Solid Earth Discussions

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Comment on se-2021-122

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Referee comment on "3D high-resolution seismic imaging of the iron-oxide deposits in Ludvika (Sweden) using full-waveform inversion and reverse-time migration" by Brij Bhushan Singh et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-122-RC1, 2021

GENERAL COMMENTS

The article presents a study using seismic data for a minining prospect/site in Norway. The study focuses on applying 3D full waveform inversion and migration to the dataset. To that goal they use a traveltime inversion software and then produce different models and migration images. They conclude that RTM images using FWI models are better than those resulting from constant velocity models or tomographic models. They then proceed to interpret their findings and relate them to prior knowledge of the study area. The main result is a good fit between the mineralisation horizon and a reflector present in the images.

The article is overall well written and the methodologies are clearly explained. The bibliography is sufficient and figures show good quality for publication.

SPECIFIC COMMENTS

This is an ambitious exercise that aims at using state-of-the-art imaging algorithms to an onshore dataset. In particular, using FWI for onshore data is difficult and typically results in a lot of trial and error in order to obtain good convergence. The paper is rather clear in explaining the choices made and which ideas resulted in worst results. In any case, several concepts are a bit obscure and may need clarification by the authors. Here follows a list of them.

The first obvious question is the choice of algorithms. FWI is an expensive imaging tool. Its use should be justified only of other methodologies fail or are not available. In the manuscript this is not clear. As it is written, the method is a given of the manuscript, but as no novel methodological approaches are presented or benchmarked, some more discussion on the choice of the method would be welcome.

Furthermore, the authors choose to use acoustic, constant-density FWI for onshore data. This is rather hard to understand, in particular given the lack of long-offset fit that could justify relying on direct P-arrivals and hence benefiting from acoustic FWI. As the results seem to confirm, the inversions mostly affect near-offset structures and might be driven by reflections. In this case, the acoustic approximation fails at reproducing the amplitudes and AVO of data. Some in-depth justification is due in this regard.

Yet another topic not fully covered in depth in this study is the shallow model. In onshore data, local heterogeneities can be large at the first meters. No static corrections seem to be applied in the present case, which seems like a good idea, but one would expect special attention being paid to very near offsets in order to get an approximation of the small-scale shallow model. Ideally, one would invert for surface waves (elastically, that is) or produce an initial model based upon very short offsets. My impression is that the authors ignore these effects and these are “collected” in the wavelet inversion. Such wavelets have quite noticeable amplitude and delay differences with each other. This solution thus potentially averages local effects from both sources and receivers.

Something noticeable from Figure 2a is that several anomalies correlate with the acquisition geometry. This might be actual or an artifact of the inversion. Most probably of the regularization used in the FAT. Perhaps the authors could give details in this aspect.

It is unclear throughout the manuscript which norm or cost function is used. Probably it is the L2 norm, the most common in FWI, but this should be clearly stated in the document, together with any specifics used in this respect (e.g. windowing or amplitude normalization).

Given the irregular acquisition geometry of the data, I believe that a resolution test would help in determining which parts of the models can we expect to resolve in optimal conditions. It seems to me that some of the deepest parts of the model obtain are being overinterpreted. As we are missing a resolution test, all parts of the obtained model are considered equally resolved and this is misleading.

Another aspect that seems lacking is QC prior to the inversion. Several traces seem prone to cycle skip (e.g. Figs 7 b-d), and a few times throughout the text we are told that long-offset traces cannot be correctly matched. Nevertheless, such traces seem to be part of FWI, which seems like bad practice. Such data cannot help convergence and as such should be removed from FWI. The traces could be used for posterior QC (as in your RMSE visualization) but should be removed from the inversion.

Regarding the long offsets, it seems like a lot of computational effort is used in keeping full offsets (i.e. shotgathers are large in the lateral dimensions) for FWI shots but no benefit is obtained from such traces (see Fig 9 for example). In fact, for elastic FWI there is previous work suggesting that removing such offsets can result in better convergence, both in data and model space (see Kormann et al 2017, *Comp. Geosciences*, for an example). Figure 9 seems to suggest that long offsets do not contribute at all.

Regarding RTM I have just few concerns. The first is that you seem to keep direct waves in the migration process, which cannot be migrated. You later filter the images to remove the artifacts, but it would be better practice to remove those direct waves from the very beginning.

Furthermore, in migration we would expect to see some QC in terms of common image gathers or other gathers that can help discern whether the inversion process is correct. This in fact is a QC for the inversion process as well, one of the few QC that can be applied throughout the domain. We expect gathers from FWI to be flatter than those obtained from FAT or a homogenous model. Some effort in this direction would strongly help improve the confidence of the reader with the results.

Last but not least, and given the simplicity of the models used, RTM is hard to justify with respect to other cheaper migration techniques, either pre- or post-stack.

SUGGESTIONS / COMMENTS

My comments mostly go in the direction that, perhaps, with other decisions in the parameterization and data selection, other results could be obtained, maybe better ones. This does not demerit the results presented, which are interesting and worth reporting, but leaves me wondering if more could be obtained from the data. In the discussion there are some ideas which are interesting, but for the manuscript to feel more complete, some extra effort would be welcome in addressing some of the issues presented above.

My only strong suggestion is a better analysis of the results. Judging the inversion and migration as successful just based on partially better coherence in some reflectors and fit between model and image or image and a single prior structure seems insufficient, given the effort made in producing those models and images. I suggest CIG or alternative methods to compare coherence of the image with respect to offsets or angles at a wide range of locations and depths.