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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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I will begin my review with some general comments, before providing section-by-section comments in greater detail.

The most obvious aspect of this paper is that it's not really a scientific paper at all, in any normal sense of the term: i.e., the presentation of a hypothesis, and the marshalling of evidence for and against said hypothesis, in an impartial a manner as possible, and contributing something novel that advances our science understanding, be it by collecting new datasets, by developing a novel theory, by inventing a new method for data analysis, or by contributing to the sum total of human knowledge in some other way.

This paper does none of these things – the only information that we gain by reading this paper is that we learn about Smythe's opinions about various other papers in the literature, as well as his opinions about particular operators currently active in the UK shale gas industry, governmental regulators and the wider UK geoscience academy. As pointed out in the previous comments (SC2, SC4, SC6), this paper even falls short in this limited remit, missing out important papers that contradict the arguments made, and misrepresenting other papers, twisting them to support the authors position when in fact they provide evidence against it.

In places this manuscript is little more than a diatribe, insulting the professional capability of the many engineers and scientists employed by government bodies such as the Environment Agency (EA) and the Health and Safety Executive (HSE). The author is entitled to express his opinions of course, but I do not believe that a scientific journal such as Solid Earth is the appropriate place to do so.

Comments on a section-by-section basis follow:

Section 1: Smythe makes claims about the different degrees of faulting in the USA vs the UK. However, no evidence is provided to justify these claims – a reference to an as-yet unpublished and un-peer-reviewed desk study, the contents of which are not available for readers to consider for themselves, cannot be considered evidence. These claims must be removed unless robust evidence can be provided. Even if a reference were to be provided, I do not believe that such conclusions can be reached from a purely desk-based study because, as noted by Younger and Westaway (2014), "Different geological surveys apply different standards as regards the scale of structures worth recording, depending on the purpose of the mapping. Typically, with much larger territory to map, US survey teams undertaking general mapping record less detail than their UK counterparts".

Section 1.1: While it would indeed be better if all of the datasets used in the Fisher and Warpinski (2012) study were made publicly available, I am in agreement with the comments by Westaway (SC2) that “partial disclosure of ‘anonymized’ proprietary datasets is common in Earth Science, when the only alternative would be non-disclosure, which would benefit no-one.” The underlying insinuation that Smythe is clearly trying to make in this section is that Fisher and Warpinski have somehow distorted or fabricated their results. He has no basis for making this insinuation. Instead, one is left with the view that the hard evidence from hundreds of shale gas wells presented by Fisher and Warpinski directly contradict his own views, and therefore he is trying to smear their professional integrity, rather than engaging with their results.

Fisher and Warpinski is not the only paper to make such observations about fracture height growth during hydraulic stimulation. Maxwell (2011) has independently produced similar plots for a range of shale plays, showing very limited hydraulic fracture growth. Maxwell (2011) has not been referenced in this paper.

For my own part, I have worked with a large quantity of microseismic datasets collected during hydraulic fracturing operations, and the conclusions reached by Fisher and Warpinski match with my own experience: hydraulic fractures rarely extend more than about 50m above the injection zones, and in the most extreme cases have only propagated a few hundred metres above the injection zone, even where they have intersected pre-existing faults. There are also sound physical reasons to expect the height growth of hydraulic fractures to be limited, as described by Flewelling et al. (2013) (another important paper that this manuscript fails to reference) and Maxwell (2011) for example.

Smythe then claims that microseismic data cannot be used to detect fluid movement along a fault. As evidence, he references a paper that uses microseismic data to identify where fluids have moved along a fault. This argument is self-contradictory. Considering the issue more broadly, the interactions between hydraulic fractures and faults are commonly observed and tracked using microseismic data (e.g. Wessels et al., 2011; Maxwell et al., 2008; Maxwell et al., 2009; Hammack et al., 2014). That microseismic events can be used to track pre-existing discontinuities has also been

demonstrated in controlled, laboratory experiments (e.g., Stanchits et al., 2012). Nor is evidence for hydraulic fracture-fault interaction uncommon: microseismic monitoring service providers have estimated that they see evidence for interactions between hydraulic fractures and faults in about 30% of the datasets they acquire (Verdon and Kendall, 2015).

The next assertion that is made is that faults intersected by hydraulic fractures would allow previously-trapped gases to migrate once released by fracking. The erroneous hydrogeological assumptions made have been described at length in the comment by Younger (SC6). Smythe misses the fact that the primary permeability pathways created by fracking must be linked to the well bore. It is possible (indeed it is common) that a hydraulic fracture will intersect a fault. However, if and when it does so, the easiest flow pathway in terms of permeability out of the reservoir rock mass will be along the propped fractures and thereby to the production well. Unless the fault was already well-connected to significant portions of the rock mass (in which case gas will already have escaped), it will not become well connected to the rock mass except through the propped fracture system created by hydraulic fracturing.

