Review of manuscript GJI-16-0524.R1 (2016-12-07)

Dear Editor and Reviewers

Before the answers to the specifics questions, let us introduce general purposes of this new version.

First of all, we thank you a lot for your both comments and recommendations. As we can see, they are very contrary views. Reviewer 2, who visibly well knows the issue of seismic measurement with ultrasonic devices and the topics of small scale measurement for seismic topics which infer, suggests a minor revision whereas reviewer 1 proposes a major revision, arguing the goal and the increase proposed in the paper, and thus its utility appears, to his mind, not so clear.

Thus, regarding the differences of their points of view, we tried in this new version, to explain more clearly in the introduction the following points for readers who does not commonly use ultrasonic data :

- the main objective of the paper aims to quantify the quality of small scale seismic data provided in the laboratory using laser interferometer and piezoelectric source on a resin-made model, for any use of imaging method. This quantification, without any pre-processing adapted to an imaging method, is very important because it can interest any geophysicist who wants to test any imaging method, applied on any scale of interest. This aspect is a reason why we submit this study in GJI : indeed it is read by researchers who are interested by very different scales of underground investigations. In this way, the data are freely distributed to the geophysics community. And anybody can ask for them, by send us an e.mail (we added this information in the part dedicated to Acknowledgements)

- two studies arise from this global objective, i.e. :

    - 1) the fine and quantitative comparison with numerical data avoiding any artifacts usually given by the 2D-3D corrections. For this part, we also prove the importance of avoiding this correction by showing the bias it provide.

    - 2) the study of the repeatability of the source impact. This point is crucial when we collect the data. It is well known for experimentations with piezoelectric transducers but not commonly tackled.

They are crucial keys because never done in the scientific literature. The small scale experimentations are usually presented for a specific imaging test. In this paper, we propose studies apart any imaging process in order to propose data to the scientific community for any kind of imaging process. However, the validation will be proposed on realistic models in terms of velocity ratio, attenuation and dispersion of surface waves velocities, in order to study realistic data.

All the corrections in the text are described below, along the answers to the remarks pointed out by the reviewers.

Best Regards,

D. Pageot and D. leparoux

<u>REVISION 2 – REVIEWER 1</u>

I reviewed a previous version of this manuscript (see my review below, in blue fonts). My first comment was about the need for important modifications of the abstract. In there response, the authors did not mention the abstract which remains, in the reviewed version, almost identical to the previous one.

For this new version of the submitted manuscript, we propose a completely new abstract with a new formulation in order to :

1) better highlighting precisions about the general goal of the study as well as the 2 objectives of the 2 parts of the proposed study.

2) summarize more precisely the particular results obtained.

Anyway, as an answer to my review, the authors tried to clarify the whole manuscript (they for instance moved the already published 3D-2D spreading correction method to an appendix section). They modified the text, mainly in by adding paragraphs in the introduction. It is, by the way, difficult to perfectly check slight differences without a version with marked changes...

In order to better highlight the modifications proposed in the previous corrected version, they are written in bold font in the new version. The new corrections which are added in the version R2 are indicated in red color.

Then, if I am correct, the authors more particularly added some text (lines 91-119 and lines 305-313) and several references to introduce the need for 3D/2D conversion techniques to interpret 3D data with 2D methods and to claim that "imaging methods are used mainly for 2D structures".

All the lines which were added in the R1 version (and appearing now in bold font) are indicated again (in the copy of the previous answers at the end of the document) with the new line number in the version R2

In section 2, they first present the laboratory rather than the numerical method, as asked by the reviewer 2. I recommended in my first review to summarise these presentations or to move them to an appendix section. The authors kept these sections in the main text arguing it was important, but I still do not see why.

In the previous corrected version, we tried to answer both to reviewers 1 and 2. The presentation of numerical and experimental tools have been remained as it is currently done in scientific papers as part dealing with "Material and Method" before the presentation of the data analysis and results. This articulation sounds a classical way of presenting experimental results and analysis. Moreover, to our mind, it is important to make possible for the reader to have a linear reading of the entire paper without he has too frequently to search informations in the end in Appendix or in others published articles. However, the goal here is not to write the same complete review already done in previous papers. Thus we invite the reader to refer to those previous papers for a more complete review of the small scale modeling. (see line 70 of the introduction)

Another reason for keeping the MUSC presentation in the paper : the issue of the measurement precisions compared to field data was not been done in the previous papers. It is a real new information. (lines 268 to 284 in version R2)

Actually, I believe these physical and numerical modelling techniques have already been used and presented in details in the past (some of the authors published, in GJI, the previous "upgrade" of there apparatus, see Valensi et al. 2015).

