**Authors’ Reply to Editor of**

**“Spatial Data Assimilation in Geologic CO2 Sequestration”**

**submitted to SPE Journal**

**SJ-0823-0080**

Dear Executive Editor Silviu Livescu,

We believe we have carefully addressed all your comments from Associate Editor and three technical reviewers, and we are looking forward to seeing our paper published in SPE Journal.

Sincerely,

*Bailian Chen, Corresponding author*

Earth and Environmental Sciences Division

Los Alamos National Laboratory, USA

**Reply to Associate Editor:**  
  
Two reviewers: TE#2 recommends major changes, while TE #1 still recommends minor revisions. TE#2 has thoroughly reviewed the whole paper and provided very comprehensive review comments. As TE#2 indicated, the framework is pre-existing, and both the method and the problem is known. Thus, the novelty or the distinct contributions of the paper has been largely compromised. One remedies here is to enhance the connection of the findings to some practical questions in uncertainty quantification for geo-sequestration (As TE#2 suggested). Similar concerns were also raised by TE#1 who recommended more defined metrics to evaluate posterior evaluations and enhanced sensitivity study of CO2 error perturbation. TE #2 has also gave many good feedbacks about improving the title, literature, methodology description,  clarity of some points, figures and captions. I concur with these comments and particularly the comment on missing description of the the methodology. As TE#2 indicated, although the methods are well established in the literature, but not every readers is a data assimilation expert. Authors should think about a better way to present this section to enhance the general readership of the paper.

**Reply to Technical Editor 1**  
  
The author's response to my previous comments remains insufficient in two key areas.

Firstly, there is a need for a more comprehensive set of metrics to evaluate the posterior evaluations. Secondly, the sensitivity study of CO2 error perturbation, as it currently stands, lacks engagement and depth, making it less compelling. Additional work is necessary to elevate the manuscript to a level suitable for publication.

* Response:

Furthermore, as highlighted by other reviewers, it is evident that this work is not the pioneering effort in exploring ensemble-based methods for assimilating spatial measurements. To strengthen the novelty of this work, it is necessary that the author considers tackling more intricate cases, seeks to enhance the performance of the ES-MDA algorithm when assimilating large amount of spatial measurements, or explores other uncharted aspects within this research domain.

* Response:

**Reply to Technical Editor 2:**  
  
After reviewing the paper by Chen et al., I believe that the paper is can be considered for publication after some revisions. The revisions I am asking for seem minor to me because it would only take looking at the data again and perhaps some replotting and additional discussion, but are major in the sense of importance to me as a reader. I recommend this publication as Publishable-Major.  
  
The paper is of importance to the field, of relevance to the readers of SPE Journal, and the objectives, methods and results are presented in a clear manner, although the paper would benefit from more depth of discussion. I enjoyed reading the paper, organization is straightforward. Not having to present the methods makes this paper an easy read, but I wonder if it is acceptable by SPE to not show methods at all. The paper would benefit from some language editing, but I did not spot too many problems.

* Response: Thanks the reviewer for the positive comments on this manuscript.

Specific comments (in the order they appear in the manuscript, not in the order of importance). Asterisks in front of comments indicate that this comment is very important to me as a reader.  
  
Title  
  
The title needs to be more specific and descriptive of the work, e.g., Assimilation of geophysics derived spatial data for state and parameter estimation in Geologic CO2 sequestration. Or something along those lines, but specific enough to point to the methods/data used.

* Response: We agreed with this suggestion. In the revised manuscript, we have changed the title to “Assimilation of Geophysics Derived Spatial Data for Model Calibration in Geologic CO2 Sequestration”.

Abstract:  
  
\*\*Line 15: the framework has been presented before and was previously developed, so it is not novel anymore and it is not developed in this paper, please adjust language. Assimilating all of CO2 saturation datapoints is no different (from a framework perspective) from assimilating well data. Or if it is, it has not been made clear in the paper. The authors say that they assimilate all of the CO2 spatial data, so methodologically, I don’t see how this is different from the previously presented framework. Please clarify.

* Response:

Line 19: The authors say that spatial data comparatively carry more information, but this is not a fair comparison, and that comparison is not shown in the paper. More data is not always more information, but in this case these are direct state values explicitly linked with both the state and the parameter being estimated. Of course they would carry more information than a few point well data, that is self-evident. The interesting question is how much more information, and do we really need the entire map of saturations and is the accuracy we get from geophysics high enough to provide high accuracy in estimation. It would be useful to deepen the question a little more than presented in the paper, because now it reads as if an existing method and framework was applied to different data and the expected result was obtained.

* Response:

Introduction:  
  
Page 2 Line 3: NRAP-Open-IAM needs to be at least explained – not everybody knows what it is.

