Round One: Reviewer A

'A Soil Velocity Model for Improved Ground Motion Simulations in the U. S. Pacific Northwest' by Grant, Wirth, and Stone is a well written and compelling paper that supports the increased development and used of regional/local velocity models in seismic hazard studies, particularly for regions with significant deposits of low velocity sediments. It is well organized and provides a clear description of the data and modeling techniques used. I am by no means an expert on simulation-based ground motions, but I found the paper easy to follow and clear.

I have several comments that can likely be addressed through minor revision to the submitted manuscript.

- Figure 1 is described in the text as providing a tectonic setting for the study, but the figure as presented provides little if any tectonic information. I suggest modifying Figure 1 to show major tectonic boundaries discussed in the text. Also, it would be helpful to show the location of the Nisqually earthquake on Figure 1 to place it in a larger geographic context for those not familiar with the Puget Sound area.
- Line 197 states that there is insufficient data to constrain additional subdivisions in velocity profiles, but no basis for this statement is provided. How was the data assessed to come to this conclusion?
- The authors state that there is a minimum value of 0 imposed on their model for the surface velocity for physicality. A Vs at the surface of 0 is not really a physical constraint. A bit more discussion of the basis for this choice is warranted. In addition, it would be informative to see the minimum and maximum Vs values their model predicts based on the constraints they impose and how this compares to measured values used in the analysis.
- There is a brief description of how the SVM was merged with the CVM, but there is no discussion of how this was done to avoid artificial impedance contrasts. There is a reference to an in press open file report in the Discussion section, but how the datasets are merged is an important component in developing the simulated ground motions and warrants greater discussion in the main text. Particularly since this merging directly impacts the results presented in Figures 7-9.
- I would like to see a bit more discussion about Figure 9 and its implications. There is a curious banding in the results where the CVM has an improved fit over the SVM. I'm curious to know if that's an artifact of not having many sites with those specific Vs30 values.

Responses to reviewers are in red

Round One: Editor's Decision Letter

Dear Alex Grant, Erin Wirth, and Ian Stone:

I hope this email finds you well. I have reached an initial decision regarding your submission to Seismica, "A Soil Velocity Model for Improved Ground Motion Simulations in the U. S. Pacific Northwest". Thank you once again for submitting your work to Seismica.

To date, I have received only one review for your manuscript. A surprising number of people have declined to provide a review, which I believe is largely a reflection of the somewhat turbulent times we are experiencing in public sector science in the U.S. Ultimately, I will want to see at least two complete reviews for this work before proceeding forward with potential publication. That being said, and given that I currently do not have any other reviewers committed at this juncture, I wanted to return the manuscript to you with the comments of the reviewer I was able to arrange. You will see that they are generally enthusiastic about the work and that they offer some reasonable thoughts for improvement. I think the best path for now would be for you to address these comments and submit a revised version. I will do some work in the background to see about arranging a second reviewer to examine that revision once ready.

Please particularly note the first reviewer's comments about the reference to Wirth et al in press and its implications for this manuscript. They did not consider this a "deal breaker" in terms of providing a recommendation for the manuscript, but they do have several questions and requests for clarification. In addition, Seismica's policy will not allow this manuscript to be published until the Wirth et al report is published.

When you are ready to resubmit the revised version of your manuscript, please upload:

A 'cleaned' version of the revised manuscript, without any markup/changes highlighted.

A pdf version of the revised manuscript clearly highlighting changes/markup/edits.

A 'response-to-reviewers' letter that shows your response to each of the reviewers' points, together with a summary of the resulting changes made to the manuscript.

If you deem it appropriate, please check that the revised version of your manuscript recognises the work of the reviewers in the Acknowledgements section.

Please note that Seismica does not have any strict deadlines for submitting revisions, but naturally, it is likely to be in your best interest to submit these fairly promptly, and please let me know of any expected delays.

I wish you the best with working on the revisions. Please don't hesitate to contact me with any questions or comments about your submission, or if you have any feedback about your experience with Seismica.

Kind regards,

Randy Williams

Round One Author Response to Reviewer A

'A Soil Velocity Model for Improved Ground Motion Simulations in the U. S. Pacific Northwest' by Grant, Wirth, and Stone is a well written and compelling paper that supports the increased development and used of regional/local velocity models in seismic hazard studies, particularly for regions with significant deposits of low velocity sediments. It is well organized and provides a clear description of the data and modeling techniques used. I am by no means an expert on simulation-based ground motions, but I found the paper easy to follow and clear. I have several comments that can likely be addressed through minor revision to the submitted manuscript.