The presented paper does not aim to present an upgrade of the apparatus as it had been done by Valensi et al. (2015). The latter tackled the horizontal component which is a new way of recording whereas we tackle here only the vertical component. The purpose of our new article is not an upgrade of the apparatus but rather a quantitative analysis of the recorded data on resin models through a fine quantitative comparison to numerical data because it has not yet done until now.
We indicate in this new version why our new study will answer issues not yet resolved in (Bretaudeau et al. 2011) (see lines 97 to 100 in version R2)
Furthermore note that if approach proposed by Valensi et al. (2015) performed a fine comparison of measurement with the analytical solution but on an aluminum block. It was done for testing the horizontal component and it used a piezoelectric source adapted to the aluminum material. It is not adapted for resin model and it does not provide an isotropic pattern, as shown by (Bretaudeau et al, 2011). Thus it is not the same transducer as the one used in the study presented here.
Furthermore, we can notice that the numerical method used here is neither the method used by Bretaudeau et al. nor Valensi et al. (2015).

**Small-scale modelling
technique are obviously not "mainstream", but there are enough references in literature about the topic to help readers who would like to know more.
I agree their
use has to be encouraged and believe the authors do work in this purpose. However, as I wrote in my previous review, GJI can just not publish each improvement of an experimental set-up.**

Our goal is not only to encourage to use small-scale modeling technique (for that, we agree that papers of Bretaudeau et al., are convenient). The main purpose is to quantitatively define the quality of small-scale data for the geophysicists to make possible to use them for testing imaging methods with the knowledge of the degree of confidence. Actually, building a small scale bench is a heavy and costly work. Thus it is more convenient to share the results and our data are freely distributed to scientists for testing any method of imaging process. For this reason, we propose to study the data independently of any imaging method. Our study is a stage before application to an imaging validation process. In this aim, we submit the paper in GJI because it is a largely read journal for a various geophysicists community with different scales of interests, from subsurface to global scale including oil exploration dimensions. We could propose it to a more specific journal but it would be a pity for the other community.

My recommendation was major revisions with (see blue comments below and the authors answers for more details) :

**(1) to work on a comprehensive review about the topic (3D data versus 2D interpretation tools) and then write an introduction with arguments and references supporting their work;
I think this point has been only partly addressed in there reviewed introduction**

and they did not thoroughly answered my questions.

In this new version, we modified the introduction. The parts which are added are written in red. Concerning why we propose a 2D version of our tool, we inserted :

lines 115-117
"This approach allows indeed to accommodate the recurrent issue of 2D/3D imaging and modeling aspects which come from the following findings. Actually real 115 media are rarely 2D, but so far .."

lines 121-123 :
"Added to the need due to the computational cost of 3D modeling, the natural process of numerical developments needs gradual stages from 1D to 3D, including the 2D case (see for example 121 Capdeville et al. (2010))."

and lines 138-141
" In order to avoid these limits, the MUSC laboratory is based on the principle of seismic 137 waves propagation in 3D blocks. In this way, it is well adapted for 3D modeling and imaging. However, considering the numerous approaches in 2D-spaces as mentioned above, a suitable experimental tool is highly needed for the the stage of 2D validation. "

By introducing these sentences, we aim to clarify our purpose : we do not think that 2D modeling is the "only and better" way for imaging processes, but, as a matter of fact, the scientific process of numerical development as well as the high computational cost of 3D need a stage of 2D tool. This stage also need experimental validation and that is why the way of carrying out 2D data proposed here can offer interesting data sets for validating different imaging methods in the stage of 2D development.

(2) to completely re-organize the manuscript and improve its outline to better convey the information;
This has not been completely done. The authors did not answer the main questions of my previous review so I am still unclear with the objectives (even if I read the manuscript several times).

In order to better clarify the objectives, that we have summarized in the introductive letter above, we added new lines and reformulate some others in the new version (red lines).

For the main objective, see lines
84 to 92
97 to 100
103 to 108

For the issue of the first experimental study arised from this main objective, see the added lines
109 to 117

For the issue of the second experimental study arised from this main objective, see the added lines :
157 to 160
162 to 164
165 to 167

(3) to show with a real example how modelling 2D-like data can help (thus

demonstrating the interest of their approach with an actual application of their experimental tool).
This has not been done. I thinks it is mandatory to clearly show the utility of their improvements and of the proposed approach.

