Review for manuscript

<u>Volcanic eruption tremor from particle impacts and turbulence using conduit flow models</u>

By

<u>Katherine R. Coppess, Fredric Y. K. Lam, Eric M. Dunham</u>

Round 1

We thank the two reviewers for their thorough and insightful reviews. Below we address all comments. The reviewer comments are listed in bold and our responses in regular text.

In addition to revisions prompted by the reviewers' comments, we also made some small changes to update the notation in Table 1 related to the conduit flow model for consistency with a manuscript we will soon submit on the conduit flow code (and another group member's PhD thesis that we now cite). Figures 1, 4, 6, 7 and 9 were updated in response to reviewer A's comments that prompted us to fix a few mistakes in our code to compute PSDs. Figure 8 was also updated because of a small modification to the conduit flow model used in the waning eruption simulations that led to slight differences in the conduit flow solution.

Reviewer A (Julia Gestrich)

I enjoyed reading this interesting work which is an important continuation of the model developed by Gestrich et al., 2020. The work is well structured and tackles two important additions in complexity: the inclusion of the full Green's function and the depth dependence of parameters. It does well explaining the importance of these changes and implications for the interpretation of seismic signals. I recommend to publish this work in the future but would like the authors to consider the comments detailed in the revision that I attached below as well as the annotated manuscript. I possibly found some mistakes in the code which might change the result and figures of this paper. Those are detailed in the attached revisions in section 4. I further want to draw the author's attention to the erratum of the paper by Gestrich et al., 2020. I am looking forward to receiving this paper again and seeing it being published in the future.

We thank Dr. Gestrich for her very thorough comments and especially for providing a detailed derivation (which we confirm is correct, in contrast to what we had originally stated in our manuscript) and for reviewing our source code and finding two bugs in it.

1 General Remarks

The presented paper shows a well-thought-through and logical continuation of the work in Gestrich et al. (2020). They focus on two aspects to add complexity to make the models more realistic. The first one is the use of the full Green's function (as opposed to only the Rayleigh wave Green's function as used in Gestrich et al. (2020)). The result shows that for higher frequencies the use of the full Green's function is required to have a better fit to eruption seismicity data. The second aspect they add is the depth dependency of certain parameters. They find that the use of averaged values underestimates the seismic energy compared to depth-dependent parameters. Overall this paper is an important step in making the model more realistic and adding important complexity without trying to over-fit the data. I believe this paper will be important to many scientists in helping with their interpretation of collected eruption seismicity data and offers an improved base to build upon more complex models for different eruption types and flow conditions.

Although I do recommend publishing this paper in the future I want to draw attention to some important points that will need to be improved upon. As detailed in section 4, I found two mistakes in the code used by the authors for part of their paper and I fear that these mistakes might have contributed to some of the discrepancies observed between this paper and Gestrich et al. (2020) and might also influence their final results. Therefore, I suggest checking the equations again and making sure that the data in the figures use the correct factor. Furthermore I want to draw attention to the erratum for Gestrich et al. (2020). The changes are included in the online version of the paper but the PDF version could not be changed and has an erratum at the end.

In the following I will go into more depth discussing different parts of the paper including the full Green's function, the parameter variation with depth (and grainsize discussion), and finally also the claims the authors make about potential mistakes in Gestrich et al. (2020).

This is an accurate summary of our work. We also apologize for overlooking the online erratum to Gestrich et al. We were using the pdf version of the paper as our reference, which didn't have the corrections. Below we address the concerns that were raised.

1.1 Organization of the Paper

I generally like the organization of the paper and the explanations are easy to follow and logically structured. My only point would be regarding the section naming and structure of section 3 "Model Modifications". In this section, they explain that there are two main modifications (full Green's function and depth-dependent parameters) but only go into detail about the Green's function and the assumption of the extended source. I would suggest adding the modifications in terms of depth-resolved parameters (as is being done in section 4 (before 4.1) and in 4.1 in the first two paragraphs) to section 3 or renaming the section to reflect that only the Green's function modification is being discussed.

Section 3 explains both of the modifications (full Green's function and depth-dependent parameters), so it would not be correct to rename section 3 to focus exclusively on the Green's function modification. However, we agree that section 3 does not provide details on the conduit flow model used to obtain the depth-dependent parameters, which probably contributed to the confusion. To address this, we have added statements at several places in section 3 explaining that the depth-dependent parameters are obtained from a conduit flow model, and that details of the conduit flow model will be provided in section 4.