Figure 1 is described in the text as providing a tectonic setting for the study, but the figure as presented provides little if any tectonic information. I suggest modifying Figure 1 to show major tectonic boundaries discussed in the text. Also, it would be helpful to show the location of the Nisqually earthquake on Figure 1 to place it in a larger geographic context for those not familiar with the Puget Sound area.

In the revised text, we now refer to the map in Figure 1 as providing a "regional overview" (i.e., as opposed to a "tectonic overview"). To show tectonic boundaries, we would need to zoom out significantly, which would make it difficult to see the mapped symbols. However, in the revised map, we have added regional crustal faults to provide additional tectonic context. As the reviewer suggests, a star has been added marking the Nisqually EQ epicenter.

Line 197 states that there is insufficient data to constrain additional subdivisions in velocity profiles, but no basis for this statement is provided. How was the data assessed to come to this conclusion?

In the revised manuscript, we now more clearly state that we require a minimum of 50 Vs profiles for a particular geologic unit to be included in this study. We feel this relatively high data threshold is required to reasonably capture the velocity structure of these geologic materials over a wide range of site conditions (e.g., Vs30). For example, there are 22 sites in loess deposits that could be used to model the Palouse deposits of eastern Washington, but that potential soil velocity model would be far less well constrained than our current soil velocity model domain with the fewest number of profiles (i.e., fill and alluvium; 93 sites).

The authors state that there is a minimum value of 0 imposed on their model for the surface velocity for physicality. A Vs at the surface of 0 is not really a physical constraint. A bit more discussion of the basis for this choice is warranted. In addition, it would be informative to see the minimum and maximum Vs values their model predicts based on the constraints they impose and how this compares to measured values used in the analysis.

The minimum Vs value at the surface (0 m/s) was selected to be as unobtrusive as possible to fitting the Vs profile shapes, while forcing values to be real (i.e. physically possible values).

While it may make intuitive sense to impose a higher Vs floor of 50-100 m/s at the surface, there are sites within our study region with extremely low Vs30 (80 – 100 m/s); these sites have actual low-Vs measurements that we hope to capture. Regardless, using a higher minimum Vs at the surface (e.g., 50 m/s) has little-to-no impact on the vast majority of our predicted sites. For example, the current surface Vs for a Vs30=400 m/s site is ~200-250m/s and is unaffected by a floor change. Our maximum Vs constraint of 1200 m/s is simply to remove hard rock sites not applicable to this development of a soil velocity model.

There is a brief description of how the SVM was merged with the CVM, but there is no discussion of how this was done to avoid artificial impedance contrasts. There is a reference to an in press open file report in the Discussion section, but how the datasets are merged is an important component in developing the simulated ground motions and warrants greater discussion in the main text. Particularly since this merging directly impacts the results presented in Figures 7-9.

We agree with the reviewer that avoiding artificial impedance contrasts is important for ground motion modeling. In the revised manuscript, we have added additional details about how the SVM was merged with the CVM for sites in Washington State. Namely:

- In the Puget Lowland, "we follow the soil velocity model Vs profile until the depth at which it exceeds the Vs of the CVM model". Since both the SVM and CVM are smoothly varying in Vs and finely-spaced in depth, this results in a smoothly varying Vs profile with depth.
- For sites on artificial fill and alluvium, we now clarify that these sites are also within the Puget Lowland, so the impedance contrast at the base of the fill and alluvium layer (i.e., the boundary between fill and alluvium, and Quaternary sediment) is minimal. We note that it is possible that the impedance contrast is stronger in the real Earth. However, our simulated ground motions at these sites match observations from the M6.8 Nisqually earthquake well.
- For all other sites, Vs values were linearly interpolated between 50 m (Vs from SVM) and 100 m (Vs from CVM) depths specifically to minimize any sharp impedance contrasts.

I would like to see a bit more discussion about Figure 9 and its implications. There is a curious banding in the results where the CVM has an improved fit over the SVM. I'm curious to know if that's an artifact of not having many sites with those specific Vs30 values.