We propose here a study which is a step before any imaging test because data should be usable for any kind of imaging method as we explain in the introduction letter above as well as in this new version :

see lines 168 to 173 :
“the approach of validation proposed here has to be conducted apart any imaging process in order to define the quality of the data, independently of any criterion of imaging hypothesis. It will make this data possible to be used by the scientific community in any kind of inversion process and any kind of scale ratio of simulation. “

However, we agree the data have to be realistic in order to be interesting for geophysicists. We added this precision in the new version

See lines 168 to 180 :
However, the validation will be proposed on realistic models in terms of velocity ratio, attenuation and dispersion of surface waves velocities, in order to study realistic data. The resin models used here for the experimental tests are presented in the first part below with the summary of the specifics of the MUSC 175 laboratory....

An other sentence is added for highlighting this purpose line 291 when we explain that the BiAlt model simulates a 2-layer model with clays laying upon limestones :
“Thus, the BiAlt model can be seen as a realistic model providing realistic data.

These 3 points were only a summary of my first review which contains way more detailed questions remaining unanswered (or too quickly tackled) by the authors. As I wrote earlier, small-scale modelling techniques are great to benchmark numerical methods and imaging techniques. I think imaging methods have to be validated with real 3D data and that is why I like small-scale physical modelling. The laboratory apparatus the authors present here is surely one of these great tools. However, I think GJI is not the place to publish each of its improvement. That is why, without an actual application of there approach, I would recommend to submit this work to more appropriate journals.

As we mentioned above, we propose here to study the data independently of any imaging method. These data are available to be used by different geophysicists and for different imaging methods. Our study is a stage before application to an imaging validation process. In this aim, we submit the paper in GJI because it is a largely read journal for a various geophysicists community with different scales of interests, from subsurface to global scale including oil exploration dimensions. We could propose it to a more specific journal but it would be a pity for the other parts of the community.

<u>REVISION 2 – REVIEWER 2</u>

The revised manuscript GJI-16-0524.R1 by Damien Pageot and coauthors entitled now as "Improving the seismic small-scale modeling by comparison with numerical methods" has been modified carefully to enhance the original submission. The authors addressed all points raised by me and another reviewer, improving the clarity and structuring of the paper. In this revision now, the authors clearly point out the new contributions and manage to present a nicely structured paper.

With this new revision, I would only recommend to slightly polish the text. Some small typos and writing mistakes made it into this revision which would be good to see corrected in a published version

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• **page 3, line 74: "MUSC (Mesures Ultrasonores Sans Contact in French)" why not drop just the "in French", the abbreviation is MUSC not MUSCF…**
It's done.

• page 5, line 119: "in te" a typo, should be "in the"
It's done.

• page 5, line 128: "the specificities" ? more likely "the specifics"
It's done

• page 6, line 161: "piezzo-electric", you also use "piezoelectric" and "piezo-electric". Please only use one version (probably "piezoelectric") throughout the text.
It's done.

• page 6, line 161: "For the linearized wave equation, ...", the paragraph becomes too long. Please consider splitting it up and separate the thoughts.

• page 10, line 287: "polynomial degree n l + 1 = 5 ...", maybe better just state "polynomial degree
n l = 4"
It's done.

• page 14, line 443: "are closed", a typo, should be "are close"
It's done.

• page 14, line 443: "and confirmed", a typo, should be "and confirm"
It's done.

• **page 15, line 473: "In the previous ...", again a very long paragraph. Please consider splitting it up**
and separate the topics.

• page 36, figure 10, caption: "cc gives ..", please specify if the cross-correlation values are for black–red or black–blue comparison
It's done.

REVISION 1 – REVIEWER 1

1-INTRODUCTION

1.1 The first part of the introduction (line 48 page 2 to line 52 page 3) intends to describe how « reduced-scale physical modelling » (as the approach is named by the authors) has been used to help developing seismic imaging methods. This section appears incomplete about the subject, more particularly if compared to cited articles in which proper references could have been found. It does not present the variety of approaches and materials used in recent laboratory studies and it lacks important past and recent references about the topic.

Some references have been added to the corrected version in the introduction and description of small-scale physical modeling. A short explanation on why a 3D measureemnt bench (MUSC) rather than a 2D approach which consists in using non-dispersive Lamb waves has been added (see remark of reviewer 2 1.3).
For this argument, a text and references has been added lines : 91 to 106 (115 to 130 in R2)

1.2 The second part of the introduction (line 54 page 3 to line 16 page 5) intends to present the main aims of this work. This section should describe the outline, the main questions and the general issues of the subject the authors are dealing with but it does not.

This part has been rewritten, completed and reorganized to present main aims of this work.
See lines : 88 to 90 and line 119 (line 121 to 180 in R2)

1.3 The authors claim their general objective is "to complete the validation of the capability of [such] ultrasonic devices to precisely and quantitatively simulate surface seismic data carried out with multisource and multireceiver settings" (see lines 8-14 page 4). It sounds pretty similar to the objectives of Bretaudeau et al. (2011) and the work described in this manuscript is only presented as the improvement of the equipment in order to "increase the potential" of the author's experimental laboratory. Up to this point, the authors are submitting to GJI a technical "refinement" of an experimental tool. I am fine with this since GJI, among other journals, recently published several articles on this very interesting and promising topic. But the introduction is too vague and does not give enough details nor arguments for the readers to figure out the actual "potential" of such equipments and to understand why it has to be "refined", as it is mentioned in the title.