Once the stimulation is complete, an operator will begin to produce fluids from the well. As such, the pressure gradient through the permeable system created by hydraulic fracturing will be from the reservoir rock mass, along the propped hydraulic fractures, and into the production well. As such, the direction of fluid flow will follow the most permeable pathway (the propped hydraulic fractures) and the pressure gradient (along said fractures to the wellbore), meaning that the fault is unlikely to provide a conduit for flow away from the reservoir. Engelder (SC4) and Younger (SC6) both raise this same point in their comments.

I also note that Smythe has neglected to reference the most significant study that has been performed into this issue to date (Hammack et al., 2014). The USA National Energy Technology Laboratory conducted an experiment where a faulted section of Marcellus Shale was fracked, using fluids containing chemical tracers to track subsurface fluid movement. Hammack et al. (2014) monitored for said frack fluid in overlying layers, as well as for increases in methane flux, or even for pressure changes, that would indicate a hydraulic connection between the reservoir and overlying layers through the faults that cut the shale layer. The interactions between hydraulic fractures and faults were observed using microseismic monitoring (as described above). However, no evidence for upward fluid migration or hydraulic connection from the Marcellus into overlying layers, despite the interaction between hydraulic fractures and faults. This paper provides a direct counter-example, with hard evidence, showing that the claims made in this paper do not correspond to reality.

Section 2: This section reads more like Smythe is trying to settle scores with other academics than trying to enlighten the reader. For my own part, I have never tried to imply that the hydraulic stimulations into conventional reservoirs performed in the 1980s are identical to those which are proposed now. Smythe would be better off referencing written work, rather than trying to assume what might have been said at a public meeting based solely on powerpoint slides which are, obviously, an incomplete record of the event.

Regardless, it is equally misleading to suggest that the differences between hydraulic fracturing in shale and in conventional reservoirs is a binary one. Hydraulic fracturing technology has seen continuous evolution over the last 65 years – I recommend for example the history provided by Montgomery and Smith (2010). Many of the stimulations now carried out in conventional reservoirs would meet the "super-fracking" definition defined by Turcotte et al. (2014).

The DECC statement provides an accurate statement of the present situation, and could be referenced without the need for 2 pages of additional discussion. However, if Smythe does wish to persevere with an extended discussion about the state of hydraulic fracturing in the UK, then the situation offshore in the North Sea must also be discussed, where multi-stage fracking in horizontal wells using large fluid quantities – what is referred to in this paper as "super-fracking" – is a common activity (e.g. Cham-

bers et al., 2010; Schrama et al., 2012).

Section 3: In this section, Smythe presents a re-interpretation of the fault position presented by Clarke et al. (2014). That different geoscientists sometimes come to different geological interpretations, is hardly surprising. While the exact hypocentre location as found by ourselves (O'Toole et al., 2013) and Westaway (2016) are slightly different, I find myself in broad agreement with the overall theme of Westaway's comments here (SC2). This section is not providing anything new that has not already been published.

Other authors have already published different interpretations of the earthquake and fault discussed by Clarke et al., (e.g., O'Toole et al., 2013; Westaway, 2016). The simple fact of the matter is that these events were not well monitored – they were recorded by 4 stations at $\sim$100km distance, one fully functional local (<5km distance) station, and one partially functional local station (where the vertical component was functional, but the horizontal components were not). So I'm sure that if we were to give the data to another 10 different geophysicists, they'd come up with 10 slightly different interpretations. But publishing 10 different scientific papers every time would not serve to advance our scientific understanding in any way, and would not be an appropriate use of the academic literature, unless such a re-interpretation substantially changed our understanding of how hydraulic fractures and faults interact (which this paper does not).

It should also be pointed out that the interpretation of Clarke et al. (2014) is based on the digital 3D seismic volume itself, while the re-interpretations made by both Westaway (2016) and this paper are reliant on a small picture of a 2D slice through the data. Therefore, all other things being equal, one would expect the interpretation presented by Clarke et al. to be more robust. Again, as stated in Westaway's comment (SC2), it would of course be better if the full 3D volume was made available to academics, but it remains a commercially sensitive dataset.

