

**Final author comments on '*Hydraulic fracturing in thick shale basins: problems in identifying faults in the Bowland and Weald Basins, UK*'**

**David Smythe**

**Introduction**

My paper has aroused far more discussion than any other paper ever published in SED; therefore it is worth recording why this has happened, and what I propose to do with the manuscript.

In a covering letter I explained why I was submitting my manuscript to SED:

*“I am submitting this paper to SE, rather than to a journal with a track record in applied geophysics, because I like your transparent reviewing and open access policies. However, I am aware that your publication history to date and the composition of your editorial board do not appear to include explicit expertise in applied geophysics or petroleum geoscience.*

*I considered submitting to HESS, but although my paper crosses several disciplines, on balance it is more tectonic than hydrogeological. So if you feel that the paper is not for SE then I won't be offended; on the other hand it could be regarded as an opportunity for you to expand your coverage. In any case I am confident that this paper will gain a wide readership.”*

**The referees**

I unwittingly gave Dr Rossetti, the topical editor, a lot of work, because he found, as I anticipated, that it was very difficult to find referees. For the record, I had written in my covering letter on submission:

*“It is difficult to find suitably qualified referees. Few potential academic referees are familiar with the details of hydrocarbon exploration, even conventional, and in addition there is a lot of controversy around proposed fracking in the UK. There are several UK academics who consider themselves to be experts in the field of unconventional hydrocarbons, but they cannot be trusted to write an unbiased report, because they have close links to industry and are actively promoting fracking. I was even libelled in the national press by one of them 18 months ago.”*

Also, for the record, I proposed five knowledgeable and impartial referees, declaring my personal connections as follows (names and affiliations have been redacted to preserve their anonymity):

*“However, I propose four academic referees and one ex-industry referee for you to choose from, and each of whom I think I can trust to write an expert and fair report:*

*A is Professor of ... at .... He has published many papers and has obtained many industry grants. He will be familiar with the applied geology and exploration content of my paper, and is knowledgeable about the stratigraphy of the basins involved.*

*B is now retired, he worked mainly in the ... oil industry, rising to become a Vice-President of ... , one of Canada's major oil companies. However, he has also published 15 papers on geophysics and tectonics. He is familiar with seismic reflection data, and with UK tectonics.*

*C is director of the ... Group and the ... . His research focuses on computer simulation of complex fracture models. As an expert on US exploration ... he will be familiar with the modelling and well logging discussion in the paper, if not with seismic reflection and UK geology.*

*D has published several papers on fracking. He is a petroleum geologist who formerly worked in the oil and gas industry. He heads the ... group on fracking, industry-funded.*

*E is Professor of Hydrogeology at ... [he/she] is involved in shale gas exploration risks ... and will be familiar with the hydrogeological aspects of the paper, if not with the details of UK geology.”*

I also declared personal links to four of the five suggested reviewers proposed above:

*“P: I have talked to him by phone about faulting and fracking.*

*Q: [We] have given a joint public lecture in ... about shale gas exploration.*

*R and S: ... friend and former colleague at ... up to [n >17] years ago.”*

I did not name anyone whom I did not wish to be a reviewer – an option often offered in manuscript submission to other journals. It appears that eight potential reviewers were asked, but declined. Dr Rossetti eventually found four reviewers.

### **The review process and the reviews received**

In view of the conflicting verdicts of the first two reviewers (Professors Aplin and Haszeldine), Dr Rossetti solicited two more reviews. These were anonymous. I do not need to comment on Professor Haszeldine's report, which was both constructive and positive.

Professor Aplin's report raised the question of why I had not published the evidence for the lack of through-going faults in the US shale basins. I answered this in my response to Dr Verdon (AC7). He confused my discussion of the “*problem of pre-existing faults*” of section 1.1 with fracking-induced fractures – a different topic. I have already commented (AC10) on his misunderstanding of the evidence for the confined aquifer below the western Fylde being fresh or saline, and why it is significant. He did not read my revision (AC3) of the evidence for drinking water well contamination in Bradford County.

The third, anonymous, referee (RC3) concentrated on the discussion of regulation, and why he/she thought that this has no place in a science journal. That may be a valid point of view, but one with which I disagree.

I regret to say that the fourth referee, also anonymous, provided a short report (330 words) which is practically incomprehensible, even when one attempts to re-interpret the very poor English. But the gist of the review seems to be that a whole book would be needed to cover properly the topics I have tried to cover initially in 17,000 words and ten figures.

In conclusion, the three negative reports, taken together, are hardly constructive or helpful.

### **Other comments received**

Comments can be constructive or destructive, but in either case can often be useful for indirectly highlighting topics or conclusions that the commentator does not discuss. Such omissions would tend to imply tacit agreement, unless the author has explicitly stated that he/she would not focus on certain topics.