The objectives of (Bretaudeau et al. 2011) were mainly to validate the MUSC laboratory as a reliable tool to reproduce field acquisition but some discrepancies remained in the comparison of the amplitudes to numerical data due to the 3D/2D conversion. Thus, the first objective of the refinement of the analysis proposed here is to reduce these discrepancies through a new test consisting in usinq an experimental source line. Furthermore, experimental measurement with a 2D line source will offer the possibility to directly validate 2D imaging methods in further works. A second aspect of the refinement of the assessment of the capacity of the measurement bench tackled in this paper consist in testing the source repeatability.

Some parts of the introduction have been rewriten to clarify the objectives of our paper and some references have been added (see answers 1.1, 1.2 above and responses to reviewer 2 1.2) .

1.4 If I understood well, the authors first point out the fact that, despite recent developments of 3 dimensional (3D) numerical modelling methods (they do not give any reference), most interpretation (processing, inversion) techniques involve 2D (or even 1D) assumptions. They

consequently claim that "the differences between 2D and 3D propagated wavefields must be explicitly taken into account to successfully validate imaging methods using field or experimental data". What I understood is that the authors want to show the great "potential" of their 3D physical modelling tool by making it provide 2D-like seismic data. I do not clearly see the interest in such approach.

As discussed in 1.1, 3D physical modeling tools have some advantages over 2D physical modeling tools even to produce 2D-like data.

Please see response to **1.1** and **1.3** for additionnal informations.

<u>1.5</u> **I believe that experimental studies and associated equipments (including the laboratory equipment presented by the authors) are extremely useful to understand seismic-wave propagation in 3D media, more particularly when numerical modelling fails to reproduce the actual complexity of real media. But why do the authors try to degrade their data which are recorded on 3D media (more particularly when they were able to built these physical models with almost a perfect control on dimensions and parameters) ? Why developing such an important and sophisticated experimental setup, dedicated to physical modelling (which means on real, hence 3D, structures) and then focusing on 2D aspects ? How 2D data can validate imaging methods when real data are obviously 3D (as far as I am concerned, an imaging method is valid when it is successful in describing real media...) ?**

The fact is that even if 3D numerical modeling is widely developed, 3D imaging is still a challenging task and 2D imaging include some approximations (cylindrical source for example) which can not be realize on field. The goal here is not to degrade a sophisticated 3D experimental setup but to provide experimental data able to fit 2D imaging method limitations without supplementary data pre-processing such as geometrical spreading correction or 2.5D approximation. More, many models used with MUSC have a 2D structure invariant in the y-direction.

Please see response to **1.3**. in addition.

<u>1.6</u> **I am fine with the idea that imaging techniques are mostly 2D. I agree that this point is, in itself, an important issue of seismic imaging and in geophysics in general. But I do not see the interest in trying to adapt 3D modelling methods to provide 2D data in order to validate 2D imaging methods. I think one should better concentrate on their improvement. To do this, I firmly believe we need to work on real data. But as it is said in the manuscript, one does not control, neither perfectly know, real media. Then 3D numerical and physical modelling tools can be used to provided realistic data (obtained on almost perfectly controlled 3D structures) to find ways to adapt imaging method, but not the contrary. This is mainly why I do not understand the objectives of this work.**

See responses **1.3** to **1.5.**

<u>1.7</u> **But possible reasons of my misunderstanding could as well be the lack of references about the subject and the poor organisation of the introduction, and of the manuscript in general.**

As said in responses **1.1** to **1.5**, introduction has been rewritten, clarified and references have been added.

<u>1.8</u> **At the end of the introduction, I understood the authors were going to present both numerical and physical modelling studies, but I did not understood how and why. The next sections (2.1 and 2.2) only give general presentations of these methods with a lot of references**

and details that should (including tables, photographies and figures given as "examples") be summarised in an appendix section since they do not directly connect with the main subject of this work and do not help reader understanding the objectives.

The aim of this work is to provide experimental data in controlled environement for validation of 2D (high-resolution) imaging methods. To do this, we explore several issues related to 2D FWI validations such as (1) absence of real 2D data and (2) importance of source function knowledge.

For the first point we show the limitations of 3D-2D spreading correction and we propose an alternative approach which allows to produce 3D experimentally constructed line-source data, i.e. 2D equivalent experimental data in controlled environement. A description of MUSC is then important. More, 3D-2D spreading correction limitations can be more easily shown using numerical modeling.

