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Solid Earth
Discussions

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Interactive
Comment

Interactive comment on "Exploring the potentials and limitations of the time-reversal imaging of finite seismic sources" *by* S. Kremers et al.

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Dear editor, Dear reviewers,

Thank you very much for your comments and constructive criticism concerning our manuscript. We improved the text, modified figures and included new references, according to the reviewer's suggestions.

When we initiated our study on time-reversal imaging for finite sources, we were very confident to find positive results – also because of the reviewer's encouraging work on the global scale and for effective point sources. However, after a large series of experiments with different setups and imaging conditions, we had to accept that the time-reversed wavefield in the near-fault region is not able to characterize finite-source processes better than classical kinematic source inversion under typical conditions (station density, source receiver geometry, frequency content, etc.). This result clearly needs to be communicated to the seismological community.

We fully understand the reviewer's skepticism, but we also note that they could not suggest a feasible remedy: sufficiently complex media with multiple-scattering characteristics proposed by Jean-Paul Montagner cannot be available due to the limited resolution of seismic tomography; and the different imaging conditions mentioned by Brad Artman have already been tested, as we clearly stated in the original version of the manuscript.

We would not wish to publish our results without being convinced of their robustness. With full-waveform time-reversal imaging – again under typical conditions for kinematic source inversion – it will be difficult to reveal the details of finite-source processes. As adjoint based waveform inversion for structure and sources is becoming increasingly popular, we are convinced that our experiments will contribute to the understanding of how to proceed in the direction of source inversion.

Please find below a summary of our response to the major issues, followed by a more detailed discussion:

Summary

Complexity of the medium: We fully agree with reviewer 1 that time-reversal imaging benefits from complexity, as impressively shown in laboratory experiments. In seismology, however, we have no precise information concerning sub-wavelength heterogeneity that could be sufficiently strong to enhance the focusing of the time-reversed field. This is because the resolution length of seismic tomography is significantly larger that the wavelength of the seismic waves. Choosing a comparatively smooth medium that explains the travel times of the major arrivals is therefore the best we can do. A complex model with multiple-scattering characteristics would be more unrealistic than a smooth model. We elaborate on this issue in the revised discussion section. To make this point more clear please see the changes in the manuscript on page 1 line 35-38, page 2 line 116-123 and page 6 431-449.

Approximation of the Greens function: Closely related is the approximation of the Greens function that may not be sufficient. However, for an improved Greens function we would require more precise information on 3D heterogeneities that we do not have in reality. Also, as the synthetic tests show, the approximation of the Greens function does not lead to the failure of the time-reversal for finite sources, which has more profound reasons. Nevertheless, we included the issue of Green function approximation in our discussion.

Imaging conditions: Quantitative imaging conditions are certainly important. However, as we already noted in the original version of the manuscript, a large variety of functionals of the time-reversed field, i.e. imaging conditions, did not lead to significant improvements. The reasons why time-reversal imaging fails to characterize finite sources – discussed in detail in section 6 – are so fundamental that they cannot be repaired with imaging conditions.

Noise: Certainly, noise acts to deteriorate the time-reversal images. However, noise was not present in our idealized synthetic experiments. In the case of the real data of the Tottori earthquake, the recorded displacement was so large that the influence of noise was entirely negligible.

References: We added all the references suggested by the reviewers.

Seismograms: We now show some of the vertical component seismograms used in the real data inversion in figure 11.

Inversion for a finite source: We highlighted our intention to perform time reversal for finite sources and the choice of the associated frequency band commonly used in kinematic source inversions (e.g. page 2 line 118-120).

Detailed response to reviewer 1 (J.-P. Montagner)

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While pointing out that our manuscript is an “interesting investigation on the limitations of the time-reversal method (TRM)” and a “serious piece of work”, the reviewer makes the strong statements that the authors have missed the “essence of the interest in TRM” and the “real basic reasons why TRM does not work for their experiments”. Reviewer 1 has raised several interesting points about the manuscript that we improved in the revised version. However, in a similar tone, we think that the reviewer missed the specific objective of our work which did not aim at an overall negative conclusion concerning the applications of the TRM! Therefore, we would like to briefly restate our specific goals:

The conditions for the simulations were not chosen by ignorance but for specific reasons:

We chose a frequency range and source-receiver setup that is commonly used in kinematic finite-source inversions with real data. The structural model is smooth because most parts of the Earth – with few exceptions on very small scales - are in fact sufficiently smooth to avoid the occurrence of scattered waves that are exploitable for time-reversal imaging. Even if sufficiently strong heterogeneities existed, we would not be able to constrain them with seismic tomography, and the high-frequency scattered fields would be attenuated quickly by dissipation. Furthermore, the use of smooth models is common practice is all seismic source inversions – without any exception. We investigated the TRM under these conditions because mathematically a first iteration for source updates is equivalent to a direct application of the TRM.

