

Reviewer Comment 3

Dear Reviewer 3,

Thank you for your constructive and supportive review. We are glad that you enjoyed reading the manuscript and that you like the research despite the issues that you raise.

We respond to your general and specific comments in the attached document.

Best wishes,

Jennifer Roberts (Corresponding Author)

In the table below, our response (in blue text) can be found beneath each of your general and specific comments (in black text) which we have numbered for ease of any further discussion.

	General comments
1	<p>The paper posits that a shared language about seismicity would facilitate risk communication. In so doing, it recasts the venerable “knowledge deficit” model of science communication into a concern about how the absence of a shared language can make science communication difficult. This despite the fact that the authors cite a paper about why the model persists and how to overcome it (Simis et al. 2016). Developing a shared language is not a bad aim in itself and I agree that their point about the messiness of language, but I think it is unlikely to yield the results that the authors desire. While I agree that consistent use of terminology is beneficial between peers, the feeling I take away from the paper is that the authors do not consider the public to be peers. And they are not, in the professional sense; but members of the public are peers in the stakeholder sense.</p> <p>The initial prompt for this research was the tendency for the narrative around shale gas to dismiss or simplify public concerns around negative impacts, including induced seismicity, or to talk about the publics as a homogenous body. We were motivated to find out how shale gas experts answered the same questions being asked of the public, to test if expert views can be simplified much like the publics. What we found was that the questions about seismicity be answered differently by different people depending on what the word “earthquake” means to them.</p> <p>We were therefore disappointed to read that you feel that the paper appears to recast the information deficit model, and does not cast the publics as peers. We had taken particular care around this framing and the language we used, such as noting the expertise that publics bring (Lines 217/8), referring to differences between expert and lay perspectives as ‘apparent’ (Line 66), criticising ‘technocracy’ (Lines 106 – 115), and making clear that language challenges cause problems amongst experts, too (Lines 119-120).</p> <p>Importantly, we feel, is the emphasis in our research that the shared language is not to ‘benefit’ publics by improving their understanding (i.e. filling their ‘knowledge deficit’). Rather, a common language framework is needed to a) help all stakeholders to communicate with each other and b) for perceived risks of a range of stakeholders to be better captured or understood. i.e. developing a shared language framework is not to facilitate one-way (expert to public) risk communication, but to support multi-way communication and understanding amongst all stakeholders, of which the publics are one/several.</p> <p>From your comments we deduce that resolving this broad issue is a case of revisiting the text with ‘fresh eyes’, adding qualifiers, and addressing the points raised in the specific comments. Rereading the introduction, we can see cause for your concerns. For example, the premise that we set in Line 64 is too simplistic, and in the text place more emphasis on the challenges in perception of the</p>

	<p>subsurface concepts, and do not present much detail on factors influencing risk perception or, say, values. We will make sure to include this, as well as to check that reference to stakeholders includes the publics.</p>
2	<p>Questions of who would develop the shared language, define the terms, etc. loom large in the paper. I get the sense, based on comments about the “nuanced” understanding of experts compared to the public throughout the paper, that this would be a top-down exercise. This would replicate the knowledge deficit model in linguistic form. To be fair to the authors, they did not specify who should develop the language. I am reading between the lines on this point. The paper would be stronger, and my concerns allayed somewhat, if they outlined a procedure for how developing a shared languages should or could happened.</p> <p>In the paper, we do not propose how a language framework should be formed; that is not within the scope of our work. We feel this would be really interesting follow on research (see also our Response to Short Comment by Dr James Verdon).</p> <p>In fact, we are not sure that a blanket language framework would be appropriate; it might be that a shared framework is ‘drawn up’ amongst stakeholders on a site by site or regional basis.</p> <p>Either way, we agree that a top down approach would not be appropriate – any framework developed by a top down approach would not be ‘shared’. Arguably there are several top down frameworks or classifications already in circulation (line 166, line 694) but – as we find - these are not widely used. While we are cautious to propose an approach in our manuscript, we will draw on these points to add a sentence or two on this in the Discussion.</p>
3	<p>Regardless, the emphasis on developing a shared language ignores how political (and industrial) affiliations and values influence perceptions of risk and the assessment of scientific information. Indeed, the authors bemoan the fact that language is “susceptible to emotional loading and misinterpretation” (Lines 30-31). Unfortunately, the public, and experts, always interpret information through a field of values and personal consequences. There is a broad literature in this area of science communication. Dietz, McCright, and Dunlap are some names that spring to mind, but there are many other sources.</p> <p>We are aware of research around politics, motivation, and risk perception. The YouGov surveys which we compare our data against find an association between responses and political affiliation (alongside demographic factors and so on). We notice that we do not specifically refer to, say, values in our text on factors that affect how individuals perceive information (Lines 121 – 126) and framing around shale gas more specifically (Lines 127 - 134). We agree that this is an oversight and should be included in the Introduction and also the Discussion. Again, we’d like to emphasise that a shared language wouldn’t resolve these challenges, or align different frames, but would support or facilitate multi-way communication amongst stakeholders. With our ‘fresh eyes’ we will make sure that this is articulated as such in the revised manuscript.</p>
4	<p>I am curious if the authors considered how politics and personal interests shaped re-sponses to their surveys. I have witnessed industry scientists and industry-friendly government officials argue all the nuances of data in a bid to halt pending regulations, whereas people with different interests and values (non-industry affiliated academics and the public) argued for restrictions. This is common in US climate change and energy politics.</p> <p>This would be interesting research; in particular how these interests shape the language chosen to justify their response. However this is beyond the scope of our paper and our research data.</p>
5	<p>Politics seems an unavoidable factor in this type of research. Language is a not a neutral tool, but one that is used to achieve certain ends. I fear that faith in the rationality of language, and those who would use it, is misguided.</p>

