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

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.

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... 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".*

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. 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). 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.

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.

(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).

(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.

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.

Review of manuscript GJI-16-0524 (2016-08-07)

This manuscript deals with the use of ultrasonic techniques and laboratory experiments for the development of seismic imaging methods. The subject is of great interest (for various applications in geophysics) and appear to be suitable for submission to Geophysical Journal International (GJI). I think that the authors have perfectly suited tools, numerical and laboratory data. But both the aim and the outline of the presented paper appear too confused to me and an actual application should be presented.

The abstract of the manuscript seems too long and is mainly introductory. It does not clearly present the objectives of this work and only suggests very general results.

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.

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. Another disappointing point is the fact that only two references are given. This is far from enough for a scientific article (even if the subject seems to mainly deal with well known methodological and technical aspects of seismic imaging). The first reference of this section is the contribution of Bretaudeau et al. (2011) who introduced, as a "A tool to validate numerical modeling and seismic imaging methods", the equipment and experimental setup used by the authors in this manuscript. 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.

Then, the authors present the main technical and methodological questions they intend to address only with one reference to support their ideas (which is way too few as I wrote above). 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.

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...) ?

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.

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.

The authors mention, at the end of the introduction, another study they performed about the reproducibility of their experimental source. Here again, I understand this issue is important in physical modelling (if one want to claim experiments to be controlled, the reproducibility of the source can be mandatory). However, there are no references about this problem and one does not clearly understand the link with the other parts of the manuscript.

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 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 ? 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). 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.

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.

Along with section 3 (RESULTS) comes an actual presentation of the main issues

addressed in this manuscript, with associated references. But here again, a lack of appropriated references remains, more particularly about the use of line-sources that could have been found in the articles the authors cite. It seems important to give more details about theses aspects and to show how it helped previous authors interpreting real data.

Anyway, 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.

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). 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.