BSSA-D-23-00271  
3D Multiresolution Velocity Model Fusion With Probability Graphical Models  
  
Dear Zheng Zhou,  
  
Your paper referenced above has been reviewed for publication in BSSA. In light of the referees' comments, which appear below, the Editorial Board has decided that significant revisions are required to render the paper acceptable for publication. Due to the level of revisions needed, an additional round of reviews may be required. Please consider these comments as you make your revisions, which are due by Jan 12 2024 11:59PM.  
  
To submit a revision, go to https://www.editorialmanager.com/bssa/ and log in as an author. Under the menu item Submission Needing Revision, you will find the submission record for this paper.   
  
1) Submit a detailed response to reviews, including a point-by-point list of changes or rebuttals. During upload of your revised paper, you will find the step "Respond to Reviewers." Entering information in the text box on that page is required; you can enter either the complete responses to reviews or a statement that responses are in an uploaded file. The information in the text box will be available to reviewers. In the "Attach Files" step, you can upload the files labeled "Response to Reviews" and "Annotated Manuscript." Both of those files will be available to reviewers. A file uploaded as "Letter to Editor" will be seen only by the editors.   
  
2) Submit a "clean" version of your revised manuscript in Word or Latex under the "Manuscript" category and a "track changes" version of your manuscript under the "Annotated Manuscript" category.  
  
3) Submit figures with the figure caption below each figure. If your paper has color figures and you have opted for color online and grayscale in print, only submit the color version of each color figure.   For information on preparing proper figures and on previewing grayscale versions of color figures, see our guidelines and tutorial at https://www.seismosoc.org/publications/ssa-art-guidelines/. If you are printing your figures in color, the cost is $250 per color figure, in addition to page charges, which can be found at: https://www.seismosoc.org/publications/journal-publication-charges/.  
  
4) Electronic Supplements should be submitted under the "Supplemental Material" category in Editorial Manager. The main page should be submitted as a Word file, and include the paper title, author names, a short description of the supplement, table and figure captions, and references. Tables and figures should be uploaded under the "Supplemental Material" category in an approved format.  
  
I look forward to hearing from you.  
  
Best regards,  
  
Clifford H. Thurber  
Guest Editor  
Bulletin of the Seismological Society of America  
  
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REVIEWERS' COMMENTS:  [Note that reviewers sometimes upload files as part of their reviews.  If attachments have been uploaded, they can be found either as links at the bottom of this email or when you log into the online submission system, select "Submissions Needing Revisions," and then select the action "View Reviewer Attachments."]  
  
Guest editor:  
First, I note that the authors have seriously violated the font size requirement of BSSA. The revised manuscript must adhere to BSSA formatting requirements.  
Response: Sorry about that. We have changed the font size to 12pt.

BBSA has received two very thoughtful reviews of this manuscript. Both call for significant revisions, and I concur. Both also support publication of the paper after suitable revisions are made.  
  
The main points raised by Reviewer 2 concern the need for additional details in a few areas. These include (1) the comparisons done for validation, (2) the methodology (perhaps an extensive presentation of the method could be put in a supplementary section), and (3) modeling choices that must be made by the user. The reviewer also points out some inappropriate referencing. Finally, the reviewer points out that the authors should make their code available upon publication, e.g. at GitHub.  
  
Reviewer 1 provides what I would call a “roadmap“ for making essential improvements to this manuscript. The review partially echoes the comments of Reviewer 2, but provides extensive recommendations for improving many additional aspects of the paper. Rather than attempting to summarize all of the reviewer's comments, the authors should endeavor to address all the points raised. I will add, however, that in my opinion, **arranging many of the figure panels vertically** instead of horizontally, and thereby enlarging them, would make the figures much clearer for the reviewers, and ultimately the readers.  
  
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Reviewer #1: Review of "3D Multiresolution Velocity Model Fusion With

Probability Graphical Models" by Zhou et al.

I have read this manuscript, and I waive anonymity.

This manuscript focuses on the problem of synthesizing multiscale velocity models using a new approach – probability graphical models.

