Solid Earth Discuss.,
doi:10.5194/se-2016-71-AC1, 2016

Interactive comment on "The imprint of crustal density heterogeneities on regional seismic wave propagation" *by* Agnieszka Plonka et al.

Agnieszka Plonka et al.

a.i.plonka@uu.nl

Received and published: 26 September 2016

[se, manuscript]copernicus color

Dear editor,
dear reviewers,

Thank you very much for your constructive comments and suggestions that helped us to improve our manuscript. In the revised version, we provide more technical details on the measurement procedure, further clarify the generation of random media and their properties, and give a more precise definition of frequently-used expressions such as "complexity". Furthermore, we corrected typos and included additional references. Please find a point-by-point response to the reviewer's comments below, alongside

with a .pdf file marking all the modifications.

With kind regards

Agnieszka Plonka, Nienke Blom and Andreas Fichtner

1 Reviewer 1

1 *"Page 5 part 15, please provide details for the moving window w(t), including shapes of window function, width, moving interval etc. Did you adapt the resolution of w(t) at different frequency band? Take wavelet transform for example, at high frequencies, narrow basis functions are used, and the moving (shift) offset also depends on scales."*

Yes, the standard deviation of the Gaussians $w(t)$ is different for each frequency band. For the highest band it is 8 s, for the medium band 15 s, and for the lowest frequency band 25 s. We changed the manuscript in section 2.3 ("Quantification of waveform differences") as follows: "In the following sections, we consider three frequency bands of variable width: 0.02 - 0.125 Hz (8 - 50 s), 0.02 - 0.067 Hz (15 - 50 s), and 0.02 - 0.04 Hz (25 - 50 s). The $w(t)$ Gaussian time windows corresponding to those frequency bands have standard deviations of 8 s, 15 s and 25 s, respectively. To stabilise the measurements,..."

2 *When I read âÂĂÂIJ2.2 Random media generationâÂĂÂİ, I had a lot of questions on how the random media are generated by combing tomographic models and empirical velocity-density relations. How rms from tomographic models is used to constrain random model generation. Did you use the ranges from*

tomographic models? etc. However, âÂĂÂIJ4.2 Random models of plausible Earth structureâÂĂÂİ provides much more details on those questions. To help readers better understand the generation of random models, I would recommend the authors show those details earlier, instead of waiting till the final discussion.

In fact, these questions raised by the reviewer are already answered in section 2.2 of the original manuscript, which describes the technical aspects of random model generation. For instance, concerning the question "How rms from tomographic models is used to constrain random model generation.", we already write the following: "Using the empirical scaling relations between crustal velocities and density of Brocher (2005), we then obtain suitable ranges for variations in P velocity and density. The resulting rms variations are 260 m/s for S velocity, 460 m/s for P velocity, and 80 kg/m^3 for density."

Section 4.2 is really more dedicated to a detailed discussion of the different, and to some extent subjective, choices that one can make in the construction of such models. In the interest of a readable and succinct text, we would very much prefer to keep the technical part (section 2.2) and the discussion (section 4.2) separated.

3 *Page 5 equation (3), a recommended way to evaluate amplitude difference is to first shift $\hat{u}_\tau$ by δT estimated in equation (2), then compare the amplitude difference of shifted $\hat{u}_\tau$ and $\hat{u}_\tau^{ref}$.* Is $ln(A(t))$ a better quantifier for amplitude difference than $A(t)$?

We agree that this is an alternative and more general approach to measuring amplitude differences of specific phases, e.g. an isolated P or S wave. In or work, we decided not to adopt this method because the time shifts are simply too small to affect the amplitude measurements in any significant way. Therefore, we preferred our simpler method that is also less computationally intense.

The logarithmic amplitude ratio is certainly not a better measurement than the pure amplitude ratio. Being related by an invertible function, they are indeed fully equivalent. Choosing one or the other is therefore a personal choice. We prefer the logarithm for two reasons: (1) It is centred around 0 for identical amplitudes. (2) It is symmetric, meaning that amplitudes that differ by a factor of two give values of $\pm ln(2)$ and not of 2 and $1/2$. It is thus a matter of making the interpretation easier.

4 *Page 6 part 10, move the sentence "Before attempting a more comprehensive analysis in the following sections..." before "Figure 3 shows a comparison of three-component...".*

Thank you for the suggestion. We made this modification.