We believe that the reviewer is referring to the fact that Figure 9 only has data at a subset of Vs30 values, which results in a "banding" appearance. This is because (as the reviewer alluded to), each "band" corresponds to the Vs30 at one specific seismic station, and some Vs30 values had no recording seismic stations.

In the revised manuscript, we modified Figure 9 to average results from stations that were in the same Vs30 'bin' (i.e., to avoid the overplotting of data points). To improve clarity, we have also provided additional details about Figure 9 in the figure caption.

Additional Note: we have added a couple of additional citations to published work including:

Wirth, E.A., Grant, A.R., Stone, I.P., Stephenson, W.J., & Frankel, A.D., 2025, Data for 'A 3-D Seismic Velocity Model for Cascadia with Shallow Soils & Topography, Version 1.7': U.S. Geological Survey Data Release, https://doi.org/10.5066/P14HJ3IC.

As it pertains to the current work, this published data release of the full CVM + SVM should be sufficient for reproducibility. We are working to ensure the remaining "Wirth et al. *in press*" is fully citable (ultimately the paper would reference Wirth et al. 2025a and 2025b), but is currently held up by the status of this manuscript.

Round Two: Reviewer F

The manuscript presents a Pacific Northwest Soil Velocity Model (SVM) developed to improve ground motion simulations by incorporating near-surface velocity structures. Using 649 Vs profiles compiled from Ahdi et al. (2017), WA DNR (2021), and Friedman Alvarez et al. (2024), the authors classify profiles into four geologic domains. The study fits depth-dependent Vs profiles using log-linear or linear relationships (Equations 1–4), constraining parameters by Vs30 and Vs100. Compared to existing models, the new SVM shows reduced RMSE and better agreement with measured profiles. The authors test the SVM by merging it with the Cascadia Velocity Model (CVM) and simulating the 2001 M6.8 Nisqually earthquake. The CVM+SVM simulations improve agreement with recorded high-frequency ground motions ($\geq$0.5 Hz), especially at low Vs30 sites.

Compiled Review Questions and Comments

- **Figure 1** – Geological groups differ between data sources (Ahdi et al., 2017 vs. WA DNR, 2021); for example, some areas are red (fill/alluvium) in one and white (Puget Lowlands) in the other. Is this due to map resolution, classification criteria, or data uncertainty?
- **Figure 1** – What are the solid black lines?
- **Figure 1 and Line 142, and other content** – The "other" category appears to include a wide variety of units; some are in the coastal range, and others seem to be in the Columbia Basin. Does grouping them together introduce extra uncertainty? Since filtering is applied to exclude rock sites, I assume the "other" category refers to "other sediments." ?
- **Equations 1 and 4** – Please provide more explanation on why fill and alluvium follow a different simpler functional form.

- **Equation 5** – You state that uncertainty is a function of depth, but the equation expresses it as a function of Vs. If the equation is correct, does Vs represent the predicted velocity profile? Please clarify.
- **L332** – What causes the output profile Vs30 to be biased relative to the input Vs30? Is it due to the functional form, depth extrapolation, or fitting constraints?
- **Equation 6** – Does this correction affect the other parameters *a–d* as well? Since Vs30 should be calculated from the velocity profile, please clarify.
- **Figures 3 & 4** – Could you add residual histograms to show the distribution and determine whether any depth-dependent bias exists?
- **Figure 5** – What does the shaded area represent? Is it uncertainty (±1σ) or measured data variability?
- **L415 & Figure 6** – The current misfit calculation (sum of absolute residuals) may mask depth-dependent bias. Please consider adding a depth-dependent bias analysis.
  The histograms in Figure 6 suggest possible overprediction, as residuals are summed across depths, which could conceal systematic bias.
- **L452 (accuracy)** – Geyin and Maurer (2023) released a national Vs30 map, not a regional one. Please revise the text to clarify that you used the regional portion.
- **L452 (methodology clarification)** – Which version of the Geyin and Maurer Vs30 map did you use, with or without topography as a predictor? Given that their machine learning model sometimes produces scattered or unreasonable Vs30 values (possibly due to overfitting), why did you choose this Vs30 map over the USGS National Vs30 map?