	<p>This is an important message. We were careful to articulate that clarifying language would not, in itself, resolve communication challenges that we highlight in the paper, and we do not posit that a shared language would, for example, reduce perceived risks. Rather, a shared language would be one step to facilitate risk communication and, in doing so, help to clarify our understanding of how the risks of induced seismicity are assessed, perceived and understood. Your comments suggest that you feel this needs greater emphasis, and, similar to Comment #3 response, we will go through the text to make sure this is articulated as such in the revised manuscript.</p>
	<p>Specific Comments</p>
1	<p>Lines 21-26 – Tom Dietz (and others) have discussed that information is understood through a filter of values. This section, and the paper, would be strengthened by considering that the public (indeed, the many publics) hold values that are different from industry scientists and thus interpret information about fracking and related issues differently.</p> <p>We agree, this is important, and agree that it needs expanding on in the Introduction and reflect on in the Discussion (see response to General Comment #3).</p>
2	<p>Comparison of closed ended surveys and qualitative data. I find this section problematic in a few ways. The authors cast doubt on survey data by expressing concern about how the surveys were constructed and analyzed. However, they do not provide any evidence from survey methodology literature to support their claims. Otherwise, statements such as the following from lines 296-304 are unsupported: “results of these closed surveys should therefore be interpreted and compared with some caution.”</p> <p>While closed surveys must always be treated with care (and we will bring in methodology literature as you suggest), we present a clear case in the manuscript for why the results of closed surveys that use ambiguous language might be treated with extra care. We do not mean to imply that the authors of these surveys are not careful in how they interpret the data, nor how they executed their study; as Reviewer 2 points out the word ‘earthquake’ might have been chosen because it’s a familiar, jargon-free, phrase (we will build this into the revised manuscript). However - as we show in our research - the term ‘earthquake’ means different things to different people, thus potentially muddying the understanding gained from any approach that uses such phrases without definition.</p>
3	<p>Providing support for this skepticism is particularly important since the authors uncritically accept the results from qualitative research (at least here) and suggest that it provides a more accurate portrayal of public opinion. To support this, a more robust comparison and discussion, rooted in literature, of these methods is needed. (For full disclosure, I am primarily a qualitative researcher, so I tend to favor qualitative methods and I appreciate the authors’ point that closed ended questions do not allow respondents to offer their full knowledge and experience about a subject.)</p> <p>You are correct that linking into research methods literature is called for in the discussion, and we will incorporate this into the revised manuscript. We are surprised that you feel we were uncritical towards qualitative research. In the manuscript, we certainly critique the reporting of qualitative research in terms of masking the phrasing used by participants to describe seismicity (although, to be fair, language wasn’t the focus of their studies). Either way, regardless of the research approach, we are cautioning against the use of ambiguous language and terminology. Regardless of the research method used, questions about earthquakes will be answered differently depending on what the word means to the participant.</p>
4	<p>There are other issues to address in this section as well. The authors compare the results of the surveys and the qualitative data, but these are apples and oranges measurements. They write on lines 330-332, “Deliberative and dialogic approaches find that concerns around the risk of induced seismicity are not as significant as the surveys suggest; while concerns around induced seismicity are raised, it is not a primary or dominant concern within the context of other perceived risks.”</p>