The second point is, again, related to 2D FWI validations. FWI algorithms contain features like source function estimation widely based on deconvolution. However, this estimation is strongly related to the accuracy of initial model (M0). If M0 is not sufficiently accurate, the estimated source function will absorb inaccuracies and degrade the final inversion result. This is why a good knowledge of the source function is critical in FWI. Thus to evaluate the source function and state on its reproducibility we have to use a stable and accurate numerical method.

2-METHODS

2.1 The models and their parameters are then presented. I did not understand the links between the Table 3 and Fig. 2. Why does the table contains properties of material not presented on Fig. 3, nor in the text, if I am correct ?

This has been clarified in the corrected version. The aim of the table is to show several materials commonly used to manufacture scale-models. Fig. 2 which was not cited in text in the previous version of the manuscript is now referenced.

Text added in the corrected version (Line 280 to 284 in R2):
« As shown on Fig. 2, even if the emission properties of a piezo-electric transducer is narrow-band, the spectral bandwith of the pulse emitted by the piezo-electric transducer is large enough to simulate a seismic pulse emitted by a hammer fall in subsurface media, through the scale ratio used in table 2. »

More details and remarks in responses to reviewer 2 section **2**.

2.2 In addition, the physical models are obviously 3D and with edges... so Fig. 3 should present every dimensions, show the 3D structures and where the acquisition setup is/are implemented (more particularly so the readers can easily find boundary conditions).

It is done in the corrected version. Fig. 3 now shows 3D models. This new figure make it easy to identify the acquisition by relying on the figures 4 (Fig. 5 in the previous version) and 7 (Fig. 8 in the previous version).

2.3. As for numerical modelling, Fig. 4 is completely useless in its present form (it does not give any information about the model) and deserve way more details.

Yes, Fig.4 is completely useless in its form but a more detailled figure would not be readable. Consequently, this figure has been removed but explaination on mesh are still present in Numerical modeling section and related description was moved to « Numerical method » section.

2.4 The next section presents in details the method developed by Forbriger et al. (2014). I understand this approach is important for the study but, as it does not correspond to the main work of the authors, it should be given in appendix and this section should concentrate on the processing workflow used by the authors for the study.

It is done in the corrected version. This part is now in appendix and has been modified (line 673 to 715 in R2).

3-RESULTS

3.1 This short introduction of section 3 helped me understanding the main aim of this work and guessing the point of the authors. But it remained difficult for me to follow the author's approach, due to a lack of organization and a confusing description of the setup. Here again, it was not easy to find the link between the two parts of this section.

As said previously, we have re-organized the manuscript and clarified some critical points to allow the reader to grasp the problem from the start.

4-CONCLUSIONS

4.1 At the end, the authors claim their study show that the equipment provides a good source reproducibility and that "an experimental source-line should be recommended instead of the hybrid correction of data". I see how the authors performed the study and, despite the great lack of clarity in the manuscript, I feel confident with the results (obviously a source-line is the best way to tend toward 2D-like data). However, the manuscript in its present form is not suitable for publication in GJI. I would recommend the authors: (1) to work on a comprehensive review about the topic (3D data versus 2D interpretation tools) and then write an introduction with arguments and references supporting their work, (2) to completely re-organize the manuscript and improve its outline to better convey the information and (3) to show with a real example how modelling 2D-like data can help (thus demonstrating the interest of their approach with an actual application of their experimental tool).

Concerning remark (1) to work on a comprehensive review about the topic (3D data versus 2D interpretation tools) and then write an introduction with arguments and references supporting their work

As said previously, it is done in the corrected version. See responses to reviewer 2 for additionnal remarks and responses on this point.

Concerning remark (2) to completely re-organize the manuscript and improve its outline to better convey the information

It is done in the corrected version.

Concerning remark (3) to show with a real example how modelling 2D-like data can help (thus demonstrating the interest of their approach with an actual application of their experimental tool).

The objective of this paper is not to present an imaging application which would be a particular case but to propose a solution to the validation problem of 2D imaging methods that require real/experimental 2D data. To address the quality of the resulting data, we estimate the correlation coefficients as efficient quantitative indicators for 2D seismic imaging methods.

4.2 I would, in addition, recommend authors to provide a numbered manuscript, with enough line-space for reviewers to comment and to give more detailed figures and captions. It is also important to correctly insert references and to check the reference list for typos.

The manuscript is now numbered, figures have been checked and modified (if needed) , and references have been corrected. Other typos/references remarks have been taken into account (see responses to reviewer 2 section **5**).