In addition, we now start the two parts of section 3 with opening sentences beginning with "The first modification to the PIT model is…" and "The second modification to the PIT model is…" to help the reader better understand the organization of this section.

2 Green’s Function

The use of the full Green’s function seems to highly improve the capability of the model to replicate the shape of the eruption PSD of the eruption at Pavlov volcano for higher frequencies which is a fantastic finding and addition to the model. However, I am missing some more description and derivation of the full Green’s function. The authors cite Zhu and Rivera (2001) and say that they use their numerical frequency-wavenumber approach even though the cited paper does not call their solutions Green’s functions which might be confusing to the reader. I understand that a numerical solution does not have an easily written down analytical expression but some explanation of the method and their assumptions would be useful for the reader to also re-create the model and use it.

We have added the following sentences explaining the relevant properties of the method: “This is a widely used numerical method for solving the elastic wave equation when properties vary only with depth, as is assumed in this study. The method provides the full Green's function, including body waves as well as surface waves.” These sentences do not explain how the Green’s functions are obtained, as that is not important to the present study (and those details can be found in the Zhu & Rivera paper). Instead, we point out what the reader should know to understand our results, which is that the Green’s functions include body and surface waves.

The full Green’s function could be computed in other ways, for example using finite difference, finite element, or spectral element codes. The results of our study would be unchanged because the Green’s functions would be the same, so the particular numerical method used to obtain the Green’s function is not important.

Finally, regarding terminology, the title of the Zhu & Rivera paper is “ A note on the dynamic and static displacements from a point source in multilayered media.” The Green’s function for the elastic wave equation is defined to be the displacements caused by a point force with delta function time dependence, which is exactly the problem solved by Zhu & Rivera.

Furthermore, the authors subsequently call the result the “Lesage-Boore-Joyner” Green’s functions without any explanation as to where this expression comes from. Is it the numerical solution after Zhu and Rivera (2001) is meant with the velocity profile following Lesage et al. (2018)? Where does the “Boore-Joyner” part of the expression come from?

As explained in section 3, the P- and S-wave velocity model is from Lesage et al. They do not provide density, which is needed to solve the elastic wave equation for the full Green’s functions. We use the Boore & Joyner empirical relation between density and S-wave speed to obtain density, as stated in equation (14) and the sentence before it. Thus it seems appropriate to call the Green’s functions associated with this elastic structure the full Lesage-Boore-Joyner (LBJ) Green's functions. At the end of the paragraph explaining the velocity model and density profile, we added a sentence that defines our terminology: “We refer to Green's functions based on these assumptions as the full Lesage-Boore-Joyner (LBJ) Green's functions.”

Because the Green's functions would be the same if we computed them using a different numerical method (but the same elastic structure), it is not appropriate to include Zhu & Rivera in the name of the Green's functions. Of course, we acknowledge Zhu & Rivera when stating that we used their numerical method (and code).

3 Parameter Variation with Depth

The variation of parameters with depth is an important point to investigate and the approach by modeling conduit conditions independently as input parameters makes sense to explore their influence. They show that with the modeled parameters profiles, the averaged values would underpredict the seismic energy. However, I am missing a comparison of some different scenarios and how they would differ from an averaged case. For what kind of eruptions does it matter to resolve the depth variations and for which eruptions is it quicker and easier to assume averaged values?

This is an interesting question but it is nearly impossible to answer without running a huge number of conduit flow models on different eruption styles. The specific conduit flow solution that we use has features similar to most other conduit flow models of explosive eruptions of magmas with more than ~1 wt% volatiles, in that between fragmentation and the vent, the velocity increases by at least one order of magnitude, and density and particle volume fraction drop by about an order of magnitude. Eruption of low viscosity magmas like basalt and/or magmas with minimal dissolved volatiles might have less depth variations of these fields (and at some point, the magma might not even fragment). That said, some of the integrands in the PIT model involve large powers of the fields (e.g., velocity cubed in the impacts model), so even a factor of two variation in velocity would lead to nearly an order of magnitude change in the tremor contribution, for example. So maybe even for these eruptions, it would be important to account for depth variations. However, because we have not studied these eruptions, then we do not feel comfortable speculating about when it is appropriate to neglect depth variations in fields.