In addition - as carefully laid out in the manuscript but never commented by the reviewer - our synthetic experiment has to be interpreted in the context of the SPICE kinematic source blind inversion test that illustrated the large uncertainties in the context of kinematic source inversion. The modest goal of our synthetic study was to investigate - despite the obvious limitations of the setup - how far TRM brings us under ideal conditions in the mathematically obvious first step, which consists in the simple time reversal of the observations. In that sense we get the impression that reviewer 1 missed the context of our study, while correctly pointing out the limitations in the overall context of TRM.

In the following, we respond in more detail to the reviewer's major comments:

Style of the introduction: The reviewer's statement that "the introduction is very poorly written" is very subjective, and not specific reasons for this statement are given. We agree that one could have written an introduction that outlines the historical developments. Instead, we have chosen to emphasize the universality of the time-reversal concept – and we think that this is a fully legitimate choice.

Also, the reviewer seems to regret that we did not explain sufficiently well what he refers to as "TRM thrives through complexity". In fact, this we did on purpose. We are well aware that the success of time reversal is proportional to the complexity of the medium. This aspect, however, has little relevance in seismology because the Earth is smooth compared to the extremely heterogeneous media used in acoustic lab experiments. We will expand on this issue later.

Approximation of the Greens function: We fully agree that one reason for the failure of the real-data TRM is the approximation of the true Greens function that results from our incomplete knowledge of the true 3D Earth structure. We point this out more clearly in the revised version. Nevertheless, one should note that poorly known Earth structure is a priori excluded as a reason for failure in the synthetic tests. Thus, even if we knew the Earth structure perfectly well, the TRM still would not work. For the real-data application one should keep in mind that the insufficiently known structure poses problems only when the model is so inaccurate that even the arrival times are not explained. This is because most of the focusing is due to the ballistic waves, and not to the scattering that is too weak to contribute significantly. In the case of the Tottori example, the model indeed explains travel times very well, which explains the occurrence of a clear focus.

Smoothness of the medium: The reviewer states that "the propagating medium is dramatically smooth and the frequency range not well suited for getting details on the fault

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(...) wavelength of 20km (...).”

Yes, indeed, the medium used in our experiments is “dramatically smooth”, and a more complex structure would have led to better results – comparable maybe to acoustic lab experiments. However, the use of a sufficiently complex medium would be unjustified for several reasons:

(1) We have no constraints on heterogeneities that are both sufficiently rough and strong to generate a significantly improved focus. Our knowledge on the Earth’s 3D elastic structure is almost entirely based on seismic tomography. The resolution lengths of seismic tomography are – in most realistic cases – on the order of a few wavelengths. This completely excludes the robust estimation of sub-wavelength heterogeneities that would be capable of generating strong scattered fields. While this would technically be possible, we cannot implement complexity that we do not know at all.

(2) In most regions of the Earth we have no indication that the Earth is sufficiently complex for the TRM to be more successful. Seismograms at periods that we can both understand and model are usually well-behaved, with little influence from scattered waves. Therefore, most of the focusing will always be due to the ballistic waves. Of course, there are exceptions, including for instance Precambrian cratons where strong scattering can indeed be observed. However, in order to come to reasonable conclusions, we have to assume the common and not the exceptional.

(3) Even if strong scatterers were present in the Earth, their scattered waves would be rapidly attenuated by viscoelastic dissipation.

Of course we know that a complex medium would improve the TRM but the important point is that in kinematic source inversion scattering characteristics that would considerable improve the imaging (in the near fault area) are basically never known! Often homogeneous half spaces or simplified layered structures are therefore assumed. Our period range was up to 2 s, so we could expect focusing up to wavelengths corresponding to shear waves (7 km not 20 km) which - given the size of the fault (dozens of km)

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- might still lead to an acceptable focusing. The most important point is that it would have been unfair - given the uncertainties of real data inversion - to simulate a strong scattering medium with the scattering structure known!