REVISION 1 – REVIEWER 2

TITLE

Please change the title to a new one, not using the same as for the conference paper published in 2015.

Yes, it is done.

1-INTRODUCTION

1.1 The manuscript motivates controlled experimental measurements by noticing that both synthetic benchmarks and validations of imaging techniques are generally limited. This is an important aspect, however it would be great to provide further motivation and background about small-scale physical modeling by the MUSC laboratory.

We have reorganized introduction and the section about MUSC laboratory .

In introduction, the following lines have been added :
lines 88 to 90 (114 to 129 in R2)
lines 91 to line 106 (120 to 180 in R2)
line 121

In the section concerning the MUSC laboratory, different parts have been added to answer to the remarks below (see the indicated lines).

1.2 When introducing small-scale physical modeling (page 3), it omitted references to many previous studies using thin-plate models for this same purpose. I think it would still be worthwhile to mention some of this work (sometimes referred to two-dimensional, plate wave or Lamb wave modelling), like Oliver et al. (1954), Angona (1960), Healy and Press (1960), O'Brien and Symes (1971), Pant et al. (1988) or more recent Mo et al. (2015).

References have been added in the corrected manuscript.

1.3 A short explanation about the differences and limitations of 2-D versus 3-D physical modeling would be great to provide, in order to motivate the setup chosen of the MUSC laboratory.

It is done in the corrected version in Introduction, line 98 to line 106 (121 to 180 in R2).

1.4 Also, the nomenclature and abbreviation used of "Physical small-scale Modeling Methods (PSM)" is somewhat new, where most other literature uses terms like "physical scale model", "physical modeling" or "scale model data". Thus, it would rather be "Small-scale physical modeling methods". The abbreviation PSM in the manuscript is hardly used and could be omitted altogether.

We use "Small-scale physical modeling methods" in the corrected version.

1.5 You mention modeling surface waves as a reason not to immerse the model in say a water tank. However, you do not further discuss this important topic and the results presented in this study omit showing surface waves. It would be great to list limitations in physical

modeling of surface waves and clarify what you refer to as ”reflection echoes”, mentioned in the conclusions.

Concerning « ommit showing surface waves »
Surface waves are presented but labelized as S-wave. Indeed, in a homogeneous medium, the direct S-wave propagating along the free-surface is a non-dispersive Rayleigh wave. It is corrected in the corrected version.

Concerning ”reflection echoes”
Our words were wrongly chosen here , “reflected echoes » in the conclusion refers to ringing effects of the source in fact. This ringing effect carried by the direct P and S/Rayleigh waves interfere with direct S/Rayleigh wave and reflected-wave arrivals respectively. This is corrected in the new version.

The findings presented on page 4 could be further structured. The manuscript mainly lists the following problem items addressed in this study: (1) line-source versus point-source modeling, (2) geometrical spreading corrections, (3) effective source time function, that is the transducer influence, (4) reproducibility of the experiment/measurement. These items could be separated into challenges related to the ”forward problem” of modeling wave propagation in 2-D (items 1,2), the ”inverse problem” of retrieving the correct source and model parameters (item 3), and finally the robustness, stability and precision of the measurement apparatus (item 4).

Line 120 to line 140 (155 to 180 in R2). This part has been restructured in the corrected manuscript taking into account the difficulties in organizing the results. We understand this remark but splitting into challenges would require, from our point of view, to rewrite largely the result sections. Indeed, these challenges are strongly linked to each other: (1) 3D-2D spreading correction and point-source versus line source, and (2) source reproducibility and source time function estimation. However, we try to clarify at best these different results and we now differentiate them in the introduction.

2-METHODS

2.1 Since the focus of the study is on physical modeling, it would make sense to start presenting the MUSC laboratory (section 2.2) rather than the numerical method used for comparison (section 2.1).

We have reorganized the manuscript to present the MUSC laboratory first.

2.2 Furthermore, when presenting the MUSC laboratory it would be helpful for readers to mention limitations which are expected. For example, it is known that ultrasonic transducers are resonant and narrow-band due to the physical properties of these devices. Also, the transducers are in general large compared to the minimum wavelength of the simulation, thus can impose directivity effects. Some of these effects have been studied by Fran¸cois Bretaudeau et al. (2011) already, but it would be great to further mention and justify the selected frequency-bandwidths of 20 - 200 kHz and 300 - 800 kHz.

