2\sigma \cos\theta_1}{r_1} = \Delta\rho g h_1$$

$$P_{c,2} = \frac{2\sigma \cos\theta_2}{r_2} = \Delta\rho g h_2$$

where $h_1 = h_2$ because $P_{c,1} = P_{c,2}$. To have equal Pc, we must also require that $\cos\theta_1 / r_1 = \cos\theta_2 / r_2$. Therefore, if $r_1 < r_2$, we must have $\frac{\cos\theta_1}{\cos\theta_2} < 1$, implying that $\theta_2 < \theta_1$. Since the heights of water column is equal and radii different, the pore wall area covered by water, the volume of water, the mass of water and the weight of water are all smaller in tube 1.

5. I see that figure 11 is in line with the conclusion made in Helland and Skjæveland 2006a publication about the effect of corner filled occupancy and oil layers on the entry pressures. Lower entry pressures are predicted if oil layers are present (may mean higher contact angles and less water in the corners). This, actually, has been observed experimentally with lower entry pressures on poorly cleaned reservoir rocks (higher contact angles with possible oil layers). In here, with figure 11, we see how larger inscribed fluid meniscus radius is associated with increase in contact angle and hence lower entry pressure with higher contact angle. What do you think? This paper considers drainage only, and oil layers would not occur in the corners during that process. Figure 11 only considers the geometry of the MA

tree (for a zero contact angle) and the DA tree (for nonzero contact angles) in 3 triangular pores. The arc menisci seen in the figures are the ones corresponding to the geometrically largest possible radii, at which two or more AMs meet. These radii are much larger than the entry pressure radii obtained by solving Eq. (6) would be. The arc menisci at entry pressure would be located closer to the corners in all 3 cases. However, the arc menisci shown in Figure 11 do represent the configurations at which snap-off during spontaneous imbibition would occur.

Paper#14: Gladkikh, M., Bryant S.: "Prediction of imbibition in unconsolidated granular materials," J. Coll. Int. Sci. 288 (2005), 526-539.

1. I liked the last paragraph in the introduction. Especially when it says:
   "Knowledge of the pore space geometry and wettability conditions allows computing the configuration of two fluid phases in porous media under the control of capillary forces".
   And also when it says: "An attractive feature of the Melrose criterion is its purely mechanistic nature. Knowledge of grain scale geometry is the only prerequisite for its application".
   These statements infer that imbibition is not solely controlled by wettability. It is also controlled by the pore geometry. Well, if this understanding is correct, I find it rather difficult to convince people about it. I had several discussions with few experts in the oil industry and most of them believe it is the wettability condition which controls the whole of an imbibition process (especially spontaneous imbibition); the pore geometry, they say, has nothing to do with it! All this started with me when I observed rather "rare" large SI occurring in carbonate samples which were drained to Swi using dead crude oil at reservoir temperature and net overburden pressure by porous plate. Such a behavior was completely "odd" given the fact the samples had been subjected to crude oil for more than 4 months! In those tests, I had different rock type samples and observed variations in the SI behavior with rock types. I published (as a co-author) those data at SCA 2009 and at the annual SPWLA conference in Texas in 2009. Prof. Svein was one of the co-authors as well in the SPWLA publication. I remember we discussed the possibility of looking into such a debatable data by applying the triangular tube model like the one in Helland and Skjæveland 2006 publications. Is this really possible? I think this is related to item#10 discussed under paper#7. This could be an effect of the interplay between wettability and pore geometry. At moderate Swi, there could be a significant amount of water present in the corners and constrictions in the pore space that would make the water entry pressures positive even though large parts of the pore walls in contact with oil turned oil-wet during ageing. At smaller Swi, the amount of water in these corner areas should be reduced and larger parts of the pore walls have turned oil-wet during ageing, resulting in lower entry pressures. This could be sufficient for a change from SI to FI behaviour. The limiting value of Swi representing a transition from SI to FI behaviour in this explanation would depend on the geometry of the pore space, and in particularly the amount of narrow corners/constrictions present (this

information would of course be difficult to obtain based on core measurements such as MICP-derived pore-size distributions). I have seen your SPWLA 2009 paper, representing an impressive amount of data. In your Figure 20, which does not show much SI, it seems to me that Swi in most cases range from 0.05 to 0.10, whereas in Figure 21, Swi ranges from 0.10 to 0.15. This increase in Swi could be sufficient to obtain a significant SI as seen in the plot, even though all samples were exposed to ageing.

