Reply to anonymous referee RC3

David Smythe

Here is my response to the detailed comments provided by referee no. 3 on 10 May 2016.

The referee writes:

> *"the paper isn't about what the title says it's about: 'Hydraulic fracturing in thick shale basins: problems in identifying faults in the Bowland and Weald Basins, UK.'"*

I disagree; pages 5 to 23 are concerned in detail with the problems of identifying faults in the two basins. But the referee presumably skipped over this part – the heart of the paper – because, as this referee admits, he/she is not an expert in seismic interpretation. The heart of the paper is wrapped up in the context in which the faults are studied, that is, 'hydraulic fracturing in thick shale basins', which is the first part of my title. Therefore, in my view, the title correctly reflects the content.

The referee states, followed by ten quotations taken from my paper:

> *"Much of the paper is about regulation and other aspects that are irrelevant. The paper contains many unsubstantiated assertions and irrelevant statements - and comments completely inconsistent with the style of an academic paper."*

It is a moot point as to whether discussion of the (perceived) failures of regulation and/or distortion of technical evidence by exploration companies should find a place in a scientific journal like SED or SE. Does this mean that such shortcomings should never be admitted or discussed? Should such a discussion be left to sociologists? Should they be left to the arena of planning inquiries and the courtroom? How could highly technical criticisms be satisfactorily dealt with in, say a newspaper or magazine article? My view is that we earth scientists have a duty to point out these failures; if not, the exploration companies may obtain licences and planning permissions based upon inadequate or even false data.

To take as an example the Celtique Energie case histories I quoted (section 4.3); I provided numerous instances where the company's two planning applications in West Sussex were misleading both the county council and the general public. Is such criticism, highly technical in places, to be omitted? For example, I stated how and why the faults in the seismic section published by Celtique appeared to have been removed by reprocessing. I included it as an example of the 'problems in identifying faults', which is part of the title of my paper. If the referee can suggest a more appropriate peer-reviewed scientific forum for the debating of such points I shall be pleased to take

his/her advice.

The referee mentions my discussion of Llewellyn et al. (2015), agreeing that it is an appropriate topic for review. But he/she has not read my long reply (AC3) to Dr Engelder (SC4) in which I have taken the interpretation much further with, *inter alia*, the use of new data on the location of the offending horizontal well. My new interpretation is, in effect, a paper in its own right, and I shall be submitting this discussion and reinterpretation for publication elsewhere.

Lastly, the referee accuses me of failing to describe the geometry of the US shale basins, and the absence of through-going normal faults therein. I have thought long and hard about how and whether to publish this information in orthodox peer-reviewed form. The referee concludes:

> *"It may be true, but has the author really surveyed the huge US basins enough to establish such a point? Where is all the evidence?"*

Yes, I have indeed surveyed (by desk study) the US basins. I have provided a fuller explanation in my response (AC7) to Dr Verdon's comment (SC7). Here is a summary. I spent the equivalent of two months' full-time research on this topic, reaching the conclusion that such faults do not occur in the areas of the shale basins that are being exploited. But most of the relevant information is to be found online, in informal sources like company reports. In practice it would be impossible to seek out all these sources to ask permission to reproduce their maps, cross-sections and seismic reflection examples. I now propose to make my findings public in an informal way, such that the data may be periodically updated as new information emerges. The data sources will be acknowledged, and if any third party objects to the acknowledged re-use of their images then the offending examples can be removed.

I encapsulated my US study, which comprises an essentially negative result (am I expected to publish blank maps showing an absence of outcropping faults?), in the phrase 'foreland basins', which I trust this referee understands. Built in to this observation is the style of faulting, which contrasts fundamentally with that in extensional basins.

In conclusion, this referee appears to prefer an uncontroversial, purist approach to academic publishing. My contention, in contrast, is that we earth scientists – at any rate in basin researches – cannot and should not ignore the real world of grey literature, exploration licences, commercial and/or confidential data, and so on.