Geosci. Commun. Discuss.,
https://doi.org/10.5194/gc-2020-33-RC3, 2020

Interactive comment on "Fracking bad language: Hydraulic fracturing and earthquake risks" *by* Jennifer J. Roberts et al.

Anonymous Referee #3

Received and published: 17 October 2020

General Comments This is a clean, well-written paper and was a pleasure to read. The paper presents information that is relevant to the governance of hydraulic fracturing and other endeavors with negative (real or perceived) externalities. I think it will be of interest to the journal's audience. Care was taken in the data analysis and presentation and the abstract and title are accurate. I have issues with the framing of the paper and the interpretation of the data and will direct my comments to these areas. I am suggesting major revisions because I feel the questions presented here are serious, but addressing them may not take that long. I do like the paper and the subject matter. I hope my comments reflect my interest in it.

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

Printer-friendly version

Discussion paper

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 aware 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. 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.

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.

The above is a major concern for me in the paper. I also have concerns about how the authors used previous work to position their own and the analysis of the data. Please see below.

I am curious if the authors considered how politics and personal interests shaped responses 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.

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.

Specific Comments 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.

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."

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.)

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." 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. 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.

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.

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.

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 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.

Line 513 – Could you say more about how experts' views are polarized here?

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.

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

Printer-friendly version

Discussion paper

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.

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.

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."

Technical Corrections I cannot locate a Whitmarsh et al. 2014 citation in the references, probably a typo.

Line 678 – typo here "event with a cause in media reporting of an event without any there being a scientific explanation for a"

Lines 683-684 – plural/singular "In particular, those who 'do not' associate earthquakes and shale gas question the 684 definition of an earthquakes."

Interactive comment on Geosci. Commun. Discuss., https://doi.org/10.5194/gc-2020-33, 2020.