<u>Round Two: Reviewer G</u>

This paper proposes a Vs profile model adapted to the The Pacific Northwest (PNW) of North America area for shallower depths with lower shear wave velocity. The objective is to model ground motion for a specific earthquake and to take into account the site response at least up to 1.75 Hz. I found this study interesting in the cope of the journal and original as it is a specific application of a nex Vs model, but it requires additional work to fully demonstrate the benefits of this specific Vs model for the ground motion modelistation and in general the authors should provide a deeper analysis of the figures and tables. Please find below some comments that should be considered before publication:

Line 271: When using Vs100 from the generic PNW soil model to constrain their Vs profile, did the authors compare the value of Vs100 to those in their database, even if it is limited. Can it be shown or at least mentionned ?

Line 272: Please provide a brief description of the method used in Ahdi et al. (2017).

Line 329, Table 2: Can the authors comment on the results obtained? Comparison of sigma with alternative Vs profile models?

The authors mention the work of Wirth (in press), which appears to be a companion paper to this one. However, as it is not yet available and part of this article relies on the results of that report, I suggest that the authors provide more details on the methods. Besides, it is a report, not a research paper.

Figure 2: The uncertainties in the predicted Vs should be shown.

I do not understand the mention of a Vs30 equal to 400 m/s because all the predicted models seem to have very different Vs30 values. I also do not understand why the authors use a Vs30 of 185 m/s for the alluvial. Please provide more details.

Line 379: Could the authors provide numbers to illustrate the uncertainty?

Figure 3: It would have been beneficial to have an evaluation of the resonance frequencies of these profiles, even in 1D, because the following models stop at 1.75 Hz. Do these Vs profiles have site amplification below 1.75 Hz?

Lines 424 and Figure 6: Figure 6 is not enougth described and discussed. When I look at the histogram of the Puget Lowlands, I have the impression that the yellow model provides a lower RMSE compared to this study. I guess if we have to look at a different metric, such as the median instead of the sum of RMS, the proposed profiles may not be as beneficial for this case. Could the authors comment and moderate their conclusions?

Line 451: Why choose 185 m/s? You have a map of Vs30, so why not use it?

Line 462: I was wondering how the transition between this model and the previous one impacts the shape of the Vs profile, especially when stopping at 50 m. Does it create an artificial Vs contrast that may affect the site response analysis? Part of it is discussed later; a reference to this discussion here would be beneficial.

In Figures 8 and 9, do the authors use Vs30 or Vs30*? It is unclear to me when Vs30* values have been used and how much they deviate from the original ones.

Figure 9: Why does the figure not go up to 1.75 Hz?

A comparison with an alternative generic soil profile dependent on Vs30, such as that of shi and Assimaki, would have been the only way to truly test the positive impact of this study.

When we look at Figure 9, the improvements in fit are not as marked as the authors suggest in their discussion. Please moderate the conlcusions.

Round Two: Editor's Decision Letter

Dear Alex Grant, Erin Wirth, and Ian Stone:

I hope this email finds you well. I have reached a decision regarding your submission to Seismica, "A Soil Velocity Model for Improved Ground Motion Simulations in the U. S. Pacific Northwest". Thank you once again for submitting your work to Seismica.

Based on reviews I have received, your manuscript may be suitable for publication after some revisions.

I have received feedback from two additional reviewers regarding your manuscript. Both seem generally optimistic about the work and its suitability for publication in Seismica. It is also true, however, that the reviewers feel that the manuscript would benefit from additional clarification and/or explanation of several of the key points and/or data sets. I believe the reviewer comments on this front are sufficiently clear that I will let them speak for themselves without additional summary. I look forward to seeing a revised verison of this work after you have had a chance to fully consider the reviewer comments.

Also recall per my previous decision letter that we will not be able to complete the publication process for this manuscript until the Worth et al in press data set is published and available. Please provide an update on the anticipated timeframe of that process in your revision letter.

When you are ready to resubmit the revised version of your manuscript, please upload:

A 'cleaned' version of the revised manuscript, without any markup/changes highlighted.

A pdf version of the revised manuscript clearly highlighting changes/markup/edits.

A 'response-to-reviewers' letter that shows your response to each of the reviewers' points, together with a summary of the resulting changes made to the manuscript.

Once I have read your revised manuscript and rebuttal, I will then decide whether the manuscript either needs to be sent to reviewers again, requires further minor changes, or can be accepted.

If you deem it appropriate, please check that the revised version of your manuscript recognises the work of the reviewers in the Acknowledgements section.