Section 4: This section discusses conventional oilfield activities in the Weald Basin at Balcombe. The UK's 2015 Infrastructure Bill precludes hydraulic stimulation taking place at depths of less than 1,000m. The wells in question are at depths of approximately 800m. It is therefore difficult to see the relevance of this section to the development of hydraulic fracturing in the UK. Smythe's apparent unfamiliarity with significant legislation relating to hydraulic fracturing in the UK is discussed in more detail in my comments on the following sections.

Section 5: I find myself in agreement with the comments made by Westaway (SC2) and Engelder (SC4) that the review of hydraulic fracture-fault interactions is flawed. It misses the most significant study performed to date of hydraulic fracture-fault interaction (Hammack et al., 2014). In as much as this study conducted a real experiment, rather than a modelling study, it's conclusions should carry far more weight than modelling studies. The fact that it is not included in this review is surprising. I share the same concerns about the realism of some of the referenced models as raised by by Engelder (SC4), particularly that they do not simulate flowback and subsequent hydrocarbon production, which produces an inward pressure gradient. Myers (2012) does not appear to include gas production in their simulation, while Cai and Ofterdinger (2014) do not even appear to include flowback of fracking fluid in their simulations.

Section 5.3: Smythe presents the Bradford County case (Llewellyn et al., 2015) as an example of fracking-related contamination caused by faulting, and therefore to provide a justification of his concerns regarding faults intersecting shale gas reservoirs. I do not have Engelder's (SC4) in-depth and first-hand knowledge of this particular case study, but as he points out, it is obvious even to the layman that Smythe (2016) is severely misrepresenting this case study, because it does not in fact support his case (i.e. that faults can provide a pathway for contamination from fracking depths to the surface). I note in passing that Smythe has made the same misrepresentations about Llewellyn et al. when submitting objections on behalf of an anti-fracking activist organisation to planning applications made by UK operators (e.g., Smythe, 2015).

This paper attempts to use Llewellyn et al. (2015) as a case study where a fault has

provided a pathway for fluids to migrate from depth to contaminate groundwater. In fact, if fluids have propagated upwards from depth (and I note the caveats mentioned by Engelder, SC4), the migration pathway would be the poorly-cemented wellbore. Had the well been drilled with the appropriate procedures, no groundwater contamination would have occurred, regardless of the presence of the fault.

In this case, faults and/or fractures seem to have only provided a pathway for fluid migration in the upper 300m or so of the subsurface. To use an example of a fault providing a flow conduit within a few of hundred metres of the ground surface to imply that faults at thousands of metres depth will provide flow conduits to the surface, demonstrates a poor understanding of hydrogeology for a number of reasons (also discussed by Younger, SC6): - In these near-surface layers, compressive stresses will be very low, and so faults and fractures are much more likely to provide conduits to flow. At depth, as compressive stresses increase, faults and fractures will become less conducive to fluid flow. - Faults at depth are more likely have experienced more diagentic effects (mineral precipitation for example) that will occlude flow pathways, again making them less conducive to flow. - A fault will cut through different lithologies at different depths, with clay-rich layers likely to smear along the faults, affecting the permeability. A fault in near-surface layers will be likely to have different properties to the same fault at greater depths. So the fact that fluids might have flowed along a fault in the near surface does not mean that the same fault could provide a flow conduit from 2km depth to the surface.

It is clear that Smythe is twisting the conclusions reached by Llewellyn et al. (2015) because he is otherwise unable to find a single case example of fracking-related contamination via fault leakage. I am happy to add the best of my knowledge to the (far more extensive) best of Engelder's knowledge (SC4) in stating that the literature does not contain such a case.

Section 6: I will refrain from detailed comment on what is essentially an attempt to discredit the various regulatory bodies that are involved in the UK. I am in agreement with Westaway (SC2) and Younger (SC6) that the claims made about regulations in the UK are not appropriate. I believe that these agencies are capable of stating for themselves whether or not they are competent to perform the job in hand. Indeed, should Solid Earth decide to proceed with publication of this paper, I would suggest that the editors contact representatives of the EA and HSE for comment prior to doing so, since this paper makes a direct challenge as to the competence of their employees.

However, I will point out some of the more egregious examples of the invective used in this section.

Smythe claims that Younger (2014) misunderstands the "fault problem", and accuses him of "ignorance" (without any attempt to produce evidence or explanation as to why Younger's position is incorrect). This is simply a personal slur without any basis. In the General Terms section of the Solid Earth website, it is stated that "The SE editorial board reserves the right to remove referee reports and any other comments if they contain personal insults." I would assume that the same conditions apply to manuscripts submitted to the journal.

Smythe claims that the EA "has a poor understanding of some the resources for which it is responsible". Unless he has carried out some sort of performance review of the EA's capabilities, he is in no position to make such a statement, which insults the competency of the many hundreds of engineers and scientists employed by the EA, and is completely unacceptable for the scientific literature.