The work is important and of interest to the Bulletin of the Seismological Society of America. Combining velocity models with varying spatial resolution is important for natural hazard assessment and geologic studies. Therefore, more research is needed, particularly in exploring other methods for producing and validating such hybrid models.

In my view, this paper is publishable. Still, it requires significant revisions – careful language, additional references, including missing information, and proper validation tests – that the authors should address before acceptance. Below are my primary concerns.

Major comments

1) On language, tone, and structure.

Lines 9 – 10: It is stated here that the goal is to enhance the precision of ground motion simulations, yet the validation tests in the paper do not seem to support it. I question whether the authors think an improvement in travel time misfits implies an improvement in ground motion predictions (lines 15 – 17).

Response: You raised a valid point regarding the validation tests and their connection to the precision of ground motion simulations. We agree that an improvement in travel time misfits does not directly imply an enhancement in ground motion predictions. We intended to highlight that the reduced misfit in travel times is an initial step towards achieving more precise ground motion simulations. In response to your feedback, we expand our discussion in this section to clarify the relationship between travel time misfits and ground motion predictions.

See Line 26 in our revised manuscript.

Lines 17 – 18, 29 – 30, 171 – 173: What conventional methods? Yes, the Gaussian and average filters tend not to preserve details depending on the dimensions of the filters. However, window functions like cosine taper or the dictionary learning methods preserve details, especially when the appropriate parameters are used (e.g., Fig. 6b4 for the DL model). It is better to specify the technique, like in lines 35 – 37. Also, note that while DL may be categorized as a conventional method, the statement in lines 222 – 225 does indicate that the DL method preserves details.

Response: In the revised manuscript, we explicitly mention the Gaussian and average filters, along with a comparison to the cosine taper and dictionary learning methods. These methods can with appropriately tuned parameters preserve details as shown in Fig. 6b4 for the DL model. We highlight the machine-learning-based methods (including DL and PGM), with their parameters adaptively optimized on the training data giving better performance.

See Lines 54 and 258 in our revised manuscript.

Lines 24 – 26, 81 – 82: The PGM method relies on experts to manually tune variables such as the inner and outer polygons defining the effective vertices set (Fig. 1c) or the number of discrete label maps (Fig. 2 right). These hyperparameters do not seem constrained by the optimization process and thus require the same manual tuning that the conventional approaches utilize.

Response: We recognize that this aspect of the method mirrors the manual tuning required in conventional approaches. In the revised manuscript, we address this by discussing how the PGM method's manual tuning differs from or aligns with that of conventional methods. Moreover, we explore ways to potentially automate these hyperparameters or integrate them into the optimization process to strengthen the method's distinctiveness and efficacy.

Lines 62 – 64: No real data is used in the validation tests, so this claim of superiority must not be made. Note that several other factors, such as computational cost, may not make the approach suitable for routine applications (e.g., 10,000 iterations in line 162). Another factor is accounting for irregular model resolution for the HR domain.

Response: To address this, we amend the manuscript to present a more balanced view, highlighting the method's potential while acknowledging its limitations, including the computational intensity and challenges in handling irregular model resolution for the High-Resolution (HR) domain. We incorporate a discussion on the feasibility of the method in real-world applications as future works.

Lines 276 ¬– 278: This statement contradicts lines 230 ¬– 231 on the performance of the methods in the checkerboard model, even though it supports the information presented in Table 3. Also, note that “all established baseline techniques” do not include the cosine taper method for the 2D tests.

Lines 183 – 211: The subsections on “Comparison with Conventional Methods” should be incorporated into the introduction by adding the relevant references suggested below.

Response: We revised these sections to resolve the contradiction between lines 230 – 231 and the results in Table 3. Moreover, we clarified that “all established baseline techniques” did not include the cosine taper method in the 2D tests. The revised manuscript accurately reflects the scope of the baseline techniques used and ensures consistency across all sections.

See Line 268 for the updates.

(2) Additional References

Lines 20 – 22: Ajala & Persaud (2021) and Ajala & Persaud (2022) should be cited here.

Lines 184 – 191: Include appropriate references on the use of Gaussian kernels here.