Please note that Seismica does not have any strict deadlines for submitting revisions, but naturally, it is likely to be in your best interest to submit these fairly promptly, and please let me know of any expected delays.

I wish you the best with working on the revisions. Please don't hesitate to contact me with any questions or comments about your submission, or if you have any feedback about your experience with Seismica.

Kind regards,

Randy Williams

Author Response to Reviewers F and G

Attached are our revisions to 'A Soil Velocity Model for Improved Ground Motion Simulations in the U. S. Pacific Northwest' and responses to reviewer comments. Throughout we have attempted to be responsive to the critiques and needs of the reviewers to improve this manuscript. In addition to requested changes, we have also thoroughly gone through the text and made minor editorial changes. We hope that this is a useful contribution to the Cascadia special issue of Seismica and our community's ability to model shallow soil velocity structure to improve ground motion simulations and hazard analyses throughout the Pacific Northwest.

Responses to reviewers are in red.

Reviewer F:

The manuscript presents a Pacific Northwest Soil Velocity Model (SVM) developed to improve ground motion simulations by incorporating near-surface velocity structures. Using 649 Vs profiles compiled from Ahdi et al. (2017), WA DNR (2021), and Friedman Alvarez et al. (2024), the authors classify profiles into four geologic domains. The study fits depth-dependent Vs profiles using log-linear or linear relationships (Equations 1–4), constraining parameters by Vs30 and Vs100. Compared to existing models, the new SVM shows reduced RMSE and better agreement with measured profiles. The authors test the SVM by merging it with the Cascadia Velocity Model (CVM) and simulating the 2001 M6.8 Nisqually earthquake. The CVM+SVM simulations improve agreement with recorded high-frequency ground motions (≥0.5 Hz), especially at low Vs30 sites.

Compiled Review Questions and Comments

- **Figure 1** – Geological groups differ between data sources (Ahdi et al., 2017 vs. WA DNR, 2021); for example, some areas are red (fill/alluvium) in one and white (Puget Lowlands) in the other. Is this due to map resolution, classification criteria, or data uncertainty?

These differences are due to the map resolution of the figure. Each profile classification by the original authors was manually checked and where necessary reclassified based on 100k scale geologic maps by Washington and Oregon state to delineate bedrock (other), Puget lowland, Willamette Valley, and alluvial/fill sites.

- **Figure 1** – What are the solid black lines?

Thin black lines are mapped faults to provide tectonic context (described in the caption) the thicker black line is the extent of the Puget lowland used in this study as labeled in the figure.

- **Figure 1 and Line 142, and other content** – The "other" category appears to include a wide variety of units; some are in the coastal range, and others seem to be in the Columbia Basin. Does grouping them together introduce extra uncertainty? Since filtering is applied to exclude rock sites, I assume the "other" category refers to "other sediments." ?

We currently discuss this in the Geologic Background (196 – 206) and Discussion (566 – 576) and have added additional text to the Discussion to further clarify the issues of our definition of 'other' and its impact on model performance and uncertainty. This grouping of disparate soil types into 'other' introduces additional uncertainty, but we have made this decision to avoid fitting more precise categories with very small amounts of data. For example, a model with low variance could be made for a handful of sites coastal Washington of alpine glacial outwash. However, this low model uncertainty may mislead users applying this model far from existing data as the limited inputs may miss the true variance of velocity behavior in these soils. While we expect that the overly broad 'other' category should at least capture the full range of profiles expected for a given Vs30 at the loss of specificity.

- **Equations 1 and 4** – Please provide more explanation on why fill and alluvium follow a different simpler functional form.

The explanation given in lines 300-301 has been expanded to more explicitly note the highly linear relationship between Vs and Depth observed in this geologic domain. We note in the text many functional forms were used in the development of these models and selected the most appropriate model for each geologic domain.

- **Equation 5** – You state that uncertainty is a function of depth, but the equation expresses it as a function of Vs. If the equation is correct, does Vs represent the predicted velocity profile? Please clarify.

This should not have included 'as a function of depth'. Uncertainty in predicted vs scales with the magnitude of velocity (as shown in Eq 5), and therefore increases with depth, but we are not actually solving for uncertainty as a direct function of depth. We have corrected this sentence.