2. Under section 3 on page 528, at the last paragraph: I find it difficult to imagine how water could be trapped during (primary) drainage, and how the trapped water could contribute to Swir. I thought Swir is the result of the tiny pores which could not be invaded by the maximum applied Pc and also the wetting water films (which may not exceed 1% of water saturation!). During primary drainage water will always find the way out thru connected wetting layers along the pore crevices, grooves and corners. This idea was later emphasized on page 529, the last paragraph at the left column. Trapped water can form when water-filled pore throats are bypassed during oil invasion, resulting in isolated water. However, it is questionable whether the water really does become isolated due to high water connectivity, and this could be an artifact of the model (a pack of spheres). Images of real rocks show more roughness and pore corners which could maintain the water connectivity not accounted for by this model. (See also comment #5 to paper #12). It said that the pore cannot be imbibed if the pore(s) invaded by oil (during drainage) are disconnected from the exit. Does this really happen in drainage? No, this disconnection occurs during imbibition. Water invasion into an oil-filled pore during imbibition can only occur if the oil phase forms a continuous path from the pore in question to the outlet. Otherwise, the oil occupying the pore has become trapped. I perhaps think, they mean, this could happen while imbibition is progressing, right? So, all the invaded pores by oil in drainage are connected to the exit. During imbibition, one or some of the pores might get disconnected from the exit due to the imbibition process and its dependence on wettability and geometry. Correct. Again, I am trying to visualize how pores or cluster of pores could get trapped during imbibition. In water wet system oil could be accessed thru water wetting layers, and oil could only be trapped by snap off mechanism. Correct, however oil could also become trapped if oil-filled pores are bypassed and become disconnected during piston-like water invasion. Here, we do not have completed pore trapping. In oil wet system oil is always imbibed. So, what is meant by trapped pores (filled with oil) that cannot be imbibed in this paper? Trapped pores containing oil are isolated and cannot form a continuous oil path from the pore to the outlet.

3. On page 529, at the second paragraph at the right column, they refer to “imbibition and snap off”. Can we say that by “imbibition” they mean piston like displacement and pore body filling? SNAP-OFF: In this paper the pore geometry is given by the space between spherical grains. They refer to snap-off as a mechanism in the pore throats which can be defined as the space between 3 spheres (see Figure 12, 13 for a cross-sectional view and Figure 15, 16 for a 3D

view). It is clear that the throats are constrictions, i.e., "tubes" with varying cross-sections. In such varying throats, snap-off events during imbibition will occur in the narrowest cross-section of the pore throat, and this is modeled as coalescence of different pendular rings (see Figures 12 – 13), and wetting phase fills the pore throat. Notice the similarity to the snap-off mechanism in networks with triangular pore throats for water-wet conditions (see the Piri/Valvatne theses).

However, still piston-like pore-throat invasion could be possible (i.e., occur at a more favourable entry pressure than the coalescence). However, because the throats are formed as constrictions, an interface invading a pore throat from a pore body during imbibition will spontaneously go beyond the narrowest cross-section in the pore throat to find its next stable position that supports the applied capillary pressure. The shape of this interface is modeled as a spherical cap (see figures 15, 16). Further decreases of capillary pressure will result in further stable displacements in which the interface is heading out of the constriction toward the neighbouring pore body.

IMBIBITION is a term used in the paper to describe pore body filling. This is modeled by the Melrose criterion. A pore body in this model would be the void between 4 spheres as shown in Figures 17, 18. Invasion into the pore body occurs when an interface meet a pendular ring and an instability is assumed resulting in complete filling of wetting phase in the pore body, and new equilibrium interfaces are formed. In the Valvatne/Piri models, the analogy to the Melrose criterion (shown in Figure 17, 18) is represented by a mechanism in which water from one or more tubes (pore throats) invades a common node (pore body) resulting in pore body filling.