In general, I conclude from the comments that the following conclusions, assertions, or statements have passed unchallenged:

- The lack of through-going faults in the US shale basins (section 1.1, but with the proviso that the evidence needs to be published).
- The orders of magnitude geometric differences in the US vs. the UK shale basins.
- Flaws in the Halliburton frack upward growth study.
- The UK history of long-reach conventional wells and of fracked wells.
- Geology and exploration history of the Fylde.
- Various details concerning errors or omissions by the operators.
- The Paddockhurst Park Fault cutting the Balcombe-1 well.

- Conclusions on better regulation and improved geophysical methods.

The following conclusions, assertions, or statements were challenged:

1. Re-interpretation of the triggered fault at Preese Hall-1 (sections 3.5, 3.6).
2. Hydrogeology in general, and of the Fylde in particular.
3. Recognition of a deeper fault in Balcombe-2z.
4. Parts of my fault modelling study review.
5. Bradford County case history of Llewellyn et al. 2015.
6. Image manipulation or alteration.
7. UK regulation (section 6.2).

I omit from this list some general comments, such as claims that the paper is unscientific, or that the title is misleading, and I also omit minor points of criticism. The points of contention listed above were challenged as follows.

Dr Westaway took issue with me, primarily about the significance of the fracking-induced seismicity at Preese Hall-1, Lancashire. He quoted from his paper, newly published in January 2016, which I showed was immediately obsolete (AC1) because it failed to take into account the revised stratigraphy of the well, released by DECC in April 2015. He had failed to obtain the released data himself, whereas I had used them. He has produced, over the last year or two, various interpretations of the fault that slipped. He seems to disagree with my geometrical analysis suggesting that the well was bored right through the fault; but then, latterly, claims that he thought of it first.

Dr Westaway quoted a defamatory article from the UK tabloid newspaper *The Daily Mail*, masquerading as a scientific scitation ('Seamark 2014'). The editor asked him to remove the offending reference, which was a web link, in which his colleague Professor Younger had defamed me. He did so in part, but later on re-inserted the offending pseudo-citation. He then went on to criticize the journal itself for having published my discussion paper in the first place. This earned him a public rebuke (SC19) from Professor Fabrizio Storti, the Editor in Chief.

Dr Verdon discussed (*inter alia*) the problem of the location of the earthquake-triggering fault at Preese Hall-1. He also made several *ad hominem* comments, and challenged my independence on the ground that I am funded by objectors' groups in England. My response to the latter is that I shall declare my earnings from these sources, which average out over the last three or four years at well under £1 per hour.

Mr Clarke of Cuadrilla Resources, the operator in Lancashire and at Balcombe (Weald) tried to defend his company's interpretation of the position of the earthquake fault at Preese Hall-1, in which

the fault location avoid passing through the well, as I propose. He also challenged my interpretation of the possible fault cutting the Balcombe-2z horizontal wellbore. He claims that my supposed fault is merely an artefact of drilling through a cement casing shoe. I had written:

*"It is possible, but unlikely, to explain the repetition by assuming that two separate logging runs were made and then poorly spliced together; but an alternative and more plausible explanation is that the wellbore went through a normal fault with a downthrow to the east (well-head side)."*

I accepted Mr Clarke's explanation in part, that the drilling out of the shoe might go some way towards explaining the apparent fault, but that the data still suggest a poor splicing of two drilling runs. I invited Mr Clarke to supply some more data to resolve the point, but he has declined to do so.

Professor Younger challenged me on hydrogeological matters. But he commits no less than four fundamental errors (for details see my responses to his comments SC8 and SC20):

- The belief that fluids migrate downwards in a fracked and faulted shale setting, in the face of six independent quantitative modelling studies which suggest the opposite,
- Failure to understand that the subsidiary Lez aquifer system at 1200-3000 m depth in SE France has nothing to do with the primary shallower limestone-hosted aquifer system, but demonstrates that deep water flows upwards along faults cutting the shales which were a fracking target.
- His generalised assertion that the confined aquifer below the western Fylde can only be saline, which I showed was based on false analogies with similar UK settings.
- The quotation of irrelevant previous work on the Fylde aquifer, presumably with the aim of browbeating the editors or other non-specialist readers that he has a superior grasp of the problem than I.

Several comments were made about my mini-review of prior work on modelling fluid flow up faults in a shale setting. I omitted to give proper attention to the recent Birdsell et al. (2015) paper, and shall give it due credit when I next have reason to review the literature.

Several commentators objected to my interpretation of the important Llewellyn et al. (2015) case study in Bradford County, Pennsylvania, saying that I had misconstrued the authors' own conclusions. Indeed I had done so, believing that if well casing had not lost integrity then the methane observed to contaminate the drinking water wells had to have emerged either at the far end of the casing (at the fracked shale) or else at the wellhead. However, I pursued this problem further,

including obtaining the detailed well plans in the area of interest. My new interpretation (AC3), which is in effect a paper in its own right, suggests that the methane did indeed come from fracked Marcellus shale, passing up the fault zone identified by Llewellyn et al.