5 *Page 6 part 15, in sentence âÂĂÂIJRelative amplitude differences are largest on the E-W and vertical components, where the displacement velocity itself is smallest so that the influence of scattered waves is largestâÂĂÂİ, do you really want to say âÂĂÂIJCompared to the N-S component, relative amplitude differences are larger on the E-W and vertical components, where the displacement velocity itself is smaller so that the influence of scattered waves is largerâÂĂÂİ ?*

We agree that this sentence is too complicated for the simple message that it tries to convey. We changed it to "Relative amplitude differences are largest on the E-W and vertical components, where the displacement velocity itself is smallest. Therefore, low-amplitude scattered waves have the largest influence on the total amplitude."

6 *Page 6 part 15, in sentence "... meaning that amplitudes for the heterogeneous density crust can be both twice and half as large as for the medium with*

homogeneous crustal density" do you mean " either twice or half as large as ..."?

Yes, it has been corrected that way.

7 *Page 7 part 30, misspell of "negative".*

Thank you. Corrected.

8 *Some suggestions on the structure of the document. It might be more compact to take âÂĂÂIJ3.1 A single-receiver exampleâÂĂÂİ as a subsection of âÂĂÂIJ3.2 The effect of frequencyâÂĂÂİ, which consists of two examples: a single-reviver, and all receivers.*

In fact, an earlier version of the manuscript had that structure. However, we found it less clear than the current version, which we would therefore like to keep.

9 *It has been assumed that all random velocity and density models used in simulations are spatially uncorrelated. In reality this assumption is not valid and velocity density variations are correlated/scaled. Could the authors make comments on how the correlations would affect the conclusions?*

First, it is important to keep in mind what "uncorrelated" really means. It means that heterogeneities in different or the same material parameter have no spatial correlation *on average*, i.e. when a large esemble of model realisations is considered. Thus, there is no point in considering uncorrelation for a single realisation of a random model. A single realisation of course has non-zero spatial correlations; and our models are no exception. It follows that the effect of correlated heterogeneities is already contained in the results.

Second, one can make a first-order argument that the extent of correlation

between density and velocity heterogeneities should have no significant effect: Considering the Born approximation, velocity and density perturbations appear as unrelated right-hand sides in the perturbation equation; assuming of course that velocity and density have been chosen as the independent parameters. Consequently, they act in isolation. Any type of correlation of these perturbations is absent in the equations and therefore does not play a role.

To make this issue clearer in the text, we added the following sentence in section 2.2 on random model generation: "The spatial variations in velocity and density are statistically uncorrelated, meaning that the spatial correlation averaged over many realisations is negligibly small. Individual realisations, considered in this work, do have non-zero correlation."

10 *Those numerical experiments left some thought-provoking implications for fullwaveform inversion, which may be biased if density variations cannot be neglected. To consider density-induced waveform perturbations in tomographic inversion, one may need to distinguish the effects from velocity and density structure on waveforms, as well as their coupling effect. Even though different phases are not separated in the analysis in this paper, the authors should understand that the effect of density heterogeneities on different seismic phases might be different.*

Thank you for this comment. Of course, we understand this aspect. In fact, this is already discussed in the introduction where we try to explain why density heterogeneities are so difficult to recover. Clearly, the traveltimes of high-frequency body waves are practically insensitive to density variations. In contrast, the dispersion of Rayleigh waves depends on shallow density structure, which has occasionally been used to put constraints on density variations. Furthermore, the long-period normal modes depend on density through self-gravitation of the Earth. In this work, we preferred to not make the distinction between different phases. This would have been a very tedious exercise, and we doubt that it would have tought us much. Also, on regional scales and a the relatively short periods that we consider, individual seismic phases are often not distinguishable anyway.

11 *It may worth to discuss the work by Yuan et al. (2015), in which density variations are updated together with wavespeed in surface-wave full-waveform inversion to lessen the bias in wavespeed inversion caused by incorrect density variations.*

Thank you for the suggestion. We added the following sentence to the manuscript: "However, depending on the tomographic resolution and data quality, the biases may be larger than the error bars. A qualitative look at the detrimental effects that incorrect density information has on the wave speed models has recently been shown by Yuan et al. (2015).

12 *To be clear, add t underneath argmax in equation (2).*

Added.

2 Reviewer 2

1 *You often talk about âÂĂÂIJmedium complexityâÂĂÂİ (P1 L9, P3 L30, P6 L6, P6 L9. . .). It is not clear what you mean by that at first. Then we understand that it refers to the correlation length of the heterogeneities: The smaller this length is, the more complex your medium is. I am not sure if âÂĂÂIJcomplexityâÂĂÂİ is the appropriate term here, because the number of parameters you need to define your random medium is the same in any case: it is just* k_{max} *(cf appendix A). IâÂĂÂŹd rather talk about âÂĂÂIJroughnessâÂĂÂİ (in accordance to the*

term âÂĂÂIJsmoothâÂĂÂİ that you introduce in P8 L20 as well as in the figure 9 captions). Whatever the name, you should define it much sooner in the paper.