* Response:

\*\*Page 2 Line 29: Using geophysical data for CO2 data assimilation is not new. Using spatial data may be newer, but there are definitely examples in closely adjacent fields (see Kang et al., WRR and references therein, or the work of Jonghyun Lee et al on how to handle large datasets in data assimilation). The question of using maps in data assimilation exists in many areas, not just CO2 sequestration and oil, and the literature review should include these works since the method is the same. In addition, this paragraph would be a good point to explain how spatial data is different from well data. Yes more information, but correlated information. Challenges with using large datasets in the data vector. Error propagation from geophysical inversion. There are several interesting questions that affect the practical applications of this work.

* Response:

Page 2: The methodology of the framework is not presented in this paper, which is fine for me being well versed in the specific area, but I do not know if it will be fine for the average reader of SPE. I defer to the editors. The methods are well established and known in data assimilation, but not every reader is a data assimilation expert.

* Response:

Page 3 - Line 30: “equivalent permeability distribution” do you mean that the same probability distribution/variogram was used to generate each layer, or that you generated the layers using a 3D variogram with the same PDF across the entire domain? Or that the layers are repeated? Unclear and difficult to check since no graph of the domain is given.

* Response:

Page 4 - Line 35: “we only focus on ..” The ensemble standard deviation shows zero uncertainty at the point of the wells. This makes me wonder if you are conditioning the field on known information at the well, and what information that would be. In that sense, are you using only the CO2 maps, or are you also using well data? Have you tried using only one at a time, and both combined to compare the relative worth of information?

* Response:

Figure 6: it would be useful to show differences spatially to identify where the benefit is most pronounced. Second column is given the name “prior”. Based on the caption, these are the data used, so I am not sure that it is technically the prior. The prior would be the prior distribution assumed for CO2 at the beginning of data assimilation. Which makes me wonder, what was the prior used for CO2 (it is specified for perm). The term “prior” becomes even more tricky if you consider that in the next figure you use the term “prior map” for a different field. Given that prior has a specific meaning in the context of ensemble data assimilation, please use specific words with more descriptive language.

* Response:

Figures 7 and onward: Captions need to be much more descriptive and standalone as was done in figures 1-6.

* Response:

Page 7 – line 1: “updated reservoir model” while this is semantics, it is good to be specific. It is better to say “the reservoir model with updated permeability field” since only permeability was updated in the reservoir model.

* Response:

Page 7 line 5: missing word Figure

* Response:

\*\*\*Page 8 line 6-8: Does the error of inverted seismic data work this way? Is the error in a geophysical inversion (seismic--> CO2) Gaussian? In this case, was the random error added and the 1% is the standard deviation of the gaussian error added on top of the saturation maps, or was the error incorporated in the processing of the seismic data? Which makes me also wonder, how did you go from seismic data to CO2 maps?

* Response:

\*\*\*Figure 9: I do not discern any difference between the three columns, which is concerning. At least a map of the differences needs to be shown. Theoretically, the added noise should translate to more uncertainty. Did the uncertainty maps change? They are not shown. If they did not, there is a mistake in the code. When a DA code does not produce any difference with different input data it can either be theoretically explained, or it is a mistake in the implementation. Please doublecheck and show uncertainty maps too, as well as differences in estimation.

* Response:

Conclusions  
  
Line 14: The only result that shows reduction of uncertainty in the paper is Figures 5 and 8, and they both show reduction with additional spatial data incorporated. More data will result in some reduction to uncertainty always. What is the argument here? Are the authors trying to see how many geophysical surveys it will take to reach a certain improvement in uncertainty reduction? Did they try with 4 surveys to make the point that 3 was enough? Are the authors trying to say that there is more uncertainty reduction than with well data? It is expected that there is, but is this shown in the paper? And is it even worth to use well data? Do we know what the uncertainty reduction is with only well data and with combined data? How do this different data change the risk uncertainty maps that the authors say is connected to their algorithm?

* Response:

I really wish the authors tried to connect their findings with some practical questions in uncertainty quantification for geologic CO2 sequestration, especially since the method is known, the problem is known and the framework is pre-existing, so the novelty of the paper could be in the contribution for applied projects. The paper begins by saying that this framework is used within the NRAP risk framework, but I don’t risk error propagation mentioned in the conclusions.

* Response:

References: the list of references does not include work that uses the same method in other related fields. Please see previous comments.

* Response:

Typos:  
Page 2 Line 8: multiple approache\*s\*  
Page 2 Line 18: site\*s\*  
Page 5 – line 5: Figures 4 and 5

* Response: Thanks. The typos were fixed.