- **L332** – What causes the output profile Vs30 to be biased relative to the input Vs30? Is it due to the functional form, depth extrapolation, or fitting constraints?
- **Equation 6** – Does this correction affect the other parameters *a–d* as well? Since Vs30 should be calculated from the velocity profile, please clarify.

[lumping these two comments together as they are on the same issue] This is a limitation of our model fitting constraints. By imposing physically realistic values on fitted model parameters (e.g., the Vs at ~0m depth must be positive) to each profile, and then fitting generalized parameters as a function of vs30 for groups of profiles, we introduce a slight bias between the input/desired vs30 and the unmodified output. The correction factors (*d*) do not affect the other parameters (*a*-*c*) as this is just a linear shift of the input Vs30 value without changing the shape of the desired profile. Alternative functional forms or modeling constraints could avoid this intermediate step entirely, but we still found this approach to give the best estimates of soil velocities as a function of depth in the PNW given the available data.

- **Figures 3 & 4** – Could you add residual histograms to show the distribution and determine whether any depth-dependent bias exists?

Figures 3 and 4 plot all residuals for modeled data as a function of depth with the intention of being fully transparent about the potential depth-dependent bias that does or does not exist in these data. We specifically describe the slight depth dependent bias we observe at shallow depths for Puget Lowland sites (figure 3c, text lines 386 – 392) specifically related to a subset of the profiles collected by the Washington DNR. We do not believe histograms would improve the visualization of depth-dependent errors and we are already providing the reader all of the data to make their own interpretations as well as directly addressing this in the text.

- **Figure 5** – What does the shaded area represent? Is it uncertainty (±1σ) or measured data variability?

±1σ **of uncertainty, as described in the figure caption as "**Grey shading shows the one standard deviation uncertainty in the predicted SVM."

- **L415 & Figure 6** – The current misfit calculation (sum of absolute residuals) may mask depth-dependent bias. Please consider adding a depth-dependent bias analysis.
- The histograms in Figure 6 suggest possible overprediction, as residuals are summed across depths, which could conceal systematic bias.

[Combining these comments as they are on the same topic] With above, figures 3 and 4 explicitly show residuals as a function of depth to allow readers to see potential depth dependent bias as well as our direct discussion of this. We believe this section of text and Figure 6 is appropriately using sums of residuals to score overall performance. The plotted values are also strictly positive by definition, so systematic over- or -under prediction would appear the same in this figure. Rather than suggest overprediction, Figure 5 reflects the spread of errors for all profiles when applying this model (essentially the quality of this model relative to alternatives for the PNW).

- **L452 (accuracy)** – Geyin and Maurer (2023) released a national Vs30 map, not a regional one. Please revise the text to clarify that you used the regional portion.

We extracted the Pacific Northwest region from the Geyin and Maurer model 1. We have revised this sentence to remove the potential implication of using some other 'regional' model and clarified which version of their model was used (expanded on below).

- **L452 (methodology clarification)** – Which version of the Geyin and Maurer Vs30 map did you use, with or without topography as a predictor? Given that their machine learning model sometimes produces scattered or unreasonable Vs30 values (possibly due to overfitting), why did you choose this Vs30 map over the USGS National Vs30 map?

In the revised manuscript, we clarify that we use “model 1”, which includes mapped surface geology and is preferred by Geyin and Maurer (2023; i.e., over model 2, which does not include mapped surface geology). Geyin and Maurer (2023) also provide a “model1alt” map, which attempts to address the authors’ judgement that model 1 could overpredict Vs on steep slopes. However, Geyin and Maurer (2023) note that the performance of model1alt is slightly worse than that of the original model 1. For these reasons, we opted to use the original model 1.

Recommendation: Revisions Required

Reviewer G:

This paper proposes a Vs profile model adapted to the The Pacific Northwest (PNW) of North America area for shallower depths with lower shear wave velocity. The objective is to model ground motion for a specific earthquake and to take into account the site response at least up to 1.75 Hz. I found this study interesting in the cope of the journal and original as it is a specific application of a nex Vs model, but it requires additional work to fully demonstrate the benefits of this specific Vs model for the ground motion modelistation and in general the authors should provide a deeper analysis of the figures and tables. Please find below some comments that should be considered before publication:

Line 271: When using Vs100 from the generic PNW soil model to constrain their Vs profile, did the authors compare the value of Vs100 to those in their database, even if it is limited. Can it be shown or at least mentionned ?