These remarks are tackled in the corrected version in 3 separated parts as following :

1) For example, it is known that ultrasonic transducers are resonant and narrow-band due to the physical properties of these devices.

piezo-electrics components are indeed resonant. One of the difficulty to avoid the resonnant effect is the impedance adaptation of the transducer to the propagating medium. However, in MUSC, we use tranducers who have been built with well-adapted shape, backing element and suited material for the contact to the medium, in order to be adapted to the impedence of the material that we use, as much as possible (for exemple, they are not adapted to alluminium blocks). That is specially the case of those adapted to a central frequency of 100 KHz in the paper. In the case of the higher frequency one (not used in the tests described here), a conical adaptator in resin between the transducer and the model allow to weak the resonant effect. This aspect is already described in (Bretaudeau et al, 2011) and, to our mind, it should not be repeated with long developments in the present paper. However, it is recalled in the corrected version as recommended in lines 161 to 163.

Moreover, even if the resonnant effect are avoided, the response of the transducer remains as a filtering effect to the electrical signal injected in input. This aspect is a critical point tackled in the present proposed article. We have added sentences lines 163 to 168 (203 to 210 in R2)

Concerning the narrow-band aspect, lines 238 to 242 (280 to 284 in R2) have been added.

2) Also, the transducers are in general large compared to the minimum wavelength of the simulation, thus can impose directivity effects. Some of these effects have been studied by Fran¸cois Bretaudeau et al. (2011) already, but it would be great to further mention

Precisions about this have been added in the corrected version lines 178 to 188 (218 to 229 in R2)

3) and justify the selected frequency-bandwidths of 20 - 200 kHz and 300 - 800 kHz.

It has been clarified in the corrected version through the text lines 188 to 196 (229 to 237 in R2)

2.3 Also, it would be great to evaluate the precision achieved of the measurements in the MUSC laboratory. For example, the precision of the positioning of the receiver position is stated as ±10µm which for a shortest wavelength of 1mm amounts to a precision of 1%. So does the vertical offset measurement by a laser beam with a 20µm diameter, thus the measurement is not exactly a point measurement but an average over a small surface fraction. One could briefly present for the chosen setups what error of the measurement would be expected. Since this is one of the advantages of laboratory settings, you could put error bars on the traces.

It has been clarified in the corrected version through the lines 152 to 154 and lines 227 to 237. (192 to 196 and 268 to 279 in R2).

2.4 The same consideration also holds for the ringing of the transducers which will likely look different at different central frequencies. One could present the ringing for different central frequencies of the source, to better highlight in which frequency regime the transducers affect measurements and need to be treated more carefully as is then done in the following of this study.

The frequancial effects of the transducer is tackled thorugh the entire waveform, as indicated in the modified version 168 to 170 (203 to 210 in R2)

3-RESULTS

<u>3.1</u> It would help to start clarifying which 2-D numerical result is compared against which 3-D experimental data and somehow introduce nomenclature for 3-D experimental point-source, 3-D experimentally constructed line-source and 2-D numerical line-source.

This has been taken into account and the proposed nomenclatures are now incorporated in the corrected version.

<u>3.2</u> Section 3.2 would be great to further subdivide into new sub-sections: The first part deals with reproducing single measurements to validate the reproducibility of the source. The second part deals with the effective source time function estimation, starting at page 14, line 29 ("In the second step ..."). The third part introduces the new 2-layer model called BiAlt, thus deals with a new experimental setup (page 15, line 8) and comparison. It would help to put these into different sub-sections and discuss each result separately.

For convenience, the RESULTS section has been reorganized into three separate parts, hoping that this will allow for better understanding of the results.

<u>3.3</u> Also, in your comparisons between different traces you use a correlation coefficient over the whole trace length. Since you already see and discuss different effects on P-, S-, PP- and PS-phases, it would make sense to further separate the comparisons into just comparing a single phase. This would make it clearer in how well a P-wave, S-wave or PP- or PS-reflection is treated in the geometrical spreading and 2D line-source approximations.

Additionnal correlation coefficients for P-, S/Rayleigh- and PP-reflected waves have been added to figures 8 and 9 (figures 9 and 10 in the previous version). Psv-reflected wave is difficult to locate on real data, so the correlation coefficient has not been calculated for it. Results are discussed.
Line 442 to line 446 (485 to 490 in R2) and from line 462 to line 465 (505 to 509 in R2).

<u>4-CONCLUSIONS</u>

<u>4.1</u> Would it be that you verified experimentally that you can construct a line-source using the MUSC laboratory? or would it be that the geometrical spreading transformation applied to 3-D data is only accurate for P- and S-phases, but not reflected ones like PP- or PS-phases?

We think that the most important result is the 3D constructed line-source since other publications have already shown limitations of geometrical spreading correction methods.

<u>4.2</u> The very last sentence mentions the addition of new measurements in the MUSC laboratory by Valensi et al. (2015). It would be great to shortly explain this addition in the introduction and why it hasn't been used for this study.

