Conjecture and refutation; author's response to Dr Engelder

Introduction

I thank Dr Engelder for commenting on my discussion paper, not least for what he has not written than for what he has written. He is an expert of long standing on the geology of Appalachian Plateau, and, no doubt, of many other regions of the USA. So I am pleased that he has not sought to question my summary synthesis of the structural differences between the US and the UK shale basins (Section 1 and Figure 1), and my conclusion that through-going faults are essentially absent in the former.

Philosophy, advocacy and agendas

Dr Engelder starts his critique by misapplying Gödel's incompleteness theorem to science in general. These are two formal theorems (not one, as Engelder quotes) in mathematical logic, with application to mathematical philosophy. Their influence may extend from arithmetic into computing science, but they do not have any relevance to the epistemology of the physical sciences, a field which I have had a fifty-year interest (I have just counted six of Karl Popper's books on my bookshelves behind where I am writing this, among the two dozen or more books I possess - and have read! - on the philosophy of science). Karl Popper did the fundamental work on conjecture, refutation, and what is meant by a testable (i.e. falsifiable) scientific hypothesis. His classic is *The Logic of Scientific Discovery* (published in German in 1939; English translation publ. 1959). I fear that Dr Engelder has confused Gödel with Popper, and so it might be wiser if he could avoid any future philosophical commentaries until such time as he has studied the field in more detail.

I am less happy that he suspects me to be an 'agenda-driven' and 'advocacy-based' scientist. Here Dr Engelder has been quoted as saying that he would "*really like to occupy the middle ground in the industry v. anti-driller scrum*", seeing himself as being above the world of agendas or of advocacy. But presumably he is, like me, an 'advocate' of sound, evidence-based science. So I am not sure that the epithet carries any useful meaning. Nor do I think that sitting on the fence, as he claims to do, is necessarily justified. Surely decisions, and therefore sides, have to be taken eventually. I note here that he has boasted of bringing in "*at least $6 million in grants from industry and $8 million from government*" (http://www.post-gazette.com/business/businessnews/2011/03/20/The-Marcellus-Boom-Origins-the-story-of-a-professor-a-gas-driller-and-Wall-Street/stories/201103200259). These are impressive figures. He is further quoted in the same piece as saying "*There is a symbiosis between academic research, and by that I mean big-time research of the type that Penn State* [his university] *does, and industry. Industry really does benefit from this. There is a reason that industry contributes very handsomely to the academic world*".

Dr Engelder has thus been so embedded in his symbiotic link to industry for the last thirty years that I think he fails to see that this must colour his thinking; and for him to claim, notwithstanding his professional dependence on industry, that he is on the fence on the subject of fracking is quite unjustified. Therefore I could equally well call him 'agenda-driven', and an 'advocate' of fracking, but such accusations or insinuations, as he has made about me, but in the opposite direction, are unhelpful and counter-productive.

I became involved in the fracking debate because I perceived that only one side of the science was being presented, and that side was pro-industry and in favour of fracking. Ill-informed science, which Dr Engelder rightly scorns, includes the report by the academic expert group convened under the auspices of the Royal Society and Royal Society of Engineering of 2012. I have explained in detail in my paper why this report falls short. I also agree with Dr Engelder that the anti-fracking camp has promoted many misconceptions and half-baked quasi-scientific notions, but I do not fall into that camp. Indeed, I consulted for the oil and gas industry intermittently between 2002 and 2011, and have no technical objections to conventional exploration. Furthermore, I have been approached to help oppose drilling applications in the UK, but I have declined these requests, because they concerned orthodox, conventional onshore exploration projects which are very unlikely to cause environmental harm. Lastly, on the question of earthquake triggering by the fracking process itself, I have always maintained, in talks for the general public as well as in writing, that this is a side issue of little impact. Triggering by disposal of waste water by injection is, of course, another matter.

To conclude this section of the discussion, if anyone can claim to be unbiased (but well-informed) in the scientific debate about fracking it is I. To have reached an evidence-based conclusion, as I have done, about the environmental risks of faults in the fracking context is not evidence of bias. Dr Engelder can hardly claim impartiality, or lack of bias, himself; his article in the geological industry literature entitled *Truth and Lies about Hydraulic Fracturing* (Engelder 2014) implies, to me, a degree of partisanship. In contrast to Dr Engelder, I have neither monetary nor reputational advantage to be gained; nor do I have an egotistical need to give public lectures or to be cross-examined in legal enquiries, as I have done; in fact, I rather wish that I could return to other more productive areas of earth science research.

The evidence for contamination of water wells in Bradford County, PA

Dr Engelder claims that I have completely misconstrued the results of Llewellyn et al. by summarising the study thus: " [It] *proves beyond reasonable doubt that contamination of drinking water was caused by passage of frack fluid and/or produced water* ***in part through the geology***". [my emphasis]. I fail to see how the latter part of this statement is inconsistent with the authors' summary, as follows:

"*The most likely explanation of the incident is that stray natural gas and drilling or HF compounds were* ***driven ?1–3 km along shallow to intermediate depth fractures*** *to the aquifer used as a potable water source*." [my emphasis].