Very few of the profiles used in this work extend to 100m, so it is not possible to produce representative statistics of observed Vs100 to compare to the Stephenson CVM values. A motivation for this work is in the service of modeling soils for incorporation to the existing CVM, so we feel it is appropriate to use values from that larger velocity model to tie shallow soil velocities into the much larger geologic velocity model.

Line 272: Please provide a brief description of the method used in Ahdi et al. (2017).

The Ahdi et al. paper is a compilation of Vs data and we feel is sufficiently described and appropriately cited throughout this work. No part of this work uses methodology developed or employed by Ahdi et al. that is non-standard or felt necessary to redefine within this text. The authors refer the reviewer to that work for full details of how those authors compiled their dataset.

Line 329, Table 2: Can the authors comment on the results obtained? Comparison of sigma with alternative Vs profile models?

These results are discussed in the context of alternative Vs profile models in Figure 5, Figure 6, and Table 3, as well as in the text of the results and discussion section.

The authors mention the work of Wirth (in press), which appears to be a companion paper to this one. However, as it is not yet available and part of this article relies on the results of that report, I suggest that the authors provide more details on the methods. Besides, it is a report, not a research paper.

The Wirth et al. open-file report is in press (DOI inactive until published):

Wirth, E.A., Grant, A.R., Stone, I.P., Stephenson, W.J., and Frankel, A.D., in press, Three-dimensional seismic velocity model for the Cascadia Subduction Zone with shallow soils and topography, version 1.7: U.S. Geological Survey Open-File Report 2025-1045, https://doi.org/10.3133/ofr20251045. [Supersedes USGS Open-File Report 2017-1152.]

Like journal publications, USGS open-file reports undergo internal and external peer review. Open-file reports are a common format for the publication of 3-D seismic velocity models in Cascadia (Stephenson et al., 2007; Stephenson et al., 2017).

Figure 2: The uncertainties in the predicted Vs should be shown.

Adding four sets of uncertainty ranges to this figure would render this figure difficult to interpret. All predicted uncertainty ranges are provided in this work (Table 2) and we show an example profile with uncertainty in Figure 5 to provide the reader with a sense of scale of predicted uncertainty.

I do not understand the mention of a Vs30 equal to 400 m/s because all the predicted models seem to have very different Vs30 values. I also do not understand why the authors use a Vs30 of 185 m/s for the alluvial. Please provide more details.

These details are provided in lines 368 – 377. Alluvial sites are not plotted for 400m/s because that condition does not exist in these data, so we plot the Vs30 value used in the later simulations. 400m/s was used for all other geologic domains as it is a generic reference condition. While each site has a different Vs30, we just wanted to use this simple figure to show the difference geology makes on the shape of Vs as a function of depth for a fixed reference Vs30.

Line 379: Could the authors provide numbers to illustrate the uncertainty?

This line imprecisely made a reference to ‘uncertainty’ and has been revised to more correctly state these values are within the ‘range’ of the model estimates. We provide more formal uncertainty values in Table 2, as well as showing model uncertainty for a predicted profile in Figure 5.

Figure 3: It would have been beneficial to have an evaluation of the resonance frequencies of these profiles, even in 1D, because the following models stop at 1.75 Hz. Do these Vs profiles have site amplification below 1.75 Hz?

We assume that the reviewer is referring to amplification at frequencies above 1.75 Hz, since the frequencies below 1.75 Hz were accounted for in the 3-D simulation. (The 1.75 Hz cutoff was chosen strictly due to computational resources available for the 3-D wave propagation simulations.) For regional velocity models in southern California, Shi and Asimaki (2018, Figure 7) showed that amplification factors as a function of frequency were much more comparable between measured soil profiles and soil profiles predicted by a soil velocity model, compared to the original community velocity model profiles. An extensive 1d analysis of alternative profiles to assess high-frequency response is beyond the scope of this work but could be assessed by future researchers as necessary.

Lines 424 and Figure 6: Figure 6 is not enougth described and discussed. When I look at the histogram of the Puget Lowlands, I have the impression that the yellow model provides a lower RMSE compared to this study. I guess if we have to look at a different metric, such as the median instead of the sum of RMS, the proposed profiles may not be as beneficial for this case. Could the authors comment and moderate their conclusions?