We fully understand the reviewer's concern, and we see that we have not been sufficiently clear. Therefore, we added and modified the following sentences. Abstract: "Both amplitude and traveltime variations increase with increasing epicentral distance and increasing medium complexity, i.e. decreasing correlation length of the heterogeneities." Outline: "For this, we conduct a series of numerical experiments, where we analyse seismic wave propagation through random Earth models with variable complexity, i.e. correlation length scale." Introduction to section 3: "Here and in the following sections, we use 'increasing complexity' as synonymous to 'decreasing correlation length'."

2 *The heterogeneities of density impact both the singly and the multiply scattered wavefields. FWI should be able to invert the singly scattered wavefield to recover density structures at the scale of the wavelength, but it cannot handle multiple scattering yet. You should state that more clearly in the abstract (P1 L12) as well as in the conclusion (P12 L32).*

This is not correct. First of all, it is important to note that already a simple Newton method accounts for secondary scattering, thus going beyond simple single scattering. Second, multiple scattering is properly accounted for by iterating towards an optimal solution. While each individual iteration may - in the case of a first-order descent method - only account for single scattering, the sequence of iterations of course takes multiple scattering into account. This is, by the way, the case for any non-linear wave propagation inverse problem, including FWI.

3 *Fig. 3: From the last sentence of the captions, I understand that there is no cycle skipping on the Z-component coda, contrary to the NS and EW components.*

Could you explain why? This is really intriguing to me.

For the one particular receiver that we have chosen for illustration, there it just happens to be no cycle skips on the Z component. It does not mean that we would not observe them for the next receiver and it does not reflect any kind of regularity.

4 *P9, L11-15 (from "One of those" to "larger zero peaks."): How these three sentences are related to the analysis you make in section 3.2? I am a bit confused here. Can you make this point clearer, please?*

In section 3.2, we investigate how changing frequency changes the shape of the histograms of density-related misfit values. When we change frequency, we effectively change two different parameters that affect wave propagation, namely, the propagation distance and the amount of scattering. For example: with increasing frequency, we decrease the amount of scattering, therefore, a histogram for a higher frequency band would be more centered around zero than a histogram for a lower frequency band. But at the same time, with increasing frequency, we increase the propagation distance, and that's why a histogram for a higher frequency band would be *less* centered around zero than a histogram for a lower frequency band. So, changing frequency introduces changes that affect the behaviour of density-related misfits in opposite ways. This results in the fact that the shape of histograms for different frequency bands is not as easily interpreted as in sections 3.3 and 3.4.

However, that is our previous explanation, which took into account only the physical effects. After some detailed discussions with our colleagues, we figured out that we might also see some signal processing artefacts, namely related to narrow bandwidth, that also may change the shape of a histogram with changing

frequency. We have reworded section 4.1 entirely and we hope that it is clear enough.

5 *Noting that density is supposed to impact P and S energies in the same way (so that there is no need for any component rotation to look at a specific energy) could be valuable.*

Thank you for this suggestion. We have modified the 2.3 section of the manuscript (Quantification of waveform differences) by adding a sentence: "Since density impacts P and S energies in the same way, there is no need for any component rotation".

6 *The two statements you make at the very end of the conclusion are reasonable. Nevertheless, be aware that the perturbations you observe in your computed seismograms have amplitudes comparable to the amplitude of a couple of seismic phases which are not modeled in regional simulations, such as PcP, PcS or ScS (see the figure below and some other examples in Cupillard et al, 2012, for instance). As a consequence, a particular care will be necessary for inverting small amplitudes in a given epicentral distance range using a regional wave simulator.*

Thank you very much for this comment. Of course, this is correct. In general, one needs to be careful to have an accurate solution to the wave equation. We added the following comment to the discussion "Finally, we note that the amplitude of the secondary wavefield scattered off density heterogeneities may have similar or smaller amplitudes than globally propagating waves, e.g. PcP, PcS or ScS. Therefore, care needs to be taken when wave propagation is modelled regionally (e.g. Cupillard et al., 2012; Gokhberg and Fichtner, 2015).

7 *In many places in the text or in the figure captions, your write "from" before the*

value of a frequency band, like "frequency band from 0.02 - 0.04 Hz". That is really odd to me. I would either remove "from" or replace the dash sign by "to".