Mr Andrew Kingdon, petrophysicist at the British Geological Survey, questioned my use of certain images. He also implied that I had withheld some data from an imaging tool used at Preese Hall-1. In fact I did not have access to these data. I refuted his allegation that my interpretation of the published seismic image through the well was ambiguous. He asserted that I had exaggerated the magnitude of fault throw in my discovery of the fault cutting the higher section of Balcombe-1. Again, I showed that his allegation was false, by providing a new, more detailed image. The fault throw is indeed 10 m as stated in my manuscript, and not the 6 m claimed by Mr Kingdon. He has not withdrawn his allegation.

Professor Aplin dismisses my recognition of the faulting at Balcombe-1 that: *“even faults with significant throws may not be visible on old, low quality 2D seismic lines, but that they can be interpreted from detailed log and stratigraphic data. Sound - but hardly novel.”* Here he fails to observe two important points; (1) My precise correlation of shales on gamma ray and sonic logs between two wells nearly 15 km apart, and correlating wiggle-for-wiggle down to sub one metre resolution, has probably never been achieved before; and (2) the operator Cuadrilla failed to observe this fault, having not bothered to consult the published geology maps. Even though Professor Aplin is an expert in aspects of shales, his experience of well log interpretation and tie-in to seismic reflection data appears to be rather limited. His review suggests a reluctance on his part to criticise sub-standard technical work by UK operators such as Cuadrilla, declaring, instead, that my work is an *“invective-strewn commentary”*.

Some commentators, and referee no. 3, thought that my criticisms of the failures of UK regulation were inappropriate, either because this sort of comment has no place in a scientific article, or because I was casting aspersions on the competence of scientists at the Environment Agency.

## **Discussion**

In view of the three negative reviews out of the four received, Dr Rossetti had little option but to reject the manuscript *“since too much work is needed to render it potentially suitable for final publication on SE”*. I concur with his view.

Even if I had been offered the opportunity to revise the paper, it would have turned out to be far too long once I had added all the necessary amendments and additional discussion. In retrospect I can see that my manuscript was over-ambitious in trying to cover five separate topics. I propose to deal

with these topics as follows, taking into account the valid review comments:

(1) *Faulting in the US shale basins*: I have explained already (reply to Dr Verdon, AC7) why it is almost impossible to publish a full accurate study in a peer-reviewed journal. I propose to put my findings into a web article, properly researched and with all the sources cited, of course. This will avoid the impracticable task of seeking permission to reproduce dozens of maps and cross-sections. Furthermore, the web page can be updated or corrected as required.

(2) *Faulting in the Bowland Basin and the Weald, England*: I shall resubmit this part of my manuscript as a new paper to SED, on the ambiguity of fault interpretation. It will cite the fault modelling studies of shale basins, while not attempting to review them in any depth.

(3) *Summary review of fault modelling studies in shale basins*: I shall leave this, updated to include the Birdsell et al. (2015) study, as a web page. A very brief version will be included in (3) above.

(4) *Bradford County case history*: My second reply (AC3) to Dr Engelder was proposed as a supplement to a revised paper for SE. However, it can now form the basis of a new stand-alone paper to be published elsewhere.

(5) *Regulation of unconventional development in the UK*: This review and discussion is probably best separated out, for submission to an environmental health journal.

### **Unintended consequence of publishing the discussion paper**

I hold a lifelong Honorary Senior Research Fellowship at the University of Glasgow, which entitles me to online access *via* the to the journal and research database. About two days after I published the SED paper online, on 27 January 2016, my habitual online access, uninterrupted since my retirement in 1998, was terminated without warning or explanation. At the time of writing I can only conclude, in the absence of any credible explanation from the University Court (the governing body of the university), that my views on fracking, and in particular the publication of the present manuscript, are the reason for the termination. The Secretary of the Court had previously written to me in 2014, expressing concern that “*the views which you have expressed, particularly on the subject of shale gas, are not consistent with work which is currently being undertaken at the University.*” I am currently engaged in discussions with the University Court, and have instructed lawyers to act for me. This action by the university is clearly aimed at silencing me, for without such access I cannot properly continue my research in *any* field, not just that of unconventional oil and gas development. Professor Paul Younger, one of my critics, is a member of the Court.

Professor Younger was quoted in *The Times* (31 October 2014) as saying, *à propos* of the cancelled disinvestment in fossil fuel shares held by the University of Glasgow, that “*the new statement would*

*help to repair relationships with oil and gas companies that funded research.”* So my *alma mater* and former employer seems to be more concerned about maintaining good relations with the oil and gas companies that fund its research than with permitting free and open debate. I find this attitude disappointing and regrettable.

## **Conclusions**

I withdraw the paper with immediate effect. I thank Dr Rossetti, the topical editor, for his hard work and forbearance over the last four months. The discussion paper, including all the accompanying comments and replies, remains available online in perpetuity.

I believe that the review methods used in SED and SE point the way to good peer-reviewed scientific publication; that is, openly attributed discussion of a manuscript published online. The next development should be to require, in addition, that all formal reviews be signed and not anonymous, but as Dr Rossetti has pointed out to me, it is already difficult to find reviewers. I look forward to submitting to SED a new slimmed-down and better focussed paper on the ambiguities of faulting in UK shale basins.