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Interactive comment on "Hydraulic fracturing in thick shale basins: problems in identifying faults in the Bowland and Weald Basins, UK" *by* David K. Smythe

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Smythe seeks to minimise differences in our view of the hydrogeology of faults by recourse to semantics (the definition of 'inherent'). This will not do. Our views are not close. No amount of special pleading over the interpretation one may put on the word 'inherent' will change that consensus. I cited textbooks in support of my position because these best represent the consensus view in the discipline. In support of his own position, Smythe quotes from a German text which makes a statement which is reasonable (after all, excessive water use or the usual spillages of any industrial sites can 'entail considerable environmental risk, particularly when it comes to water resource conservation' – at least in administrations with poor regulatory processes).

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This point has been made repeatedly in major reviews, such as those of Mair et al. (2012) and Masters et al. (2014). However, the quotation from the German text makes no mention of faults, and thus in no way supports Smythe's central and erroneous contention in relation to them.

Having been forced by my comments to acknowledge that he misleading cited irrelevant papers concerning karstified limestones, Smythe clings to the wreckage of his argument by referring to (though not, I note, citing any literature to support) deep circulation systems that are very definitely hosted by limestones. Again, citation of some literature - any literature – that actually substantiates his minority-of-one claims about hydraulically continuous high permeability in faults traversing thick shale sequences might have helped take the argument forward. However, he has signally failed to cite any such literature, for the simple reason that no such literature (and no such phenomenon) exists. (Incidentally, Smythe does not bother to cite any of the many papers on the origin of the Bath hot springs; had he referred to the literature he might have discovered that, although the Carboniferous Limestone source and approximate minimum age of the waters ($\sim$ 1,000 years)are now reasonably well constrained (Edmunds 2004), the actual location of the recharge area has never been definitively established; while the Mendips is widely presumed (e.g. Atkinson and Davison 2002), other karst hydrogeology specialists argue convincing for a South Wales source area (Wilcock and Lowe 1999)).

Again, without bothering to quote any relevant sources, let alone engaging with the irrefutable temporal sequence of hydraulic head conditions induced by shale gas operations, Smythe simply states that "upward flow is possible; the questions which remain the subject of debate are the precise mechanisms, and the timescales". As I pointed out in my earlier comment, no one denies that upward hydraulic gradients are temporarily established during the very brief period of hydraulic fracturing. There is no doubt about "timescales" – fracking takes days, whereas depressursation of wells to allow gas to enter them persists as long as the well is in production, and thus ensures

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downward gradients over periods of years to decades. Unless we are to set aside the basic laws of physics, there is no legitimate “debate” about this. Given the miniscule permeability of the unfractured shale beyond the artificially fractured zone (cf Yang and Aplin 2007), re-establishment of an upward gradient after cessation of production (and thus suspension of active depressurisation) is unlikely to occur over anything less than geological time. In any case, in the UK, the history of ongoing uplift that commenced in the early Cenozoic (e.g. Westaway 2009) means that over-pressured conditions are not anticipated in onshore unconventional reservoirs even before exploitation – let alone after years or decades of sustained depressurisation. Undaunted, Smythe goes on to say “conclusion, [sic] upflow can happen, driven by several different forces”. No. Anyone who has read and understood the mainstream hydrogeological textbooks I quoted in my previous comment will know that there is only one “force” that drives groundwater flow, and that is hydraulic head - a summation of pore pressure and relative elevation (a proxy for the effect of gravity). No other forces drive upward flow – or flow in any direction. This again is basic physics.

Smythe affects to suggest – without offering any argument or references in support of his position – that the examples of saline springs which I referenced do not constitute an appropriate analogy. Why not? Until some argument to the contrary is proffered, the analogy stands unchallenged. Furthermore, to argue that the analogy “evidently excludes gas (especially methane) migration” is to miss the point: these springs do not emit methane. In fact methane migration through groundwater is a very slow process – unless it is at very high bubble pressures (which is not to be expected in the UK, given the history of Cenozoic uplift mentioned above). Other than that the only mechanism of transport is by diffusion through the groundwater, which is invariably far slower than even the slowest advection (again see the standard groundwater textbooks I cited in my earlier comment). For this very reason, the flooding of methane-emitting mines in the UK invariably leads to an abrupt cessation of methane release (Jardine et al. 2009; Younger 2014). This, it should be noted, regularly puts abandoned mine methane capture systems out of business – and this in a hydrogeological environment that is