Lines 193 – 200: Ajala & Persaud (2021) and Ajala & Persaud (2022) should be cited here. They pioneered the design of 3D window functions with arbitrary support for merging multiscale datasets. Also, note that it does not have to be a cosine taper. It can be any window function. See the supplementary material of Ajala & Persaud (2021) for an example of a Trapezoid window function. So, this subsection can be retitled “Window Taper Function Interpolation.”

Lines 202 – 211: Cite the recent work by Zhang & Ben-Zion (2023) on using sparse dictionary learning for merging multiscale models. [If preprints can be cited].

Response: Thanks for suggesting the valuable reference, we have added these references to the introduction and methodology sections.

(3) Unclear or missing information

Lines 72–74: How is the continuous velocity map determined? Table 1, which summarizes the algorithm, states that A is initialized by superimposing AHR over ALR. Yet, the map of A shown in Fig. 2 appears smooth, unlike the direct superimposition in Fig. 1c.

Response: The smooth appearance of the continuous velocity map (Fig. 2) comes from the results superimposition of HR over LR after processing by our PGM processing steps. Fig. 2 doesn't directly represent the initial superimposition (shown in Fig. 1c) but is the result of additional smoothing and integration processes. Here we just use the smoothed model as a demo to show what a discrete label map looks like.

Lines 216–218, 218–219, 220–222: I think these sentences may refer to a different version of Fig. 6 in the manuscript. No ray path density information is presented in Fig. 6 (a2, b2). Fig. 6 (a3, b3) does not show the label mask maps. There is no Fig. 6 (a6 and b6). Where are the stations that are uniformly distributed for the checkerboard model?

Response: Thanks for pointing this out. Now we add the station distribution and ray path information in Fig 6.

Lines 230 – 231, 276 – 278: What does the uniform distribution of stations mean, and where is it shown?

Why isn’t the cosine taper method applied to the 2D example?

Response: We have changed the uniform distribution to a uniform grid of stations with a grid spacing of 10 pixels.

Line 251: How was the cosine taper applied? What were the tapering parameters? Also, no cosine tapered model is shown in any of the figures.

Response: The cosine taper was applied in a manner standard to geophysical data processing, with parameters chosen based on the spatial frequency content of the data. The tapering parameters are specified in XX made. The absence of a cosine tapered model in the figures is an oversight.

Lines 259 – 260: The 2D example uses the Gaussian filter, not the cosine taper.

Response: The decision to employ a Gaussian filter in the 2D example, rather than the cosine taper, was based on the specific requirements of the data and the objectives of the 2D analysis. The Gaussian filter was chosen for its effectiveness in smoothing the data while preserving important features, which was crucial for the objectives of the 2D example.

(4) Validation tests

The travel time validation test relies on synthetic travel times. Is it possible to use real observed datasets? If not, lines 15 – 17 must be removed from the abstract as it is misleading.

Response: We used the calculated on the synthetic model as the ground truth. Now we highlight in the abstract we used the computed travel time in our tests.

See Line 18 for the revision.

Lines 174 – 178: This validation test does not seem sufficient. I suggest using real data that samples the true Earth structure and seeing how well the merged and original low-resolution models predict the observations.

Response: Good suggestion. However, this is beyond the scope of the current work, but the focus of current and future studies.

Lines 254 – 259: What reference model is used to compute the reference travel times? Is it the background LR or the directly superimposed LR and HR model?

Response: Yes, we used the model that directly superimposed HR over the LR model, and used it as the reference model to compute the travel times.

The evaluation of Earth models cannot be based solely on quantitative metrics. The geology matters, too. I suggest adding a little discussion on why the Ridgecrest area was selected for the study and how well the merged models represent what is known about the region's geology. The geologically reasonable models may not always represent the models with the best

quantitative metric (e.g., Ajala et al., 2022).

Response: We have added a paragraph discussing the geology around the Ridgecrest area and explaining the motivation for selecting this area because we observed improvements in ground motion simulation results with the properly fused velocity model. This area could be a representative area for testing.

See Lines 232-242.

Minor Comments

(5) Figures

The figures are not always clear. I am guessing they will be updated with higher-resolution versions.

Response: Correct! We have updated the resolution of the figures.

Figure 7: I suggest plotting these in simple map views to make the differences more transparent.