The RMSE value for the yellow (generic PNW model) is provided in this figure (116) where the proposed SVM has a lower RMSE of 110 for the Puget Lowlands. These values and sum of absolute residuals are repeated in Table 3 showing the consistent better performance for the proposed SVM. The visual discrepancy is likely due to the lowest single histogram bin for the Puget Lowlands being greater (more low-error profiles) than the proposed SVM. However, overall performance is still better for the proposed model in this work for the Puget Lowlands and other considered geologic domains.

Line 451: Why choose 185 m/s? You have a map of Vs30, so why not use it?

For the Nisqually earthquake simulations, we utilized detailed local maps of fill and alluvium from Frankel et al., 2007 (fig. 26) that do not neatly align with the model of Geyin and Maurer estimates of Vs30 (for example, some areas of known mapped fill are modeled with high (~400m/s) Vs30). Because these maps were developed completely independently and the occasional mismatch, we defaulted to an average value for Vs30 for fill and alluvium sites (185) using the map developed by K. Troost for Frankel et al. (2007).

Line 462: I was wondering how the transition between this model and the previous one impacts the shape of the Vs profile, especially when stopping at 50 m. Does it create an artificial Vs contrast that may affect the site response analysis? Part of it is discussed later; a reference to this discussion here would be beneficial.

As mentioned, in the discussion we note the concern of potential artificial impedance contrasts and the need for further data collection to accurately reflect velocity structure at depths from tens of meters to a few kilometers. We do not feel it is necessary in this methods section to link to the discussion directly.

We included the velocity of the larger CVM at 100m depth in the fitting of soil velocity profiles to mitigate the potential for large artificial impedance contrasts, as well as including as many profiles as possible with high-fidelity data to several hundred meters to capture the intermediate depth signature of Vs in the Pacific Northwest. While care was taken not to create artificial impedance contrasts while model merging, without additional data collection and verification of these models we cannot accurately say if any of these impedance contrasts are 'artificial', or incorrect, but they could affect site response calculations. We note the combined model is compared to real ground motion recordings in this work showing a good improvement to not including the SVM, so overall any imposed impedance contrasts are reflecting some degree of real behavior.

In Figures 8 and 9, do the authors use Vs30 or Vs30*? It is unclear to me when Vs30* values have been used and how much they deviate from the original ones.

Figures 8-9, and all of the simulation results, use Vs30* when predicting the soil velocity model that was used to generate the simulations. We have further clarified this in the text of both the results and application to the 2001 Nisqually earthquake questions.

Figure 9: Why does the figure not go up to 1.75 Hz?

In Figure 9, we did not show frequencies up to 1.75 Hz because the Fourier spectra were averaged across an octave range of frequencies (i.e., in order to smooth the spectrum and achieve a more representative value around each frequency of interest). In the revised manuscript, we have made this more clear in the caption for Figure 9. In addition, we have updated Figure 9 to go up to 1.2 Hz; beyond this frequency, an octave bin would go beyond our fmax of 1.75 Hz.

A comparison with an alternative generic soil profile dependent on Vs30, such as that of shi and Assimaki, would have been the only way to truly test the positive impact of this study.

Shi and Asimaki are cited throughout and discussed in the context of this work (see Figure 5 where we plot their predicted profile next to ours). That model is for southern California soils and does not perform as well in the Pacific Northwest. We felt it was unfair to those authors to overly compare their work to these data as it is for a different region and does a very good job for the region it was developed in. However, we do compare this work to models developed for generic PNW soils and attempt to make a fair and direct comparison you suggest. See Figures 5, 6, and Table 3 where generic PNW models that are dependent on Vs30 are compared to the proposed SVM to test the positive impact of this study.

When we look at Figure 9, the improvements in fit are not as marked as the authors suggest in their discussion. Please moderate the conlcusions.

Figure 9 was completely remade and improved in response to the first *Seismica* review, which are echoed in your concerns of the original version. The revised version of Figure 9 averages results from stations that are in the same Vs30 "bin" (i.e., to avoid the overplotting of data points). The revised version of Figure 9 clearly shows that the CVM+SVM performs better (blue colors) than the original CVM. For nearly all frequency-vs30 pairs, we find a reduction in bias (improved fit to real data) when using the combined SVM+CVM model.