When we bandpass filter, we need to specify the range. If an entity takes a range of values, we describe it as ranging from (value A) to (value B). Both prepositions are needed and losing one would not be correct. We could agree on replacing the dash with "to", but not with dropping "from". Writing "to" instead of a dash would be indeed more consistent. However, we think that if a string of numbers is interrupted with a letter, it is harder to read and understand what is being said. We think now that the best option is to leave the decision to the editors.

8 *P2 L15: I'd specify "direct body wave" to implicitly notice that reflection data carry information about density.*

Thank you for the suggestion. We made this modification.

9 *P3 L1: Please update the reference to Koelemeijer et al, 2015.*

We updated the reference. This paper has meanwhile been submitted, but is not yet published.

10 *"represent a range of"*

Thank you. The typo is corrected.

11 *P 3 L29: "Scattering is most ... than the wavelength." Isn't it the definition of a scatterer? I would remove this part of the sentence*
We understand the concern, however, we have chosen to keep it for clarity.

12 *"amplitude spectrum of these variations in the real Earth". I would add that to prevent any ambiguity.*

Done.

13 *Please interchange table 1 and table 2.*

Done.

14 *P7 L8-12: This is a little confusing to me. You just do the same measurements than before, working now on a 300 s long window after the P-arrival to avoid cycle skips and low amplitude, right? I would remove "In line with surface and scattered waves..." -it is useless; we perfectly got this point in the abstract and the introduction."*

The measurements and the windows stay the same. What we do is we measure time shifts and amplitude differences for each of the receivers of the receiver grid. Then we take the already calculated misfits and stack their values into histograms. We stack only misfits calculated up to 300 s after the first difference between the waveforms (so, not exactly after the P wave arrival, and the mentioned 300 s is not a "window" in the commonly used meaning of the word). This ensures two things: firstly, that the different epicentral distances of the receivers used in our grid do not affect the histogram shapes, and secondly, that we're looking at density-related misfits in the most interesting part of the seismogram. That way we avoid low amplitude and numerical error parts. Understandably, cycle-skipping is not related to this procedure, since it occurs in the previous step, when we actually measure time shifts. (Speaking of cycle skipping: it is inevitably there, but when looking at a time shift histogram, we know how to interpret the entries that have absolute value bigger than 4 s. Since we do not observe a significant amount of those values, they should not affect the standard deviation calculations).

We understand that this part is confusing. Therefore, we changed section 3.2 as follows: “After calculating the misfits for all of the receivers of the grid, we stack their values into histograms, each histogram corresponding to a different frequency band. The values that we consider in the stacking procedure are measured up to 300 s after the first difference between the two waveforms (shortly after the first arrival of the P wave). This ensures, firstly, that the difference in epicentral distance between receivers does not affect the histogram shape, and secondly, that most of the waveforms with large enough amplitudes are included, while excluding low-amplitude parts of the seismograms... .”

We would like to keep the sentence starting with “In line ...” because we think it is a useful and still sufficiently short re-iteration of our objectives.

15 *P7L25:"for two out of five". Captions of figure 7 says "three out of five"!?!*

Thank you for picking this up. It really is two, and the caption is corrected now.

16 *P7 L28-33: "In the lower ... by ~ 1 s." is a useless sentence; "Histograms for ... faraway stations" is too vague; I would remove these two sentences. I would also rephrase the following: "We examined the distance dependence further, using synthetic data from one of the numerical experiments contributing to the tail. We found that, for all the frequency bands, stations of epicentral distance between 1000 and 1200 km show a mean time shift centred on negative values between 0.08 and 0.14 s. While this value is similar for..."*

This sentence may appear as a repetition of what has already been said a few lines before, but our intention is to describe figure 7 and highlight that the feature we write about is visible there. Also, first we talk about the time shift visible in histograms, now we are showing it using a particular waveform. The "faraway stations" in this sentence refer to the two distances we chose further for

comparing (1000 km - 1200 km). We see now that the paragraph is missing this detail and it may seem unclear.

Taking all those comments into account, we have reworded the whole paragraph to make it clearer and more specific, and to avoid any confusion .

17 *P12 L5: "0.125 Hz".*

Done.

18 *Appendix A: I would write phi(k) instead of phik. Moreover, you should mention that f(k) is flat below k_{max}.*

We have modified the Appendix that way.

19 *"Fig. 11: "0.125 Hz".*

Done.

Other changes made in the manuscript: We updated figure 1 to a version that really shows an enlarged triangle symbolizing the receiver chosen for illustration. We also replaced figure 10 and changed the caption to make sure it depicts the correct density heterogeneity amplitudes. We have also added three references: Cupillard et.al., 2012, Yuan et.al, 2015 and Groenenboom and Snieder, 1995. Please excuse our latexdiff tool for the quite unfortunate handling of the reference list.