Response: We have replotted the slices in the plan view.

Figure 8: Panel label typo in the bottom right panel of Fig. 8b. “B3” -> “B4.”

Response: We have fixed it in our revised manuscript.

I hope the authors and you find this review helpful.

Best regards,

Rasheed Ajala

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Reviewer #2: Review of "3D Multiresolution Velocity Model Fusion With Probability Graphical Models" by Zhou et al.  
  
In this work, the authors present an innovative method that is based on the Probability Graphical Models (PGM), commonly used in image processing, to combine local high resolution and regional low resolution models. To fuse/combine models with different resolutions with each type having its benefits is a challenging undertaking but may turn out very useful for ground motion simulations and other seismological studies. This work, with more convincing tests, will benefit and inspire other model fusion works at local to global scales. I have a couple of suggestions and comments below. I think this work would suit BSSA after moderate revision.  
  
Method validation. The authors validate their PGM method and other methods including dictionary learning via travel time misfit and a couple of other criteria and find the new method systematically outperforms the others. I did not find details about what the authors are comparing to. For example, is the travel time misfit derived from comparing with the travel times calculated using the high resolution model? I'm wondering if it is possible to compare the performance of different merging methods using real observations. For example, selecting several well-located earthquakes in the Ridgecrest region and comparing the travel times (S wave arrivals for merging Vs model) using different merged models. This comparison will be more convincing than a seemingly 'high resolution model'.

Response: We acknowledge the need for a more detailed comparison in the method validation section. However, this is beyond the scope of the current work, but the focus of current and future studies.  
  
Other minor comments:  
The methodology section is somewhat obscure. After reading the methodology section (Markov Random Field Methods and the following subsections) several times, I have a general grasp of the method but still have not understood the method very well. I would suggest the authors add more details, for example, some intuitive physics of the method to help the readers understand how and why the method works. The current version is a little technical with many nomenclatures and acronyms which makes the method difficult to follow.

Response: We realize that the methodology section, particularly regarding Markov Random Field Methods, may be too technical for some readers. To address this, we revised this section to include more intuitive explanations and physics-based perspectives and also added a pipeline figure for our algorithm. Our aim is to make the method more accessible to a broader audience, without compromising on the technical rigor. See Lines 205-213 for the updates.  
  
Some details need more clarification. For example, what is the reason to choose 6-cluster labels? I imagine this parameter could affect the final merging results, as choosing 1 would lead to similar results as Gaussian smoothing? Also, how to choose the border between low resolution and high resolution models (line 82) and will different choices affect the final merged model?

Response:

Your point about the choice of 6-cluster labels is well-taken. We will add a paragraph to explain the rationale behind this choice and discuss how different parameter settings, including the number of clusters, impact the final model fusion results. Additionally, we clarified the criteria for choosing the border between low and high-resolution models and explored the sensitivity of the final model to these choices.

See Lines 108 and 205-213 for the updates.  
  
Line 24 and 35. There are some inappropriate citations. For example, the Fang and Zhang 2014 paper is more about using wavelets, similar to dictionary learning, in seismic tomography rather than Gaussian kernel smoothing. So I would not cite Fang and Zhang 2014 here. Also, the Zhang et al., 2013 paper (line 48) is about full waveform inversion based on a variational method to improve the computational efficiency and obtain the uncertainty at the same time, so I am not sure if it is appropriate to cite here to indicate the application of PGM in seismology.

Response: We appreciate your attention to the accuracy of our citations. The references you mentioned have been reassessed, and corrections made to accurately reflect the literature related to our study.

See Line 31 for the updates.  
  
The authors did not mention the computational efficiency of the method. I feel merging the 3D models may take more time but the 2D case would be less computationally demanding. If this is the case, would it be possible for the authors to share their codes (A Jupyter notebook would be great for people interested in playing around with their own cases).  
  
Response: We have provided a code availability statement in the Data and Resources section.

Attachments for Author (if applicable): \*\*\*\*\*\*\*\*  
  
There is additional documentation related to this decision letter. To access the file(s), please click the link below. You may also login to the system and click the 'View Attachments' link in the Action column.  
  
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