4. Under section 3.5, they refer to figures 7 and 8 and say that the wetting conditions are governed by the Young-Dupre contact angle equation. This is a new "name" for me. It was always introduced as the Young equation! I could certainly check the reference they quoted for it but thought to see if you have any background about the "Dupre" name in that equation. Thanks You are correct. The Young equation is given by $\sigma_{os} - \sigma_{ws} = \sigma_{ow} \cos\theta$. However, the Young-Dupre equation is very similar: $C_s = \sigma_{ow}(1 - \cos\theta)$, where C_s is the spreading coefficient given by $C_s = \sigma_{os} - \sigma_{ws} - \sigma_{ow}$.

5. Under figure 18 on page 535, it describes that the NW phase withdraws from the pore, leaving W phase completely filling the pore. Well, my understanding in this assumed water wet system is that NW is trapped in the middle of the pore. Therefore, W phase is not completely filling the pore! What is the general understanding here? Shall we say that NW trapping may or may not happen depending on the physical feature (pore geometry and wettability) of the pore? I think this is again related to the discussions/comments in item#10 and #11 in paper#8. Figures 17, 18 describe the Melrose criterion for pore-body filling during imbibition. The figures show one interface (modeled as a spherical cap) and a separate pendular ring of water in between 4 spherical grains. When

capillary pressure decreases, the pendular ring grows and the interface moves further into the space between the spheres. Finally they meet and merge, at which the instability occurs. In most cases it is reasonable to assume that the NW phase can escape out of the pore body through an existing NW-phase path (at least if the number of pendular rings that the interface come into contact with is small). The water must rearrange to a new position in agreement with the present capillary pressure. In this model it is assumed that water rearranges by filling the void, resulting in withdrawal of NW phase and a new stable configuration forms at the given capillary pressure. You may find it strange that this coalescence mechanism occurs in the pore bodies, but this is due to the geometry: The spherical grains allow for pendular rings, which is an attribute that cannot be represented properly in the geometry assumed in the type of network models considered by Valvatne and Piri. In the Valvatne/Piri models, coalescence of the pendular ring and the interface shown in Fig. 18 would rather be represented by a mechanism in which water from one or more tubes (pore throats) invades a common node (pore body) resulting in pore body filling (see the I-n mechanisms in Lenormand's papers). Hence, the coalescence of pendular ring and interface in the void between spherical grains is a mechanism for pore body-filling. However, it could trigger trapping of NW phase at other nearby places (see Figure 19).

6. Clearly, the trapping mechanism described in this paper assumes NW phase entrapment to take place on complete pores or clusters of pores. Figure 19 shows this. Does this really happen in porous media? Do pores get complete isolation from imbibition as shown in figure 19? Water Wetting layers or films should facilitate at least the "partial" imbibition of all pores! The mechanism in Fig. 19 is an example of what I have been referring to as trapping of NW phase because the NW-filled pore is being bypassed during water invasion, resulting in an isolated and trapped NW phase in this pore. The NW phase becomes isolated because it is completely surrounded by wetting phase, and therefore, any wetting layers cannot be responsible for further imbibition in this pore. The NW phase present in this pore cannot be displaced because the NW-phase path from the pore to the NW-phase reservoir at the outlet of the model has become disrupted. Referring to Figure 19, note also that if a snap-off event occurs in the pore throat separating the two NW pore bodies, the NW-phase path could become disrupted, resulting in trapping.

   For mixed-wet conditions, fluid compartments may not become completely isolated in this manner, because both fluids could be connected by wetting layers to their respective reservoirs making further imbibition of a bypassed pore (or clusters of pores) possible.

7. In figure 20, I very much liked the link between the SI curve and the disconnectedness effect of the W phase (the first two curves as they appear on the legend of the plot). Having said this, I could not understand how this disconnection in the W phase could occur with zero contact angles? We could use this reasoning perhaps when we compare WW and OW system! I tend to agree

with your reasoning. However, in the model the thickness of films is not estimated – their existence/absence are assumed only. One could follow the reasoning in the 1st paragraph in section 9.1, that the films are too thin for water transport, making it impossible for water pendular rings that are only connected by water films to reach the input capillary pressure. Considering Hirasaki's papers, it is possible to have – depending on mineralogy and fluid composition – a disjoining pressure isotherm at which no equilibrium film thicknesses exist. Hence films are not present, they collapse immediately. However, I am not sure if this implies that the contact angle must be nonzero.

