

Interactive comment on “Uncertainty in regional estimates of capacity for carbon capture and storage” by Mark Wilkinson and Debbie Polson

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Thanks for your comments. You raised several points:

1) “The Introduction to the paper does not contain a hypothesis to be tested, nor a clear statement of a central objective, nor a clear statement of a knowledge gap to be addressed.” The paper tests the hypothesis that regional storage estimates are subject to considerable uncertainty, despite many published estimates having no indication of this. We’ll make this clearer in the revised MS

“I dispute that the authors have assessed the “accuracy” of any estimates – that would require comparing an estimate to a known or trusted value”. True – we have assessed the precision. We do not know the true value (it is unknowable) so accuracy cannot be

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assessed.

2) “it is not clear what we can conclude or take away from the exercise performed here by the authors.” In the same paragraph, the reviewer’s says that it is ‘obvious’ that regional estimates have substantial uncertainty. This may be true, but you would not know that from the published literature, most estimates are single numbers with no indication of uncertainty. As the ‘true’ value of capacity is unknowable, another approach has to be used to assess reliability – which is what we present. Actually, we can probably make this clearer – if your estimate of storage capacity is similar to your likely CO₂ volume to be stored, then there is a substantial probability that you will find that the CO₂ will not fit in the store (because you have over-estimated the capacity)!

3) “I think it is unclear that the probability distribution of each parameter individually should be considered to be a uniform pdf.” Assessing the pdf of an unknown quantity is always going to be difficult. If 2 experts find the same literature value for a variable, does that make that value more probably than if only 1 expert finds it? Surely not. Conversely, you could argue that if there are 10 different values published for a single parameter (e.g. mean porosity for a formation from 10 different locations) that the best estimate of the regional mean value is the mean of the individual data, making the centre of the distribution more probable than the margins, as with e.g. a normal distribution. For most of the data used in the exercise, there is so little published information that it is not possible to realistically assess the distribution, so a uniform pdf is no worse than any other.

Re: The reviewer’s section on rolling dice. We clearly stated this: “though there are more combinations of variables that will produce storage capacities around the median value than the extremes, making an estimate around the median more likely overall.” Lines 191 -193.

Re: “They may want to consult with an expert in probability or statistics”. One of us (DP) is a professional statistician.

4) “It is an interesting question whether 12 experts is enough to represent the range of uncertainty of the individual parameters.” As with the other review, there were actually 13 experts (we’ll make this clear when revising the MS), though not all respondents answered for each storage formation. I agree that more is better in this context, as in most other cases with data. However, considerable work was involved for each respondent (tens of hours) so getting more volunteers will be tricky. The point of the paper is that the experts came up with a wide range of answers - having more experts might increase the range a little, but will surely not decrease it. Equally, if you do the calculation only once (and a consultancy company for example, asked this question, would surely only do the calculation once), you will not know where is the range of possible answers you lie. Having more respondents will not change this conclusion.

The other comments do not require replies, thanks for spotting the errors, we will amend suitably.

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2019-45>, 2019.

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