**Reply to Technical Editor 3**  
  
This paper deals with data assimilation using saturation maps for CO2 sequestration. The authors used ES-MDA-GEO for history matching to update permeability models. They analyzed the effect of the number of spatial data and the effect of noise level in saturation maps. The overall flow of the paper is well-written, but several key information for the methodology is missing in the current manuscript and additional analysis is required to meet the standard of SPE Journal. Therefore, my decision for SJ-0823-0080 is major revision.  
  
1) 4D seismic history matching for CCS has been researches by several researchers including the papers as below  
[https://www.earthdoc.org/content/papers/10.3997/2214-4609.20140110](https://urldefense.com/v3/__https:/www.earthdoc.org/content/papers/10.3997/2214-4609.20140110__;!!Bt8fGhp8LhKGRg!Ekv9b_BxMcAiullaniMnZT1QgjK6o7Fq47lRFpm9M6sqLNK76UFib2GLsVZ75TsMj9fzkAivJq9K_WcmHogYbEbpsu9gaKg$)  
[https://www.sciencedirect.com/science/article/abs/pii/S0098300420305963](https://urldefense.com/v3/__https:/www.sciencedirect.com/science/article/abs/pii/S0098300420305963__;!!Bt8fGhp8LhKGRg!Ekv9b_BxMcAiullaniMnZT1QgjK6o7Fq47lRFpm9M6sqLNK76UFib2GLsVZ75TsMj9fzkAivJq9K_WcmHogYbEbp46bokuU$)  
[https://library.seg.org/doi/abs/10.1190/tle36030234.1?journalCode=leedff](https://urldefense.com/v3/__https:/library.seg.org/doi/abs/10.1190/tle36030234.1?journalCode=leedff__;!!Bt8fGhp8LhKGRg!Ekv9b_BxMcAiullaniMnZT1QgjK6o7Fq47lRFpm9M6sqLNK76UFib2GLsVZ75TsMj9fzkAivJq9K_WcmHogYbEbpVePwMzg$)

* Response:

2) Related to numerical simulation model,  
  
2-1) Was the injection well completed on all 11 layers? If so, what was the reason for not simulating just single layer because the 11 layers have the same properties? Only simulation grids would increase. Also, in case of CO2 injection, the lowest layer is applied for completion.

* Response:

2-2) Provide relative permeability curve

* Response:

2-3) Provide pressure increase at the end of injection (5 year)

* Response:

2-4) In Figure 6, it should be clarified that the observed saturation maps were obtained from the reservoir simulation results of ground truth model, not from seismic surveys.

* Response:

2-5) The geological model does not have any anticline structure, which results in minimal changes in CO2 saturation maps between the 15-year and 5-year of the ground truth in Figs. 6 and 7. Therefore, it is meaningless to consider for the 15-year timeframe.

* Response:

3) Related to ES-MDA-GEO,  
  
3-1) Is it correct that for the first assimilation step (the first year) the prior models are updated using ES-MDA-GEO using the saturation map data from the first year, and then, for the second assimilation step (the third year), the updated model are modified again using ES-MDA-GEO using both the first-year and third-year saturation maps?

* Response:

3-2) If yes for 3-1), please compare the results that ES-MDA-GEO is applied to prior model once using 1, 3, and 5 years saturation maps together.

* Response:

3-3) What is the inflation factor and the number of assimilation step for each assimilation step?

* Response:

3-4) How the 3D saturation map data (51x51x11) is used for state vector? Please provide the state vector for the assimilation step 3 (may be 51x51x11 permeability, 51x51x11 1st year saturation,  51x51x11 3rd year saturation, and 51x51x11 5th year saturation).

* Response:

4) Related to results,  
  
4-1) In Fig. 5, What is the reason that the 1-year averaged model matched permeability properly even beyond the CO2 area of the ground truth data in Fig. 2? The 1-year averaged model is similar with the updated R1 model at 5-year in Fig. 4.

* Response:

4-2) Please provide the top and base layers together for Figs. 3, 4, 5, 6, 7, and 9. If there are too many figures in the manuscript, please provide the results of the base layer in the appendix.

* Response:

4-3) In Fig. 8, Please provide the equation for MSE.

* Response:

5) Related to data noise,  
  
5-1) Was noise added only to the observed saturation map, or was noise also added to the saturation maps of each model?

* Response:

5-2) Was the same error set to the measurement data error, Cd, in ES-MDA-GEO as well?

* Response:

5-3) When you mention a 10% error, what is the reference point for this 10% error?

* Response:

5-4) Regarding the noise, was it assumed to be white noise with specified mean and standard deviation following a normal distribution?

* Response:

6) Related to conclusion, Please compare quantitative metrics like MSE when comparing with previous research (Chen et al., 2020).

* Response:

**Reply to Executive Editor:**

Thanks to the authors for submitting their work to SPEJ. As two TEs and the AE raise significant issues of substance, the authors should address those in a suitably revised version. I would expect another round of reviews.