Llewellyn et al. conclude:

"*The data released here do not implicate upward flowing fluids along fractures from the target shale as the source of contaminants but rather implicate fluids flowing vertically along gas well boreholes and* ***through intersecting shallow to intermediate flow paths via bedrock fractures****. Flow along such pathways is likely when fluids are driven by high annular gas pressure or possibly by high pressures during HVHF injection.*" [my emphasis].

For the lay person, co-author Susan Brantley has explained the geological link thus:

"The most reasonable explanation of our findings indicated that ***a highly diluted chemical mixture used in shale gas wells traveled more than 2 kilometers across natural fractures in the Earth's rocky subsurface*** *and entered drinking water wells."* [my emphasis].

So Dr Brantley's quotation of 'the rocky subsurface' as being the "*most reasonable explanation*" of the pathway clearly excludes surface flow or unconsolidated sediments.

My original Figure 10 was prepared from an anamorphic version of the cross-section in Llewellyn et al.'s figure S9, squashed horizontally and simplified to show one of the Welles series of wells, not five, and with a schematic vertical fracture zone added. It has a clear error in showing the well cutting the Marcellus shale vertically, and not landing horizontally in the shale, because the original diagram on which it is based has the same error. I had also included arrows indicating direction of flow of methane and contaminated water, but removed these before submission. They showed vertical flow up the wellbore, then transmission up-dip and to the south along bedding, and then vertical transmission up the schematic fracture. But I had not appreciated that Llewellyn et al. had ruled out vertical transmission from a source as deep as the Marcellus. So apart from implying that the fugitive gas and contaminated water came from the

Marcellus shale, **my diagram and text is basically correct in summarising Llewellyn et al.'s view that the pathway was through the shallow and intermediate depth geology.**

I withdraw my claim that Birdsell et al.'s paraphrase of Llewellyn et al.'s conclusions was wrong and misleading, and will instead add a brief summary of the importance of this new paper to my modelling review section, as well as adding a new box to my organogram. Birdsell et al. themselves review the previous modelling studies of fluid flow up faults, but it is a pity that they did not include Cai and Ofterdinger (2014) in their discussion.

My partial misunderstanding of Llewellyn et al. arose from my assumption that the fracking chemical 2-BE must have come from depth. The importance of Llewellyn et al.'s paper is that it demonstrated for the first time the passage of fluids and fugitive gas through the geology – albeit at shallow to intermediate depths (the uppermost 500 m) – and not just up faulty wellbores, as is now well-documented.

I am providing separately a detailed comment which re-interprets somewhat the conclusions of Llewellyn et al. My re-analysis shows that the preferred pathway of Llewellyn et al. - travel from wellbores at shallow to intermediate depths up geological fractures – is only part of the story. The notion of surface spills as being implicated in the homeowner well contamination is also discounted. My re-interpretation uses new data, including the detailed horizontal well plans and production history for the W1 and W3 pads which were not available to Llewellyn et al. Dr Engelder will be welcome to comment on this. I want my final paper to have resolved as many problems and arguments as possible, and I don't care how many rounds of comment and reply it takes.

I propose that my sections 5.3 on this subject be completely revised, and that a Supplement be added, since my re-interpretation requires several more figures. My original Figure 10 will be scrapped and replaced by a more detailed cross-section and map. I have no agenda here; my only interest is to establish something as near to the truth as we can get, based on imperfect and incomplete data.

Critiques of Myers 2012 and other modelling papers

Dr Engelder devotes much attention to the controversial Myers (2012) hydrogeological modelling paper, castigating me for having given it attention. He suggests that the best thing for a flawed paper, such as this one, is to ignore it. On the contrary, I discussed it, by no means uncritically (some 250 words on page 25),and by citing the other critics, because it may indeed have an application in a generic sense for the case of vertical or near-vertical faults. It may therefore have some validity in the UK basins. Furthermore, it may well apply to the vertical fractures postulated by Llewellyn et al., as I show in my detailed comment mentioned above. If I am guilty of not ignoring the supposedly flawed Myers paper, then so are Vidic et al. (2013) and Birdsell et al. (2014), who gave it serious attention. The lesson is that we can learn even from flawed papers.

Dr Engelder criticises my organogram (Figure 9) showing the sequential development of papers related to fluid flow up faults. I should perhaps have explicitly stated in the caption that it was *in the context of fracking*, as I made clear in my text (p23 lines 14-15). We all know, as Dr Engelder states, that "*there are a basketful of geologists and geophysicists who have contributed to the peer-reviewed literature about leaking faults long before Northrup's web posting in 2010*". Indeed, there is, in addition, a whole oil service industry devoted to differentiating between faults as seals or as leaky pathways. But if he can cite any such papers referring to leaking faults *in the context of high-volume shale fracking*, then I would be the first to add them to the organogram. If they do exist, it is surprising that they have not been cited by the early papers and reports depicted on the diagram. I also used colour to highlight the papers that reported the results of quantitative modelling of flow up faults from shale. Again, I would be pleased to include any such studies that I may have missed, if Dr Engelder would be good enough to provide the references.