	<p>Regarding the first part of this statement, there is no way to compare the level of concern in the surveys with the level of concern in the qualitative data. Each method uses different measures and the authors offer no way to compare them systematically. This is a major problem.</p> <p>This is an important point, and we will remedy the language and the message here so that we are not, as you put it, comparing apples and oranges with regards to the relative levels of concern. However we do not feel that these limitations restrict us from being able to synthesise broad themes from these different approaches and studies.</p>
5	<p>The second part of the statement is also problematic in that, in at least one of the surveys I reviewed (Whitmarsh et al. 2015), there was no claim that induced seismicity is the public's major concern about fracking. Indeed, in the Whitmarsh et al. 2015 paper, respondents, as the authors mention (Line 289), found that on average, rated water contamination as more pressing concern than earthquakes (3.53 for water contamination versus 3.27 earthquakes on a 5-point scale, Table 2). However, this difference does not appear to be large and it would seem inaccurate to imply, as I feel that the authors have done here by not providing the measurements in the text, that the public is not nearly concerned about earthquakes as water contamination.</p> <p>You are correct that the reporting on the Whitmarsh et al., 2015 paper is slightly ambiguous and we will add in qualifiers about the relative scale of concern for earthquakes and water contamination. In the article, we do not claim that the surveys show that earthquakes <i>the</i> major concern, but "an important issue". It is difficult to say how important the issue is, when not all the issues of concern to publics are included in the survey. We will emphasise this in the revised manuscript, linking back to the methods literature.</p>
6	<p>I understand that the authors are trying to carve out a spot for their own mixed methods research with this review. However, I recommend that they revisit this section and recast their claims, using methods literature as support. This section, as currently written, gives the impression that the authors have a bias for qualitative methodologies and perhaps even for the outcomes they perceive in the cited studies. I want to be clear that I am not suggesting this is actually the case; rather, I wonder if it is an artifact of their analytic approach, which I do think could be improved. I did think that lines 395-407 gave a more nuanced discussion of the surveys compared to the qualitative data.</p> <p>We are not trying to carve out a spot for our mixed methods research. We are trying to establish - from the literature and through survey - the perceived risk of seismicity from hydraulic fracturing and how this varies between stakeholders. We find that our understanding of perceived risks gets muddled by ambiguity around the language commonly used to describe seismicity.</p> <p>We agree that a brief critique or overview of the strengths and weaknesses of qualitative approaches and closed question surveys would be appropriate to include in Section 2.1. We will also remove elements of analysis and discussion in Section 2.1 to make the approach clearer.</p>
7	<p>Line 399 – The authors write, "In contrast [compared to expert assessments], evidence on the perceived risk of induced seismicity amongst lay publics is mixed." I do not think this is true. Every piece of research the authors introduced notes that the public perceives risk related to fracking. Perhaps if the authors change the sentence to read something like, "Evidence on the amount (or level) of perceived risk: : :) But again, I don't see enough here to make comparisons of levels of risk perception between studies.</p> <p>This is a very fair point. All public perception studies report perceived risk of induced seismicity, and we will modify the text to reflect this. RE: comparing levels of perceived risks between studies, see response to Specific Comment #4</p>
8	<p>Line 476 – The authors write, "The public cohort were not intended to represent the perspectives of the general public." But then in Line 482, they compare the results of the survey with the Nottingham YouGov, which is meant representative of the general public. Although the authors say</p>