8. Under 9.1, at the second paragraph, it discusses the effect of the increase in contact angle on the residual NW phase saturation. The first reason given does not seem (to me) to be a reasoning, rather a repetition of the statement! I agree. I would like to see this as follows: When contact angle is high, piston like displacement and pore body filling are favorable over the snap off and hence NW phase may be displaced from the middle of the pores resulting in lower NW phase. But by comparing the plots with and without coalescence in Figure 20, snap-off is never favourable in this unconsolidated Finney pack. Hence, it must be that the mechanism in Figure 19 does not occur as much when contact angle is increased. Reason (2) in Section 9.1, 2nd paragraph, may contribute a bit too.

9. At the end of section 9.2, I liked the link given between the mechanism of imbibition and this specific type of pore geometry which actually emphasizes the dependence of imbibition mechanism, not only on contact angles and wettability conditions, but also on rock type and nature. It says that snap off in this unconsolidated porous medium is not important. This enhances my understanding in point#1 above. Good.

10. I would like to take the advantage of discussing expected Sor from WW against OW systems in imbibition. Figure 22 shows qualitative similarities in Sor between predictions and experiments. It concludes that at higher contact angles we obtain lower Sor. This is in line with my understanding that WW systems always yield higher Sor. However, I still see a debate in the oil industry about this. Some people think that Sor would be much lower for WW system compared to oil wet systems. Most of those people are generally not lab engineers rather filed engineers who evaluate oil productions in the reservoir! This has caused (as I see it) great confusion and debate in the oil industry. The work of *Jadhunandan and Morrow* 1995 suggested that the least Sor may be obtained from mixed wet systems. What is the truth here? Is there an absolute truth in here or it may depend on rock type/nature? I have started to come to a conclusion that it is easier to produce oil from WW systems and that is why people see "more" oil produced from WW reservoirs than in oil wet reservoirs "at the early stages" of production. If we reduce the Pc to negative values and wait long enough we can obtain much lower Sor values in the reservoir thru oil layer production. What do you think? I agree completely with your view on this. Actually, I tend to be a bit worried by the big research efforts aiming at changing wettability of rock samples toward

more water-wet conditions in order to increase the amount of oil produced by spontaneous imbibition. I am worried that this could increase the Sor in the end, since more oil could be trapped at water-wet conditions. However, fractured rock is a different story: If the pore spaces in the matrix blocks are water-wet, water flowing in the fractures could imbibe into the matrix pore spaces and displace the residual oil occupied here. Otherwise, if the matrix blocks are oil-wet, water may only flow in the fracture system under influence of dominating viscous forces.

I also agree with Jadhunandan and Morrow. Also, check out SPE96448 by Spiteri et al (2005). It includes plots relating Swi to Sor for different advancing contact angles based on pore-network modeling results.

11. At the start of page 539 in the conclusions section it says that the hydraulic continuity affected predicted Pc curves only slightly. Well, if I consider figure 21, I see large effect! I agree.

Paper#15 Hassanizadeh, S.M., Celia, M.A., Dahle, H.K.: "Dynamic effect in the capillary pressure - saturation relationship and its impacts on unsaturated flow," Vadose Zone Journal 1 (2002), 38-57.

1. Eqn 4 on page 40 is similar to eqn 42 on page 3400 in a previous publication of Hassanizadeh and Gray which I studied in DPE180 (paper#3). That journal title was "Thermodynamic Basis of Pc in porous media". However, eqn 42 did not have the porosity term. Considering Eqs. (35) and (37) in the paper by Hassanizadeh & Gray (1993), the porosity should also appear in the inequality given by Eq. (42) in that paper, making it similar to Eq. (4). However, if the porosity is just a positive constant, it can be eliminated from both equations.