many orders of magnitude more permeable than even the most well-fracked shale gas reservoirs (Younger 2014). Smythe indulges in a nostalgic reflection on the bad old days of dilute-and-disperse approaches to management of contaminants. As it has long been accepted that it is the loading (dimensions of M T-1) rather the concentration (M L-3) that controls the dosage affecting sensitive biological receptors, dilute-and-disperse has been out of regulatory fashion for decades, and has no relevance to this debate. At no point in my text did I advocate a return to that approach. I was simply arguing that undetectably low concentrations effectively equal zero pollution. It is not "invalid" to argue as I did. My arguments instinctively follow the common approach to first-tier hydrogeological risk assessment (e.g. Banwart et al.2002) in which a breakthrough concentration of 100% of source strength is routinely assumed – not merely the 90% of initial concentration which Smythe triumphantly quotes (finally citing a source, Gassiatt et al. 2013). Assuming 100% is routine because it defines the worst-case scenario. At no point in my comment did I argue for any lesser percentage. Nevertheless, my paragraph began with the words 'let us suspend disbelief'; if we decline to do so, my arguments become even less assailable.

Smythe appears to assume that the dissolution of halite is the principal source of salinity in the Permo-Triassic sandstone aquifers of the UK. This is erroneous. Sources of salinity at depth in UK aquifers are many and varied, including trapping of ancient sea waters, dissolution of silicate minerals over very long time-scales, possible 'membrane filtration' through shales subject to high hydraulic gradients, and sub-permafrost solute exclusion during the Quaternary cold periods (see Younger et al. (2015) and extensive literature cited therein). Localised production of saline water by evaporite dissolution within the Mercia Mudstone Group has long been recognised, of course, and diffusional transfer into adjoining aquifers over geological time has been documented (see, e.g. Younger 1995). The lack of rapid circulation at depth in deeply confined aquifers means that such diffusion can indeed contribute to the salinity in deeper aquifers, though such evaporite sources are by no means the principal explanation of the ubiquitous saline groundwaters encountered at depth in UK aquifers (see, e.g., Bottrell et al. 2006).

Smythe then proceeds to “examine the Fylde evidence” – though, breathtakingly, this examination does not engage with a single one of the many hydrogeological studies of the Fylde Aquifer published over the decades (including Allen et al. 1997; Barker and Worthington 1973a, 1973b; Brereton and Skinner 1974; Lovelock 1977; Oakes and Skinner 1975; Seymour et al. 2006; Tellam and Barker 2006; and Worthington 1977). Had Smythe taken the time to read these (and many other) publications on the Fylde Aquifer, he would have had the opportunity to understand that the distribution and movement of water in the aquifer is very well understood, and leaves no room for his uninformed speculation. He would also have had the opportunity to appreciate that historical pumping of formerly freshwater boreholes in the aquifer has led to saline intrusion – principally from saline waters down-dip within the Sherwood Sandstones. Being unsupported by any engagement with the extensive literature on the Fylde Aquifer, Smythe’s special pleading DOES NOT make the case for the persistence of freshwater to great depths – on the contrary, the loss of freshwater status in the boreholes he mentions is but another example of the worldwide phenomenon of saline intrusion, which demonstrates the opposite of what Smythe claims. A basic understanding of the tendency for higher-density saline waters to sink beneath lower-density freshwaters, and the delicate balance between them (as per the well-known Ghyben-Herzberg relationship that is documented in all groundwater textbooks) might have spared Smythe from making his rather embarrassing suggestion of the occurrence of freshwater beneath the saline water within the aquifer. Hence Smythe’s claim that “the EA has written off a past and future potential groundwater resource in the Fylde“ is not in the least justified by the brief notes from his own partial study, which has resolutely ignored the work of the bona fide hydrogeologists I cite above.

Other commentators have dealt with Smythe’s defamatory pronouncements on what he alleges to be a systemic lack of professionalism, honesty and integrity on the part of the Environment Agency. He has yet to undertake (let alone cite, for none exists) any sociological investigation that would remotely support his scurrilous remarks. Without rigorous evidence of a proper examination of the performance of the EA by appropri-

ately qualified social scientists (which Smythe is not) his own anecdotal “evidence” has no place in a supposedly scientific article.

Smythe closes his response to my earlier comment by remarking that he finds my “comments to be somewhat dogmatic in tone”. While I do not consider that personal taste in literary ‘tone’ is a legitimate point for scientific discussion, this curious remark prompts me to respond that this is the first time I have had to deal with a supposedly scientific debate in my core discipline (hydrogeology) in which I have had to expend much of my text explaining (and now re-explaining) absolutely basic points of hydrogeology that anyone could learn by spending a little time reading the various textbooks I cited in my previous comment. It is a characteristic of entry level classes in any subject that they tend to be rather ‘dogmatic’, since those who are not versed in the basics of any discipline are not yet at a sufficient level of understanding to engage in sophisticated debate. Accordingly, I would not presume to submit a discussion on points of deep seismic surveys (in which Smythe is expert and I am not); but were I rash enough to do so, I would not be in the least surprised if my arrogance were to be met by somewhat dogmatic answers.

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