The authors separate a discussion about varying grainsizes into a different sub-section as the model used in the previous section does not predict grainsizes. As the authors explain well, the grainsizes in the conduit are unknown and very difficult to predict. They continue to use three different potential depth variations of grainsizes to discuss the influence of them. This is an interesting approach. However, I am missing a discussion about the term "representative grainsize" introduced in Gestrich et al. (2020). This term describes the grainsize that represents a whole grainsize distribution in terms of their seismic energy which is [*see equation in reviewer's pdf*]**. This representative grainsize is always on the upper end of the grainsize distribution as explained and shown in S7 in Gestrich et al. (2020) and not the average or mode of the grainsize distribution. Does the consideration of this term change the interpretation and discussion of the results?**

We defined the representative grain size following equation (5); however, this is just a mathematical definition that is easy to overlook. We agree that it is important to point out the difference between representative grain size and average grain size. We have modified the manuscript to do this in two places: first, following equation (5) where representative grain size is defined; and second, at the start of section 4.2 that explores different profiles for the depth-dependence of grain size. Our statements emphasize that representative grain size can be several orders of magnitude larger than average grain size.

We still feel that our interpretation and discussion of the results is accurate, specifically our conclusion that extreme parameter values are needed to achieve consistency with the observed tremor amplitudes. The only eruption in the compilation featured in Figure S7 of Gestrich et al. that has a representative grain size greater than 0.1 m is the 2008 Kilauea eruption, which is a very different eruption style than modeled with our conduit flow simulations. Furthermore, most of the large ejecta were not sourced by fragmentation in the conduit, as stated in the abstract of the paper that provided the Kilauea data: “the grain size of 19 March 2008 clast population [referring to the largest ejecta] is unrelated to a volcanic fragmentation event and instead was “inherited” from a population of talus clasts that temporary blocked the vent prior to the eruption” (Houghton et al., 2017). If this is correct, then those large ejecta could not have generated seismic tremor via the particle impacts mechanism and should be excluded from the calculation of representative grain size.

I agree with the authors’ assessment that there could be an interdependence between the parameters that could be varied to result in the same mass eruption rate which was not accounted for in Gestrich et al. (2020). They conclude that ultimately the grainsize needs to change to produce the same MER with different seismic energies. However, I am missing the explanation of how the exact variables responsible for the MER (e.g. density, velocity, and radius) are all interdependent and therefore there is a unique combination of parameters that explains a certain MER.

The nonlinearity of the governing equations for the conduit flow model prevents any simple relation between MER (or the fields that determine it, i.e., density and velocity at the vent, which are output from the conduit flow model) and some of the input parameters to the conduit flow model (e.g., chamber pressure and conduit radius). The covariation of the fields that determine MER can only be obtained by solving the conduit flow equations. One way to quantify this would be to run a set of simulations in which certain input parameters were varied systematically (e.g., chamber pressure and conduit radius) and the MER and other output parameters were calculated. One could then find the combinations of the input parameters that produce some specific MER. In fact, one could even formulate an inverse problem to determine the input conduit flow model parameters that best matched observational constraints (e.g., MER, eruption tremor PSD). However, this is beyond the scope of our study and the parameter space that would need to be explored would be quite large (as it would include magma viscosity, fragmentation criterion, volatile content, etc., in addition to chamber pressure and conduit radius). But this line of research is moving toward inversions, and if those were done in a Bayesian manner, then it would address this covariation issue.

I like their observation of different points in frequency and time when the turbulence or impact model shows higher frequencies. This could be expanded upon to underline what exact factors determine the crossing point in frequency and time.

After correcting our code (to address the mistakes that were identified by the reviewer), the transition between turbulence and impact contributions as the eruption wanes does not occur. Therefore we have removed the final paragraph of section 4.3 that discussed this transition.

4 Mistakes in Gestrich et al. (2020)

4.1 Claim: Wrong Factor in Equation 29

The factor in equation 29 in Gestrich et al. (2020) is correct (as shown in the appendix). However, after looking into the python functions in the github repository provided I noticed a possible source of the difference between the turbulence PSD by Gestrich et al. (2020) and the one shown in this paper: In "modular PSD.py" there is a factor called "FGjz2" (the summed Green's function and impact force) in the function "turbulence PSD" which is:
[see equation in reviewer's pdf]
However, in equation 17 in Gestrich et al. (2020) for the turbulence spectrum they follow Gimbert et al. (2014) (equation 6) and assume that forces in different directions act independently from another which means we can write this part as:
[see equation in reviewer's pdf]
This factor should make the difference in the resulting PSDs.

