Dear Authors,

Thank you for your responses and explanations. While I agree with some of your points, overall, I am not satisfied with the responses. I would like to address three key aspects: the format of the responses, the answers to my main questions, and the comments in the text.

1. The form of the answers.
   I could not locate an updated version of your manuscript; I only see the old file with your responses. It is difficult to follow your answers without the use of track changes. Please ensure that the updated manuscript with track changes is provided for easier review.
2. The answers of my "main concerns"
- Objective Function Parameters (Equation 14):

You stated that the weighting factors (w_d, w_x, w_s, w_C) either have equal contributions or are adjusted based on initial model information.

My concern is that if these parameters have no effect when equal ("equal contributions"), how does the operator determine their weighting values?

Could you please provide a clearer explanation of how prior information (e.g., geology, boreholes) is incorporated into the inversion process?

I suggest adding more detailed statements, discussion, and supporting evidence to the text.

- Clustering Parameters:

The parameters related to clustering include the "Gustafson-Kessel fuzzy C-means" method, fuzziness, and the number of clusters.

- Your theory section discusses conventional Fuzzy C-means (FCM), but there is no clear distinction between FCM and the Gustafson-Kessel method in relation to your inversions.
- The theory section lacks a discussion of fuzziness and the number of clusters.
- Although spatial properties are mentioned, I do not see them addressed in the inversion process.

Please clarify these points and provide relevant discussions and evidence in the manuscript.

- Inversion Results with Different Noise Levels and Starting Models:

I believe adding results for different noise levels and initial models would not require much extra space if you manage the presentation more concisely and reduce the number of cases.

Regarding the noise levels, it seems that results for SNR = 5 dB or even SNR = 1 dB show fewer vertical and horizontal artifacts than those for SNR = 10 and SNR = 100. Could you explain this?

Additionally, for the initial model cases, could you clarify why the less smooth model (case 1) results in a higher NMSE (0.04) than case 2?

3. Comments in the Text

Since I have not received the updated file, I will address a few points:

- Line 95 to the end of page 3:

My comment was that the statement "The membership sections that show the degree of belonging of each data point to different rock types or facies are new concepts that could be used in seismic interpretation" is not a new concept.

Your response was, "I couldn't find any previous work that creates membership sections. Please see section 3."

However, this concept has been presented in several works, including Kieu and Kepic (2020), which is already in your reference list. I believe you need to review the previous work more thoroughly.

- Page 10, Line 298:

My question is "why do you use 6 clusters instead of 11 as the results of elbow method (line 277 and 278)?".

Your answer "(Chevitarese et al., 2018)"

This work is not referenced in the relevant statement within your manuscript. I think this is not a proper scientific explanation. It is your responsibility to provide a convincing, evidence-based justification for your choice.

- Page 23, Figure 5:

My comments "I think low resolution of the image make difficult to see. The caption of figure should detail enough to help reader understand major idea of the figure without reading the text".

Your answer "the original figure is a sharp 500dpi image. something happens while converting the files to pdf. We have replaced it again"

It is your responsibility to ensure the quality of all versions of your manuscript.

I look forward to your revised manuscript with these points addressed.

Best regards,