Valensi et al. (2015) have introduced horizontal component in the MUSC laboratory. However, when this study was realized the horizontal component was not fully calibrated and signal-noise ratio issues were persistant. Furthermore, some data were generated before the improvement of the laser interformeter. Consequently, we use only the vertical component.
comment – It is a work in progress that will require a little more time before being fully finalized.

<u>5- OTHER REMARKS</u>

<u>Manuscript text</u>

page 2, line 2: ”2 D and 3 D” please see if 2D and 3D or 2-D and 3-D could be used in a more coherent way throughout the text.

It's done.

page 2, line 49: ”algorithms innovations” just use e.g. ”algorithms”.

It's done.

page 3, line 56: ”sharp similarities” maybe better ”close ..”.

OK.

page 4, line 33: ”It is based on the following findings.” awkward ending of a paragraph.

Yes, it's awkward...corrected.

page 4, line 38: ”3D/2D geometrical spreading effects” it would be clearer saying ”3D-to-2D geometrical spreading corrections”.

OK.

page 6, line 56: ”Since the wave equation is linear” the sentence sounds a bit odd, maybe ”For the linearized wave equation, ..”

It's done.
h
page 8, line 24: ”finely” ? could be omitted

OK.

”In the far-field approximation, ..” You show the single-velocity transformation in eq. (10) and then state ”is recommended for small offsets”. This seems to contradict with the initial far-field assumption. Maybe you could clarify it.

We have no physical explanation for this. We can just refer to Schafer (2014) and Forbirger (2014) who have shown, on the basis of numerical tests, that single-velocity transformation is better for small-offset while direct-wave transform is better for large-offset. This “clarification” has been added to the manuscript (see appendix line 695 to line 698).

page 15, line 57: ”..to inverse crime.” the sentence sounds strange.

Indeed...

<u>Figures</u>

***Figure 2*: This figure is not referenced in the text. There is also no explanation of why the traces lack in the middle (closest to the source). Furthermore, the images all show glitches at different offsets which are not apparent in for example figure 6 (c). It would be nice to explain in the manuscript text what is shown here.The revised manuscript GJI-16-0524.R1 by Damien**

Pageot and coauthors entitled now as “Improving the seismic small-scale modeling by comparison with numerical methods” has been modified carefully to enhance the original submission. The authors addressed all points raised by me and another reviewer, improving the clarity and structuring of the paper. In this revision now, the authors clearly point out the new contributions and manage to present a nicely structured paper.

With this new revision, I would only recommend to slightly polish the text. Some small typos and writing mistakes made it into this revision which would be good to see corrected in a published version

.

• **page 3, line 74: ”MUSC (Mesures Ultrasonores Sans Contact in French)” why not drop just the ”in French”, the abbreviation is MUSC not MUSCF…**

It's done.

• page 5, line 119: ”in te” a typo, should be ”in the”

It's done.

• page 5, line 128: ”the specificities” ? more likely ”the specifics”

It's done

• page 6, line 161: ”piezzo-electric”, you also use ”piezoelectric” and ”piezo-electric”. Please only use one version (probably ”piezoelectric”) throughout the text.

It's done.

• page 6, line 161: ”For the linearized wave equation, ...”, the paragraph becomes too long. Please consider splitting it up and separate the thoughts.

• page 10, line 287: ”polynomial degree n l + 1 = 5 ...”, maybe better just state ”polynomial degree
n l = 4”

It's done.

• page 14, line 443: ”are closed”, a typo, should be ”are close”

It's done.

• page 14, line 443: ”and confirmed”, a typo, should be ”and confirm”

It's done.

• page 15, line 473: ”In the previous ...”, again a very long paragraph. Please consider splitting it up and separate the topics.

• page 36, figure 10, caption: ”cc gives ..”, please specify if the cross-correlation values are for black–red or black–blue comparison

It's done.

• there are some more sentences with some strange wording, please double-check again.

Figure 2 shows three multireceiver acquisition for the BiAlt model. The traces lack in the middle is related to the position of the laser over the piezo-electric source so that no trace can be recorded. The glitches on figure 2 which are not present on other figures are related to interferometer wear during acquisitions shown in figure 2.

***Figure 3 and 4*: The figures only show the BiAlt model. It would be great to also show the first model used which is the homogeneous one next to it, in both figures. This would make it clearer that the study is conducting experiments on two different models.**

Done for figure 3, figure 4 from the previous version has been removed.

Figure 9: there is no need to additionally label the image with (a) or reference it as Fig. 9(a). It then is just figure 9.

It's done. Figure 9 is now Figure 8.

References

Please note that in many references on page 17 - 19, the usage of 2D, 3-D or CO2 became 2d, 3-d and co2 and would need to be corrected.

It is done.