Many thanks for the reviewer's extremely careful review! We agree that equation (29) in Gestrich et al. is correct, so we have removed all statements claiming otherwise. The reviewer is also correct that there was another bug in our code (we were summing the Green's functions, then squaring them, rather than summing the squared Green's functions as is appropriate when the contributions are uncorrelated). The additional revisions to our manuscript are explained after the next comment.

4.2 Claim: Wrong Parameter Inputs

As explained in the erratum of Gestrich et al. (2020) we indeed missed to vary the parameters df and Db in Figure 10 of Gestrich et al. (2020). However, their influence on the maximum PSD is rather minor as explained in section 4.2 and Figure 6 in Gestrich et al. (2020). The corrected figure is shown in 1. As one can see, the maximum PSDs from both the turbulence and impact PSD are both slightly higher. However, this does not change the fact that the turbulence PSD alone under predicts the observed PSD signal during the eruption of Pavlof volcano.

Furthermore, I noticed a mistake in the python functions in the provided github repository in calculating the mass of the impacting particles in the function "impact_PSD" in the "modular_PSD.py" file as they use the diameter of the particle instead of the radius.

We also fixed the diameter vs radius mistake in our code. After fixing these mistakes, we reproduce (the corrected online version of) Figure 10 in Gestrich et al., which appears in our Figure 1. We updated the description of that figure accordingly. These updates also affect several other figures and results in our paper, specifically Figures 4, 6, 7, and 9. However, the interpretation and discussion of our results remains the same, with the exception of the transition between turbulence and impact contributions as the eruption wanes (so we removed that paragraph discussion from the end of section 4.3 as explained above).

Reviewer B

This study expands a previously published model of Gestrich et al., (JGR 2020) that proposes an eruptive tremor source mechanism arising from particle impacts and turbulence within the conduit. It assumes that magma is fragmented within the conduit, i.e., gas fraction is above a critical fraction at which the liquid matrix breaks apart, changing the volcanic flow into a particle-laden gas-rich flow.

The model improvements proposed in the present study are twofold:1/ Replace the Rayleigh-wave Green's functions by Green's functions describing the full wavefield,2/ Replace the point source assumption by a vertically extended source enabling for depth variations in the properties of the volcanic flow.

Following these improvements, the model predictions show better agreement with observations during the 2016 Pavlof eruption. In particular, the use of full Green's functions allows to properly model body-waves that significantly contribute to the high-frequency part of the eruptive tremor spectrum. Extending the model to allow for depth variation does not seem to improve the fit of the observations. However, it allows the author to use a more realistic conduit flow model, showing that tremor sources are much shallower than the depth of magma fragmentation (i.e., within the top 500 m of the conduit) .

This is an accurate summary of our work. We address your comments below.

This is a very good study, which looks appropriate for Seismica. I have a few minor comments, none of which should be difficult to address in a revised manuscript.

Questions and comments:
1/ From what I understand, the model assumes a vertical conduit, which is a simplistic assumption of conduit geometries in volcanoes. One might wonder if and how the tremor spectrum would be affected by assuming a tilted conduit.

Probably not much. What might be more important is a non-cylindrical conduit geometry. We have added a brief discussion about this to the next-to-last paragraph in section 3.

2/ The predicted tremor spectrum is much flatter when using full numerical Green's functions compared to Rayleigh wave Green's functions. As authors suggest, this is likely due to the increasing importance of body waves relative to surface waves at high-frequencies. Is there a way to prove this more clearly? For example, by looking at particle motions in different frequency bands?

Figure 3b compares the vertical velocity PSDs for Rayleigh wave and full Green's functions. The elastic structure is identical, so the differences must be attributed to body waves. Yes, one could investigate this further by examining particle motions in different frequency bands as the reviewer suggests, but this doesn't seem necessary to support our statement. Furthermore, it would be difficult to apply the quantitative results from such an analysis to real data, due to the lack of scattering in our plane-layered elastic structure. Scattering is pronounced at volcanoes and would alter the particle motions (and probably convert between body and surface waves). A study of particle motions only seems warranted if we added scattering heterogeneities to the elastic structure, but that would require a different code for computing Green's functions as well as some means to separate scattering attenuation from intrinsic attenuation when selecting model parameter choices. We prefer the simpler approach taken in the manuscript, which focuses on vertical component PSD, and uses attenuation (or Q) values that are common for volcanic environments and which represent both intrinsic and scattering attenuation.

