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 the MUSC approach (3D) seems better than the 2D approach using non-dispersive Lamb waves has been added (see remark of reviewer 2 **1.3**).

Text was added **Lines !** :
« 2D small-scale seismic laboratory exist (Oliver et al. (1954); Angona (1960); Mo et al. (2015)) and have several advantages compared to 3D laboratory: (1) the variety of materials which can be anisotropic and which are less expansive and easier to handle contrary to sometime huge 3D epoxy-resin models or (2) much less source energy requirement (Oliver et al. (1954); Mo et al. (2015)). However, this method is based on the propagation of pseudo-longitudinal waves which are, in fact, non-dispersive Lamb waves while 3D methods propagate the full wavefield through the model without proxy for propagation velocities and wavetypes. More, 3D methods are best suited to reflection, refraction and diffraction problems than 2D methods (Angona (1960)) which are critical in high-resolution seismic imaging. »

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.

Lines ! This part has been rewritten, completed and reorganized to present main aims of this work.

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. are mainly to validate the MUSC laboraty as a reliable tool to reproduce field acquisition. Here, we wish to show, beyond the improvement of physical measurements, the need to bring physical modeling closer to numerical modeling. Both have advantages and limitations but they are complementary and can lead to interesting results if they are used together. Validation of 2D-FWI algorithms, validation of 3D modeling using homogenized numerical model or validation of a new methodology based on numerical tests are some examples of the importance to have reliable complementary methods.

Some parts of the introduction have been rewriten to clarify the objectives of our paper and some references have been added (see 1.1 and responses to reviewer2 1.2)

<u>1.4</u> 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.

<u>2-METHODS</u>

<u>2.1</u> 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 :
Lines ! « As shown on figure 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**.

<u>2.2</u> 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 was 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.

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).

I am not sure to understand the remark. However, the objective of this paper is not to demonstrate via an application the efficiency of a method but to propose a solution to the validation problem of 2D imaging methods that require real/experimental 2D data. I think that the correlation coefficients given in this paper are a sufficient indication of the quality of the data for use in 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**).