2. Eqn 6 here on page 40 is also similar to eqn 43 on page 3400 in the previous publication, again with no porosity term. This is because the porosity has been eliminated in Eq. 42 in Hassanizadeh & Gray (1993), but remains present in Eq. 4. In addition, there is an extra "-" term in eqn 6. This is an error. The authors' considerations in the paragraph above Eq. 6, that $\frac{DS}{Dt}$ must be negative when $(P^n - P^w) - P^c$ is positive indicate that the "–" sign on the lefthand side of the equation should not be there. Moreover, there is missing brackets in eqn 6. Yes, this is an error.

3. In the second paragraph under eqn 4, can we say that the movement of the fluids by Pn-Pw is assisted by imbibition capillary forces? Yes, during imbibition capillary forces assist the displacements.

4. Is there a missing negative sign in eqn 8 when this is compared to eqn 7? If there is no printing mistake this would then mean that the decrease in saturation in eqn 7 is equivalent to an increase in the contact angle in eqn 8. What do you think? I don't think there is any error here. θ is volumetric water content in this paper, not contact angle. It is defined as water volume divided by bulk volume, i.e. volume

of solid and pore space. Hence it differs from water saturation S. The relation between these quantities is as follows: $\theta = \varepsilon S = \frac{V_{pore}}{V_{bulk}} \frac{V_{water}}{V_{pore}} = \frac{V_{water}}{V_{bulk}}$. In Eq. 8 it is assumed that the pressure in air is equal to the constant atmospheric pressure. Hence, the derivation of Eq. 8 based on Eq. 7 goes like this:

$$P^c_{dyn} - P^c_{stat} = -\tau \frac{\partial S}{\partial t}$$

$$(P^{air} - P^w_{dyn}) - (P^{air} - P^w_{stat}) = -\tau \frac{\partial S}{\partial t},$$

$$P^w_{stat} - P^w_{dyn} = -\tau \frac{\partial S}{\partial t}, \quad \Psi_{stat} \rho g = P^w_{stat}, \quad \Psi_{dyn} \rho g = P^w_{dyn}, \quad \partial\theta = \varepsilon \partial S,$$

$$\Rightarrow \Psi_{dyn} - \Psi_{stat} = \frac{\tau}{\varepsilon \rho g} \frac{\partial \theta}{\partial t}.$$

5. At the column to the right on page 44 at the line#9 from top of the page, it should read "…prediction of eqn 8 for drainage." rather than eqn 10. What do you think? I think so too.

6. My understanding from this paper is that the externally applied drainage pressure difference (P_1-P_2) between the two phases is higher than the actual microscopic Pc_{dyn} at the interface. Moreover, the pressure drop across the interface during desaturation (which is Pc_{dyn}) – if it could be measured at the pore level – would be higher than the static Pc_{stat} at the same Sw. If the dynamic Pc is the Pc_{dyn} and if the equilibrium Pc is Pc_{stat} what would then P_1-P_2 be? P_1-P_2>Pc_{dyn} >Pc_{stat}? This seems correct from my point of view. I would like to point out the following: There are difficulties involved in measuring dynamic capillary pressure. I do not know if reliable procedures have been established yet. It is also a problem how dynamic capillary pressure should be calculated in pore-network models. The capillary pressures across interfaces moving through throats are described by Young-Laplace equation in this paper (see Eqs. (16) and (17)). These micro-scale capillary pressures are different for different interfaces that are moving. To obtain a value for dynamic capillary pressure, in this paper two different expressions are considered: Eq (19) which would be correct for quasi-static conditions, and Eqs. (20) and (21), which would imply that $P^c_{dyn} > P^c_{stat}$. The latter definition may be viewed as a "macro-scale" dynamic capillary pressure because the phase pressures are volume-averaged. The use of Young-Laplace equation for dynamic conditions at the micro-scale describing moving interfaces is assumed as an approximation because it is strictly known to be valid only in the static case.