3/ One of the results presented in the manuscript is that particle impacts are the dominant tremor source at high-frequency. As eruption rates decrease, particle impacts become dominant at lower and lower frequencies. For very small eruption rates, the authors extrapolate that the particle impacts become dominant over the entire 1-5Hz frequency range. It would be nice to show this more formally by updating Figure 9 for a very low mass eruption rate.

After fixing the mistakes in our code that were pointed out by the other reviewer, this transition does not occur. We removed the final paragraph of section 4.3 that discussed this. Given this, there would not be much learned by adding another mass eruption rate simulation – and switching from 3 to 4 simulations would make the figures too busy.

4/ It might be instructive to supplement Figures 1, 4 and/or 6 with a figure showing the ratio between (m)Pit Turbulence and (m)Pit impacts spectra. This would clearly show which is the dominant source of tremor at different frequencies.

This is also no longer relevant.

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Round 2

We thank the reviewer (Julia Gestrich) for her additional comments, which we address below. The comments were provided in an annotated manuscript, so we reproduce them as best possible.

1. Our paper opens with “Volcanic eruption tremor is a universally observed seismic signal from explosive eruptions within the 0.5-10 Hz frequency band. It is characterized by its **coincidence with explosive eruptions** and its incoherence…” The reviewer asked if the text in bold was a repeat of the previous sentence. We rephrased the opening to read “Volcanic eruption tremor is a seismic signal that is universally observed during explosive eruptions. It occurs within the 0.5-10 Hz frequency band and is characterized by its incoherence…”

2. Our paper reads “Comparing the two mPIT results, using the depth-averaged values underestimates the spectral power by a few dB, with greater impact at lower frequencies.” The reviewer requested, “I would prefer if you stated rough dB variation numbers to give readers an idea of the impact of the changes.” We did not make any changes, as this sentence does provide rough dB variations (“a few dB”), and readers can see more details in Figure 6. That figure does show what we claim in the sentence, but it would be difficult to be more detailed than we have already been, given that the differences vary with frequency and there are even some frequencies (but not a lot) where the spatially uniform properties model has the same or even greater spectral power.

3. Our paper reads “Gestrich et al. (2020) found that one of the input fields that had the greatest impact on the tremor PSDs through the particle impacts force spectra was representative grain size. The representative grain size, defined following equation (5), is the equivalent grain size that would produce an identical seismic PSD if grain size were constant.” The reviewer notes, “This sentence only makes sense if you mention what the representative grain size should replace (which is a grain size distribution).” She suggests adding the text in bold “...equivalent grain size **for a given grain size distribution p(D)**...” which we have done. We also restated the equation definition here.

4. Our paper reads “Since we are focused on the waning period, we choose all solutions to have subsonic flow out of the vent, with the highest mass eruption rate chosen to be somewhat close to the reference solution with choked flow.” The reviewer stated, “You did not mention choked flow before. What reference solution are you referring to? Please refer to this reference solution as choked flow in the previous section when you introduce it to make sure the reader knows what is meant.” We introduced this reference solution at the end of the first paragraph of section 4.1: “In the reference case simulation, the bottom pressure is sufficiently high that flow chokes at the vent (i.e., magma is erupted out at the mixture sound speed). Other simulations used later to illustrate changes in eruption tremor as mass eruption rate decreases feature subsonic outflow, and for those simulations we set pressure at the vent to atmospheric.” To help the reader remember this, we added in bold “...with the highest mass eruption rate chosen to be somewhat close to the reference solution with choked flow **that was introduced in section 4.1**.”

5. Our paper reads, “High mass eruption rate requires high flow velocity in the upper conduit, which is hugely influential on the predicted force spectra for this tremor model.” The reviewer suggests, “Please make clear that this is the result for one scenario only. Other eruption waning scenarios could show different results.” We address this by adding the text in bold “High mass eruption rate requires high flow velocity in the upper conduit**, at least for the eruption scenarios modeled in this work**, which is hugely influential on the predicted force spectra for this tremor model.”