The statement that UK academic community "cannot be relied upon to comment independently or authoritatively on UK shale development" is an obvious attempt to smear both the competency and professional integrity of the entirety of the UK earth science academia. I ask the editors of Solid Earth to consider whether the EGU is serving its community effectively by hosting such invective.

Section 6.4: The author discusses UK shale gas regulations with respect to the the Preese Hall well, drilled in 2011. However, the regulatory landscape has changed

substantially since then. Most notably, the Infrastructure Act (2015) has imposed numerous additional regulations on the industry. By failing to mention these additional rules, this paper paints an inaccurate picture of present regulations (available online at: http://www.legislation.gov.uk/ukpga/2015/7/section/50/enacted).

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References: Chambers K., Kendall J-M., Barkved O., 2010. Investigation of induced microseismicity at Valhall using the Life of Field seismic array: The Leading Edge 29, 290-295.

Flewelling S.A., Tymchak M.P., Warpinski N., 2013. Hydraulic fracturing height limits and fault interactions in tight oil and gas formations: Geophysical Research Letters 40, 1-5.

Hammack R., Harbert W., Sharma S., Stewart B., Capo R., Wall A., Wells A., Diehl R., Blaushild D., Sams J., Veloski G., 2014. An Evaluation of Fracture Growth and Gas/Fluid Migration as Horizontal Marcellus Shale Gas Wells are Hydraulically Fractured in Greene County, Pennsylvania: EPAct Technical Report Series, U.S. Department of Energy, National Energy Technology Laboratory, Pittsburgh, PA, NETL-TRS-3-2014.

Maxwell S.C., 2011. Hydraulic fracture height growth: Canadian Society of Exploration Geophysicists, Recorder 36(9), 18-22.

Maxwell S.C., Shemeta J., Campbell E., Quirk D., 2008. Microseismic deformation rate monitoring: SPE Annual Technical Conference, SPE116596.

Maxwell S.C., Jones M., Parker R., Miong S., Leaney S., Dorval D., D'Amico D., Logel J., Anderson E., Hammermaster K., 2009. Fault activation during hydraulic fracturing: SEG Annual Meeting, Expanded Abstracts.

Montgomery C.T., and Smith M.B., 2010. Hydraulic fracturing: history of an enduring technology: Journal of Petroleum Technology, December 2010 Issue, 26-32.

O'Toole T., Verdon J.P., Woodhouse J.H., Kendall J-M., 2013. Induced seismicity at Preese Hall – A review: EAGE Annual Meeting, Expanded Abstracts.

Schrama E., Naughton-Rumbo R., van der Bas F., Shaoul J., Norris M., 2012. Tight gas horizontal well fracturing in the North Sea: Offshore Engineer, October 2012, 45-48.

Smythe D.K., 2015. Planning application no. ES/3379 by Island Gas Limited to drill at Springs Road, Misson, Nottinghamshire: Objection on grounds of geology and hydrogeology. Downloaded from http://bassetlawagainstfracking.co.uk/wp-content/uploads/2015/12/DAVID-SMYTHE-REPORT-Dec1st-2015.pdf on 12.2.2016.

Stanchits S., Surdi A., Edelman E., Suarez-Rivera R., 2012. Acoustic emission and ultrasonic transmission monitoring of hydraulic fracture initiation and growth in rock samples: 30th European Conference on Acoustic Emission Testing, University of Granada.

Verdon J.P. and Kendall J-M., 2015. Response to Call For Evidence on the Environmental Risks of Fracking from the Commons Select Environmental Audit Committee. Downloaded from http://data.parliament.uk/writtenevidence/committeeevidence.svc/evidencedocument/enviror audit-committee/environmental-risks-of-fracking/written/17012.pdf on 12.2.2016.

Wessels S.A., De La Pena A., Kratz M., Williams-Stroud S., Jbeili T., 2011. Identifying faults and fractures in unconventional reservoirs through microseismic monitoring: First Break 29, 99-104.

Westaway R., 2016. The importance of characterizing uncertainty in controversial geoscience applications: induced seismicity associated with hydraulic fracturing for shale gas in northwest England: Proceedings of the Geologists' Association, in press.

Younger P.L. and Westaway R., 2014. Review of the inputs of Professor David Smythe in relation to planning applications for shale gas development in Lancashire (planning applications LCC/2014/0096 /0097 /0101 and /0102) and associated recommendations. Downloaded from http://eprints.gla.ac.uk/108343/1/108343.pdf on 12.2.2016.

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