	<p>that the public respondents in their sample were meant to represent those who take their information from media sources, this comparison still seems inappropriate to make since the public they sample are self-selected to be at the conferences and meetings where they were encountered. They are more highly engaged on the topic.</p> <p>We make clear in the article that the ‘lay public’ in our sample are not representative of the general public (see Line 476). We compare all closed question responses (across all specialist conferences and public events) with the YouGov surveys to see whether and how participant views (our surveys) compare the general public (YouGov).</p>
9	<p>Line 513 – Could you say more about how experts’ views are polarized here?</p> <p>I suspect that the phrase “these experts” has led to ambiguity here. We will modify the sentence to clarify which experts we are referring to. In the preceding sentence, we detail how experts who obtain their information from research papers answer the closed question: 49% do; 47% do not (shown in Figure 3C). A very small proportion (4%) of this group are undecided. Thus, it might be perceived that these experts have split views. However, as we explore in the next subsection (3.2.2), the qualitative responses suggest that this apparent polarization is an artefact of language ambiguity. As such, we will add the qualifier that the experts’ views are <i>apparently</i> polarized.</p>
10	<p>Line 623-624 – This section where the authors report that some people thought their questions were “leading” or that the term earthquake was “way too strong” hint at boundary keeping and political motivations. It would be interesting who in the sample said these things.</p> <p>Yes, interesting! However it won’t be possible to reputably infer this from our data since we gathered no information about, for example, political motivation. Further, the question that they were asked is technically leading in how it was phrased and the issue of magnitude (and thus whether the word earthquake is appropriate or ‘way too strong’) is a technicality, too.</p>
11	<p>Line 648-651 – The authors write, “Nonetheless, our results do shed light on the ambiguity in the language around induced seismicity and the confusion that this can cause, the differences between publics and expert views on the matter (and difficulties in assessing expertise), and the limitations of using close surveys to elicit views on risk”. The authors mentioned a variety of terms that respondents in different sectors tended to favor. However, I did not see where they demonstrated actual confusion. (If this is in the paper, then I apologize, but I have missed it.) Some of this language, when taken in combination with criticisms about terms being too strong or questions having a leading quality, might suggest that some respondents are using minimizing language. How much of the choice in terminology is a struggle for accuracy and how much is a struggle to frame the issue in a particular light? The paper would benefit from considering such questions.</p> <p>It is not the survey respondents who are confused. The survey results can be essentially flawed due to language ambiguity, which could lead to confusion on the perceived risks around induced seismicity. Take the example of two experts who perceive similar levels of risk around induced seismicity, but giving two different responses: one essentially says that they “do associate shale gas with earthquakes, but any earthquakes will be microseismic and will not be felt”. The other says they “do not associate shale gas with earthquakes, any induced seismicity will be microseismic, and will not be felt”.</p> <p>The question ‘how much of the choice in terminology is a struggle for accuracy and how much is a struggle to frame the issue in a particular light?’ is important, and we agree that we should consider this in the paper. This links into political motivation and other such values previously raised. Using the example above, the two experts could have the same views about the risks of seismicity posed by hydraulic fracturing, but answer the yes/no question (do you associate shale gas with earthquakes?) differently because either (a) they have different preferred definitions for the term earthquake (b) they have different views about the shale gas industry, with one in favour and one opposed.</p>

12	<p>Line 665-666 – The authors write that there is no consensus amongst their survey respondents about whether or not earthquakes are associated with shale gas. It would be interesting to know who the authors define consensus.</p> <p>The use of the phrase ‘consensus’ was picked up by Reviewer 2 also. We will look to the peer reviewed literature for guidance on how ‘consensus’ and ‘general agreement’ are defined, and use an appropriately defined phrase in the manuscript. In any case, in the context of Line 165 any consensus would be ‘apparent’ given the issues we highlight in our paper.</p>
13	<p>Line 722-724 – The statement about doubt over public concern does not follow from experts’ nuanced understanding of risk. The authors should identify who used the surveys to imply that concern among the public is high. Who is making the claim? The researchers or other parties? “However, by examining the reasoning provided by participants to explain their responses, we find that in reality this is much more nuanced amongst experts, and thus public concern about risks of induced seismicity may not be as high as the results of previous surveys have been used to imply.”</p> <p>Firstly, our sentence shouldn’t state “amongst experts”; our surveys of (informed) publics also show nuance. This is our mistake. We will add references to the studies that conclude (from their research) that publics are concerned about risk of earthquakes. We were not referring to our study here.</p>
	<p>Technical comments</p>
1	<p>I cannot locate a Whitmarsh et al. 2014 citation in the references, probably a typo.</p> <p>Whitmarsh et al (2014) is a technical report. Our apologies for leaving out of the reference list. We will double check whether the 2014 report or associated 2015 publication is most appropriate.</p>
2	<p>Line 678 – typo here “event with a cause in media reporting of an event without any there being a scientific explanation for a”</p> <p>It’s not a typo, but a very confusing sentence. We will find a better phrasing.</p>
3	<p>Lines 683-684 – plural/singular “In particular, those who ‘do not’ associate earthquakes and shale gas question the 684 definition of an earthquakes.”</p> <p>Thanks. Have replaced with “definition of an earthquake”.</p>