My organogram differentiates between peer-reviewed papers and non-peer reviewed literature. Dr Engelder refers to one study as being from a group "*with a known agenda*". Why does he exclude the industry-produced reports (shown in yellow) from this criticism? In my view these reports might be regarded with equal suspicion, their "*agenda*" being to promote their own possibly biased point of view and thereby profit financially. But my Section 5.2 on the fault modelling studies restricted itself to being an impartial review of work to date, whether peer-reviewed or not, and from whatever source. Dr Engelder seems to be unable to accept this as being a sensible way to review the literature.

Dr Engelder, included among his twelve self-citations, quotes his own work on imbibition. These include his comment on Warner et al. (2012a), but he omits to mention either the original paper or the reply to his comment by Warner et al. (2012b). But here is not the place to develop a discussion of the relative importance of imbibition and/or well suction for reducing the flow up faults or fractures. Birdsell et al.'s results show that well suction is the dominant process in reducing the mass flow to the aquifer, and that imbibition is relatively marginal. The significance of Birdsell et al. is that they show, using realistic generic conditions, that mass flow reaches an aquifer quickly, but in very small quantities. One somewhat unrealistic value for an important parameter chosen by Birdsell et al. is their assumed 20-year timespan for production. It would be interesting if a more realistic value of around 8 years were used (http://www.marcellus-shale.us/Marcellus-production.htm), and to model what happens once production ceases, either temporarily, perhaps due to an interruption in the supply chain, or permanently once production is deemed to be uneconomic.

2-BE (2-n-Butoxyethanol) as an indicator of frack fluid

Dr Engelder almost had me fooled for a moment with his homely discussion, starting with drilling his own water well, segueing into air (percussion) drilling for the 13 inch surface casing of a typical gas well, all with the aim of insinuating that AirFoam (which contains 2-BE) *might* have been used in the drilling of the Welles series of wells. Never mind that his volume calculation is way out – the actual volume of soil and bedrock that are "*disturbed*' is about 30 m^3 (I think he forgot the π factor). For earthworks, this is not exactly a large figure; for example, it's about one-fifth of the water volume of my domestic swimming pool. He then goes on to review the apparently low toxicity of the chemical – an irrelevant diversion, because its identification by Llewellyn et al. was in pursuit of sourcing the household well contamination, and had no bearing on the toxicity or otherwise of the drinking water. He considers "*the possibility that the source of the very low amounts of 2-BE in local groundwater are local septic fields into which household products with 2-BE may have been flushed for years and years.*" This attempt at diverting attention from the Paradise Road homeowner well contamination omits mention either of the timing of the whole episode in relation to Chesapeake's gas drilling activities, or of the uncontaminated homeowner wells B1-3 in the locality.

No, the fact is that 2-BE was a documented component of the frack fluid in the Welles 2 to 5 pads. We do not know whether it was also used at Welles 1, but "*it is reasonable that the same nonemulsifier agent (which contained 2-BE) was likely used*" (Llewellyn et al. 2015). The composition of the frack fluid used for W 1-3H and W 1-5H has not been published on www.fracfocus.org, the voluntary industry website. Without sounding too paranoic or suspicious, it is reasonable of me to ask why the data for the Welles 1 pad has not been disclosed.

For Dr Engelder to imply that the source of the 2-BE is the vertical well air drilling, or homeowner septic fields, and not the frack fluid, is a classic example of disinformation that he ascribes to others. Septic systems were discounted by LEA in a supplementary 'Frequently Asked Questions' paper (Llewellyn et al. 2015). Lastly, even if it were true that the air drilling used AirFoam, *in addition to* 2-BE being likely present in the frack fluid, I have shown in my re-interpretation of the case history that the explanations of (i) surface spills, leaks or vertical air drilling as a source for the 2-BE, and (ii) shallow to intermediate wellbore leaks as the source for the fugitive methane, are both unlikely.

Additional references

Engelder, T. 2014. Truth and lies about hydraulic fracturing. *AAPG Explorer*. Available from: http://www.aapg.org/publications/news/explorer/details/articleid/12416/truth-and-lies-about-hydraulic-fracturing.

Llewellyn et al. 2015. Frequently asked questions about the study "Evaluating a groundwater supply contamination incident attributed to Marcellus Shale gas development" by Llewellyn et al. http://www.appalachiaconsulting.com/home/whats_new/pnasarticlefaqs

Vidic, R.D., S. L. Brantley, J. M. Vandenbossche, D. Yoxtheimer, and J. D. Abad. 2013. Impact of shale gas development on regional water quality. *Science* 340, 1235009 (2013). DOI: 10.1126/science.1235009.

Warner N.R, et al. 2012a. Geochemical evidence for possible natural migration of Marcellus Formation brine to shallow aquifers in Pennsylvania. *Proc Natl Acad Sci USA*

109(30):11961–11966.

Warner N.R, et al. 2012b. Reply to Engelder: Potential for fluid migration from the Marcellus Formation remains possible. *Proc Natl Acad Sci USA* Early Edition www.pnas.org/cgi/doi/10.1073/pnas.1217974110 PNAS.