August 26, 2016

to: Geophysical Journal International

Dear Lapo and Authors:

The manuscript GJI-16-0524 by Damien Pageot and coauthors entitled "Refined experimental studies for improving the reduced-scale physical modeling of seismic subsurface measurements" highlights challenges in reconciling physical and numerical modeling for geophysical prospects. The authors demonstrate the validity of scale model data by examining closely the reproducibility and apparatus response for high-quality reduced-scale measurements by the MUSC laboratory. They compare multi-receiver and multi-source traces with numerical SEM synthetics, with special consideration of geometrical spreading corrections and point-source versus line-source approximations.

The manuscript is interesting and relevant for GJI readers as it highlights the challenges and limitations of both physical and numerical modeling techniques. The manuscript takes special care of validating and presenting geometrical spreading corrections and line-source approximations. However, this submitted manuscript must further highlight its new contributions with respect to the previously published conference paper of the same title and authors at the 21st European Meeting of Environmental and Engineering Geophysics, Near Surface Geoscience 2015 meeting in Turin, Italy.

I suggest to publish this manuscript after further changes, where in the following I list general and more detailed remarks which would be great to see addressed in a revised version.

General remarks

- *Title:* Please change the title to a new one, not using the same as for the conference paper published in 2015. I have read your conference paper which is briefly touching line-source approximations and reproduciblity as well (and additionally discusses reflections and surface wave measurements). You could use references to that work where appropriate, but also highlight in this study what has been further refined and extended in the experiments conducted here.
- *Introduction:* The introduction is well written, however it would be great to further structure it and highlight the main contributions of this study. As example:
  - 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.

    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). 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.

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.

  - On page 3, 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.
  - 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).

- *Methods:* 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).

  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 François 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.

  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 $\pm 10 \mu m$ which for a shortest wavelength of $1 mm$ amounts to a precision of 1%. So does the vertical offset measurement by a laser beam with a $20 \mu m$ 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.

  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.

- *Results:* The manuscript is sometimes a bit confusing of what specific topic is treated and what experiment was taken for comparisons. 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. I got confused when reading "experimental 2D-results" (section 2.4), "2D measurements" (section 3.1) or "experimental 2-D data" (section 4), when in principle all experimental measurements are 3-D. You do re-construct a 2-D line-source in the experimental setup by summing up single 3-D point-source measurements. There is no direct two-dimensional measurement when doing the physical modeling experiment.

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.

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. It would also be great to plot close-ups just showing a single phase (P,S,PP,PS) for example for figure 7,9 and 10 to better show differences. It is also hard to distinguish blue and green curves in figure 6.

- *Conclusions:* It would help to further highlight the main findings in this study. 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?

  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.

More detailed remarks

Reading the manuscript, I noticed some small typos in the text which are listed in the following. Also, I add short comments to some points I found would be great to clarify in a revised draft.

Manuscript text

- *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.
- *page 2, line 49: "algorithms innovations"* just use e.g. "algorithms".
- *page 3, line 56: "sharp similarities"* maybe better "close ..".
- *page 4, line 33: "It is based on the following findings."* awkward ending of a paragraph. the following paragraphs would also be great to re-structure.
- *page 4, line 38: "3D/2D geometrical spreading effects"* it would be clearer saying "3D-to-2D geometrical spreading corrections".
- *page 4, line 56: "specificities"* ? maybe "specifics"
- *page 5, line 56: "GLL quadratic integration"* quadratic is wrong, could be omitted
- *page 6, line 56: "Since the wave equation is linear"* the sentence sounds a bit odd, maybe "For the linearized wave equation, .."

- *page 8, line 24: "finely"* ? could be omitted
- *page 8, line 29: "echoes"* ? maybe "reflection"
- *page 9, line 45: "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.
- *page 15, line 57: "..to inverse crime."* the sentence sounds strange.

  Please note that the term "inverse crime" was coined in the book by Colten, D. & Kress, R. "Inverse acoustic & electromagnetic scattering theory", 1992, Springer, Berlin - not by the paper cited of Wirgin (2004). The problem itself was recognized by many others earlier on already. As a reference one could for example look at Tarantola, A. & Valette B., "Inverse Problems = Quest for Information" (1982, J. Geophysics, 50, p. 159 – 170) where the error on theory (which includes approximations on physics and model parameterization as well as numerical discretization errors) is considered as well in their probabilistic setup of the inverse problem. Often however the error on theory (such as when using the same forward solver for the inverse problem) is neglected or assumed to have a minor effect on the imaging result.

  To me the term "inverse crime" sounds more like a populistic expression rather than a scientific term and therefore would reduce usage of the term. While it certainly points to an additional complication when dealing with inverse problems, there is no "crime" in setting up the inverse problem under certain assumptions (which often are left implicit unfortunately). It's up to the authors of this manuscript to use this term. I would prefer having it stated in clear words that using scale model data avoids these intrinsic problems of numerical benchmarks and physical modeling is therefore very important to conduct.

Figures

- *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.
- *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.
- *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.

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.

Reference to Festa, G. & Vilotte, J.: the paper title starts correctly with "The Newmark scheme as .."

As a summary, I think the work presented here is very interesting and will lead to very useful seismic experiment studies. However, it would be great to see an improved manuscript to make the author's contribution clearer and to set it apart from the earlier conference paper. I therefore recommend a moderate modification to the manuscript to give some more time to polish it before publication in GJI.

Sincerely,

Daniel Peter

References mentioned:

Oliver, J., Press, F., and Ewing, M. 1954. *Two-dimensional model seismology*, Geophysics, v. 19, p. 202.

Angona, F.A. 1960. *Two-dimensional modeling and its application to seismic problems*, Geophysics, v. 25, p. 468–482.

Healy, J.H., Press, F. 1960. *Two-dimensional seismic models with continuously variable velocity depth and density functions*, Geophysics, v. 25, p. 987–997.

O'Brien, P.N.S. and M.P. Symes 1971. *Model seismology*, Rep. Prog. Phys., 34, pp. 697–764.

Pant, D.R., S.A. Greenhalgh, S. Watson 1988. *Seismic reflection scale model facility*, Explor. Geophys., 19, pp. 499–512.

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