7. In figure 8 is the solid line for static and the dots for transient data? What is this symbol on the plot which is set to equal 700 cm.sec?¨ I think that the left-hand side of Eq. (8) represents the y-axis (i.e., the difference between dynamic and static capillary head), and $\varepsilon \frac{\partial S_w}{\partial t}$ on the right-hand side of Eq. (8) represents the x-

axis. The remaining part on the right-hand side of Eq. (8) will then be $\frac{\tau}{\varepsilon\rho g}$ which has units [cm * sec]. I think the circles represent the experimental data, and the straight line could be a fit to the data. The slope of the straight line gives an estimate of $\frac{\tau}{\varepsilon\rho g}$ which was found to equal 700 cm*sec.

8. On page 47 the last two lines before the last paragraph, it says that the Pc curves for faster drainage lie higher than those for slower drainage. Is this also true if equilibrium porous plate experiments were applied? For instance, establishment of drainage Pc curve by porous plate using 5 Pc steps versus 10 Pc steps, apparently the curve established using 5 Pc steps would be considered faster. Would this yield higher curve than the 10 Pc step curve? The two curves should be identical, as long as the measurements are performed after equilibrium is reached (=static capillary pressure). Static capillary pressure curves are unique, and dynamic capillary pressure curves are non-unique (i.e., higher ΔP over the sample gives higher dynamic capillary pressure curves.

9. On page 48 under "Estimation of the Dynamic Capillary Coefficient" instead of eqn 10 or 8 should be 7 or 8, right (printing mistake?)? Yes, probably a printing mistake.

10. In table 3 there seems to be printing mistake in the unit of r_{min}. micro-m is the correct unit! Absolutely.

11. On page 50 at the column to the right, it says, "if ganglia of fluids that have become disconnected from their respective reservoirs constitute a significant portion of the averaging volume, then non-monotonic Pc curves may result. To me this seems unlikely to occur in drainage? It may seem unlikely in real rocks, but it does occur in pore-network models because the idealized geometry does not support realistic water connectivity. Please see my previous comments on this issue.

12. Considering fig. 12 and fig. 13, I have got several questions/comments on them:

    a. Y-axis in fig. 12 is the macroscopic Pc (boundary fluid pressures) and that in fig. 13 is $Pc_{dyn.}$ Correct. However, I think that Eq. (19) and Eqs. (20), (21) describe two methods for estimating P_{dyn}^{c}. It is demonstrated that Eqs. (20), (21) represents an improvement over Eq. (19), since it is more in agreement with the experimental measurements that are discussed.
    b. Why the two data points corresponding to the 5000 delta P in fig. 12 are far away to the left? Could this phenomenon be linked with the phase diagram concept where, high velocity and hence capillary # due to high delta P, produces stable displacement which results in lower Sw? Yes. Another explanation is that wetting phase which has become isolated and trapped (see item #11 above) can be transported along with the non-wetting phase (and finally be produced) if the viscous pressure drop over

the isolated ganglia is larger than the capillary pressure drop. This can occur in flows with large capillary numbers, but it should not be possible in capillary-dominated displacements where viscous forces are small in comparison. In capillary-dominated displacements the isolated fluids are trapped instead. See also the last 10 lines in the paragraph below Eq. 21 on page 50.

c. Checking these data points with the corresponding curve in fig. 13 indicates that macroscopic Pc is larger than Pc_{dyn}. This is also clear from comparing the y-axes in both figures. This is reasonable. Eq. (19) calculates the pressure difference between inlet and outlet fluid reservoirs. Eq. (20) instead calculates an average pressure in each phase. Due to the viscous pressure gradients, the average pressure of the invading phase should be lower than the pressure of this phase at the inlet, and the average pressure of the displaced phase should be higher than the pressure of this phase at the outlet. Eq. (21) gives the difference between these average phase pressures.

d. Why there is drop in pressure for the 5000 delta P curve in fig. 13? See point (b) above, and the paragraph below Eq. (21) on page 50. The oscillations occur because isolated fluid ganglia are being transported and produced, and the pressures in these ganglia are apparently accounted for in the calculation of the phase pressures by Eq. (21). However, it is unclear to me whether these ganglia should be accounted for in the phase pressure calculations.

13. In eq 25 the first "-" sign should be "=", and the "-" sign in the middle should be "+". What do you think? I agree. This should be in agreement with Eqs. (9), (24).