Adjoint method: Concerning our statement that the adjoint method is mathematically rigorous compared to time reversal: Time reversal sensu stricto applies to differential equations that are invariant with respect to a sign change of the time variable. From there, it is quite a big – and in fact purely intuitive – jump to time reversal in the seismological sense. There is a big difference between the backward propagation of a complete 3D field and the backward propagation of a field that is excited by point sources at the surface. The link between the two is intuitive. The adjoint method, in contrast, is a mathematically sound concept for the computation of Fréchet derivatives.

Focussing in the synthetic experiments: As already explained, we agree with the reviewer that good focusing, i.e. the absence of strong ghost waves, relies on the complexity of the medium. However, we cannot simply invent an Earth that suits our purpose.

Application to the Tottori earthquake: We do not agree with the reviewer's statement that the "application of TRM to Tottori earthquake is a complete failure." There is a clear focus visible. In particular the quiescence at "negative" times is indicative that the kinematics of the back-propagating wave field cannot be completely wrong!

Discussion: We do not agree that the discussion needs to be rewritten, because we supposedly did not explain why the TRM does not work for finite source imaging. As already explained several times, there is no freedom to choose the physical conditions. We cannot invent a complex Earth model that is not constrained by data. Therefore, the statement that our experiments partly fail due to "the poor chosen physical conditions" is not correct. The physical conditions are not chosen, they plainly exists as they are.

Incomplete information: We agree with the reviewer that there is insufficient information in the seismograms due to the insufficient complexity of the Earth. But again, that is how the Earth is made and how we are able to see it.

Missing sink and iterative improvement: Indeed, the next step would be to go from the simplistic and intuitive time reversal to an iterative finite source inversion. We already mention this in the discussion. Clearly, this is future work that goes far beyond the objectives of this study.

Detailed response to reviewer 2 (B. Artman)

In his very short statement, reviewer 2 claims that our manuscript is “a bit incomplete” – unfortunately, without being very specific. Also, most of the references that the reviewer suggests to include, are not obviously related to the problem that we studied. We therefore belief that the very objective of our manuscript has been misunderstood.

Overly pessimistic? We do not think that our results are overly pessimistic. In fact, we started our work with the expectation that the outcome would be very optimistic. However, in the course of this study – and after endless tests with weighting schemes, imaging conditions and receiver geometries – we had to come to the conclusion that time-reversal imaging is not well suited for the characterization of finite sources under the conditions employed.

Imaging conditions: We fully agree that automatic imaging conditions are important – and this is why we explored many of them. As we already mention in the original version of the text, a large variety of functionals of the time-reversed field, i.e. imaging conditions, did not lead to significant improvements. In other words: imaging conditions cannot enhance information that is not present at all. Also, we provide evidence (station densification, small station arrays) that improve the situation.

Point sources vs. finite sources: In reference to his own work (Artman et al., 2010) the reviewer does not mention that it was concerned with effective point sources. As we have shown as well, there are hardly any problems with time-reversal imaging when the source is small compared to the wavelength, i.e. a point source. We really want to stress that the difficulties that we encountered are due to the spatial extent and variability of the source.

Noise: The reviewer suggests to include the reference to Witten and Artman (2011), which is concerned with the influence of noise. First, we note that most of our experiments used synthetic data without any noise. Second, in the Tottori data, noise is not an issue at all, because the earthquake was so large that the recorded displacement was far above the noise level (please see also the newly included figure 11).

Surface waves in the Tottori data: It is true that surface waves can have deteriorating effects on the time-reversal images. However, one must distinguish vertical and horizontal resolution. Horizontal resolution near the surface actually improves as surface wave data are added. This is why the back-propagated wavefield converges towards the source of the Tottori earthquake. Vertical resolution, in contrast, suffers from the dominance of the large-amplitude surface waves that tend to mask the body waves converging at greater depth. In the original manuscript we already made this subtle but very important distinction.

We hope that we were able to respond appropriately to all the issues raised by the reviewers.

With kind regards

Simon Kremers
Andreas Fichtner
Heiner Igel

Please also note the supplement to this comment:
http://www.solid-earth-discuss.net/3/C190/2011/sed-3-C190-2011-supplement.pdf

Interactive comment on Solid Earth Discuss., 3, 217, 2011.