Dear Reviewer,

In this letter we answer the comments and we correct the small points also suggested.

First, we focus on the comments:

Q1: If I understand it correctly, the error measure considers the wavefield in the entire domain. In many applications, however, one would only be interested in a few measurement points at the surface. Have you done any analysis using such a restricted error criterion, and would you expect a qualitative change of the results?

A1: Yes, you are correct. We haven't focused on a specific area, because in many applications (e.g. migration, inversion) quality is required for the complete domain, and not just at the surface. We do not expect significant differences between the subset of nodes at the surface and the rest, for long simulations.

Q2: You mention in section 2.1 that by setting the ABL parameter to zero, the same wave equation can be used in the physical domain and in the exterior. Do you distinguish those domains in the implementation? This would potentially affect computational performance considerably (with potential implications on load balancing)?

A2: We use a monolithic approach to solve the same equation for each node, only that in the interior nodes a zero multiplies the absorbing terms. It definitely affects performance, but we didn't want to go deeper into this subject.

We clarify this point including this text around line 440 of the new document:

"Finally, we remark that for all methods, we solve the complete absorbing equation for each grid node, only using non-zero values for the absorbing parameters inside the absorbing layers."

Q3: I understand that absorbing boundaries for elastic media are a totally different can of worms, but I would be interested in getting your take on the validity / transferability of the results to VTI acoustic (and maybe elastic) media.

A3: Yes, this is part of our future work. We definitely need to re-run the calibration process for each physical setup to test that 1) the methodology holds and 2) which specific set of parameters are found to be optimal.

To address this point, we write this text at the end of the third paragraph in the conclusions section:

"The methodology to calibrate ABLs in this work could be applied to other wave equations such as the elastic wave equation or anisotropic wave equation. We do not expect the same calibration values to hold across all the equations, but the methodology should reveal the optimal values for each case. This will be subject of future work"

There just a few more small points I noted, which I am listing below:

Line 100: Do you assume differentiability of the source in time or in time and space? Just wondering about point sources here.

We use a regularization of Dirac's delta function for the spatial component of point sources, which is a gaussian. In time we chose a Ricker wavelet which is the second derivative of a gaussian. We atttempt, with this process to avoid contributions beyond the Shannon-Nyquist sampling theorem.

To clarify this point, we write at the end of the first paragraph of Section 3.2 this text:

"Note that we use a regularization of Dirac's delta function for the spatial component of point sources, which is a gaussian. In time we chose a Ricker wavelet which is the second derivative of a gaussian. We attempt, with this process to avoid contributions beyond the Shannon-Nyquist sampling theorem."

Line 125: What is the reasoning behind adding the additional point with a pressure of zero? Is it correct that this essentially gives a homogeneous Dirichlet boundary condition? Why don't you use some first-order condition at the outer boundary instead?

In Fourier spectral methods, we need to ensure continuous periodicity of the spatial distributions. By imposing exact zero values this condition is met. This is not sufficient to avoid artifacts but necessary to mitigate the Gibbs phenomenon. First-order conditions would not be able to ensure periodicity in the same way.

At the end of the first paragraph of Section 2.1,we write this text:

"It is important to remark that these extra nodes are essential to avoid the Gibbs phenomenon at the edges of the spatial mesh. Note that spectral derivatives require imposing periodicity to the spatial distributions, therefore in this way, we ensure spatial periodicity in any direction of the mesh."

Line 160: Have you analyzed the effect of the non-differentiability of sigma, i.e., the kink at the transition from the inner to the outer domain? I can imagine this might lead to artificial reflections.

No, we have used only a linear profile which indeed results in impedance. However, all discrete versions of these ABLs incur in impedance at the boundary or inside the layer. For a detailed analysis of higher-order profiles we recommend Spa et al (2014).

To clarify this point, we add this text at the end of the final paragraph of Section 2.4:

"Finally, it is worth to mention that there exist other profiles that perform better, see for instance \cite{Spa2014} that they suggest order 3 and 4 polynomial absorbing profiles. However, in this analysis, we chose a linear profile because we prefer to focus on both, the calibration methodology and the design of the numerical experiments, rather on studying specific absorbing profiles of each method."

Eq. 11: Is there any physical intuition behind the exponential decay? I assume artificial reflections can occur similar to DWE if the slope is too steep? Intuitively, I would have assumed that N_ABL also enters into the formula, but it seems the values at the outer boundary will differ depending on the distance only? Just as a personal preference, I probably would have used other symbols than σ and μ for the ABL-related coefficients, as I would associate those with stress and shear modulus, respectively.

We are just following the standard reference here (Cerjan 1985). As we mentioned in a previous answer, we do not focus our attention on particular profiles, but rather on a methodology to calibrate the main parameters. Definitely, there should be a dependence between the parameter and

N_ABL. However, as our methodology always analyzes pairs of N_ABL and the parameter, such dependence loses relevance, at least for our purposes.

At the end of the section 2.3, we write this paragraph to clarify this point:

“It is important to mention that this profile is neither polynomial nor dependent on N_{ABL}. As we mentioned in the previous subsection, we do not focus our attention on particular profiles, but rather on a methodology to calibrate the main parameters. Definitely there should be a dependence between the parameter and N_{ABL}. However, as our methodology always analyzes tuples of N_{ABL} and the parameter, such dependence loses relevance, at least for our purposes.”

Line 265: Typo: expressions

Corrected.

Line 288: Out of curiosity: Is there a reason for not using a more recent version of g++?

No, we just used the version that we had installed at that moment. We believe that only relative performance between runs should be used, being the absolute performance dependent on the hardware and the compiler, as well as potential optimizations carried out by the authors. Hence we only report relative times.

Line 397: Just to double-check: Are you using a free surface condition at the top? I don’t think this is the case, but I somehow would have expected this for the realistic SEG/EAGE model.

No, we are not using a free surface. We have focused on the infinite case. We believe that adding a free-surface should require a complete re-calibration of the parameters. We leave this for a future work.

We add this text into the first paragraph of section 4.2:

“We remark that we are not adding a free surface condition to be compatible with the calibration exercise of the previous sections which also were unbounded.”

Line 445: Consistency when referring to Fig., Figure, figure.

Checked

Line 480: Typo: Such -> such.

Checked