Anonymous Referee #1

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I congratulate the authors on a very nicely presented study. The work is relevant to both local community and a more general scientific community worldwide. The research topic has been well introduced, previous research discussed and relevance of the study clearly highlighted. This was done in very systematic and easy to follow manner.

My main concern with the manuscript relates to the data processing part. This part needs to be redone with respect to detailed comments in the pdf file attached. In the current state of the manuscript, part of the reflectivity seen on the stacked sections looks real, while parts more appear as processing artifacts. This needs to be corrected and I have left specific comments in the pdf file attached.

When it comes to the data interpretation and discussion, both have been done in a very well structured, comprehensive and systematic way. The findings have been thoroughly scrutinized and scientific relevance clearly shown.

Conclusion follows a logical tread with clearly summarizing the main findings of the study and showing its relevance. Same is true for the abstract.
Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2018-22/se-2018-22-RC1-supplement.pdf

Author: Many thanks for the suggestions! In fact the manuscript should not focus on the data processing, instead of the experiments, results, and main findings. The description of the processing is now improved widely following the suggestions. Where not, explanations are carried out below.

On the other hand the often used reviewer wording „look real" or „seems real" or „looks like a processing artifact" or not is a subjective statement only. Such subjective statements cannot be cleared by any argumentation, it can be applied every time. It is not constructive and claims in a hidden kind the authors have manipulated the data or carried out a non professional processing.

This is seriously not the case, I'm involved in shear wave reflection data processing more than 15 years by >100 km profiling. Some of the reviewers arguments show that he/she is not familar in shear wave reflection data processing. But I'm very happy of the detailed processing comments, suggestions and questions.

Content of the supplement:
111
We further show that shear wave reflection seismics has advantages to study highly porous, partly saturated alluvial fans with complex compositions
This is a rather dubious sentence. Shear-wave seismics has advantages over what, P-wave seismics in the same geological setting or compared to the refraction surveys of El-Isa et al. (1995) and Sawarieh et al. (2000) or MASW by Bodet et al. (2010)? If it is the latter, the comparison is not valid.

Author: In this geological setting shear wave reflection seismic is superior to
   a) P-wave reflection in terms of resolution, ~ 1.5 m for S-wave, 5-6 m for P-wave.
   b) In general the reflection method is superior to the refraction method because it handles velocity inversions, where the refraction method fails.
   c) MASW because it can handle lateral inhomogeities, while the MASW inversion algorithms are from definition restricted to lateral homogeneous (1D) media only.

Here I don't understand why the comparison is not valid, because there is no argument to the statement.

253
Typically, two records were gained at each source location and stored separately.
I assume that the authors here are referring to one positive (+) and one negative (-) polarity? If so, this should be mentioned.

Author: included.

265
A variable split-spread source-receiver configuration was applied to enable geometry optimization for the detection of dipping structures, and to facilitate workarounds due to obstacles in the profile track.
This needs to be clarified. A split-spread geometry would imply that your source is located in the middle or around the middle of your receiver spread. Figure 3 shows that the source is located at the end of the landstreamer, which is violating what is stated in the text with split-spread geometry. Has the landstreamer been moved after every shot location by 2 m while the source was kept in the middle of the receiver spread or the source was kept at the tail of the landstreamer? Or was there any other source-landstreamer configuration involved?

Author: Naming „variable split-spread" include all source position variations within the receiver spread (not only in the middle or around) and is contrary to a fixed source-receiver configuration. Even if one source position of a profile is at the beginning (Fig 3a) and another at the end of the receiver setup while all others are variing within the receiver setup (Fig 3b), it is a variing split-spread configuration, not off-end or a fixed configuration where source and receivers move up the same interval. It is also clear from the records shown in Fig 5 and 6 that the source-receiver split configuration is always variing.
For clarification,we included a reference where the process is explained in detail.

270
Without differential GPS corrections, the final positioning of the profiles from the 2013 campaign required laborious optimizations by manual corrections based on the known distances along the receiver units. Reliable elevation data could not be restored.
What would be the estimated accuracy of the source and receiver coordinates after the manual adjustment?

Author: relative to the profiling less than 5 cm, absolute 0.5-1 m. We included this.

284
Examples of recorded shot gathers
I would like to see the corresponding amplitude spectra of the shot gathers shown in Figure 5 and also Figure 6. This would give the reader an idea of the data frequency bandwidth and an overview of the vertical resolution of the data using the mentioned velocities.

Author: For all records spectra are now included. Spectra are very similar along the profile because it represents the vibrator source.

284
Bandpass filter (18-20-78-82 Hz)
The selected tapers both on high and low frequency ends of the spectrum are rather narrow. The narrow tapers can cause Gibbs phenomenon or the so called "ringing" in the data. Has that been seen in the data?

Author: That's correct, but there is no Gibbs effect visible in the spectra in Fig. 5 and 6 and it was also not observed during processing. There is no dominant frequency causing a ringing.

But there are harmonic distortions visible in some records in Fig 5 close to the source which reverb the love wave pattern. Due to the low velocities, this pattern disappears after stacking if it is only small. In extended cases, it was suppresed by Vibroseis Spectral Balancing followed by a FK-filter applied to shot gathers.

303
Examples of recorded shot gathers
Apart from the top reflection with an apex of ~150 ms, it is not that easy to see the other reflections using the variable density plots. Could the gathers have been plotted as wiggle plots? Also, the reflections shown at times greater then 200 ms show repetitive nature implying a possibility of "ringing" either due to energy staying trapped between some layers or a crosscorrelation problem. I would prefer to see the shot gathers and the corresponding amplitude spectra after crosscorrelation with only AGC applied without any filters applied.

Author: It would be the same difficulty using wiggle plots, but wiggle would require more plotting space. Plotting the same number of traces in wiggle would result in a quite black display. It is a highly scattering media and required multifold coverage for statistic S/N enhancement. There is no ringing or other kind of periodic signal visible in the spectra, there is no geology for energy trapping. There is also no problem during vibroseis correlation. Because it makes no sense to include such processing details as additional figures in the paper, I attached the gathers of Fig 5 correlated, AGC 220 ms, filtered and without filter as pdf to demonstrate that the differences are small. If you switch between the sheets you will see the small difference.

308
4. Data processing
The processed stacked sections shown in Figure 7 and following one show numerous "worm-looking" reflections and, in general sense, reflectivity patterns that do not have a clear justification and confirmation in the shot gathers, apart from a few selected ones. The wormy-looking reflections are more likely a processing artifact of the fk-filter (Baker 1999) or due to improper parameters in the FD migration algorithm then the real feature. The aforementioned, combined with the narrow bandpass filter tapers can cause additional ringing and result in introduction of false reflectivity patterns. This needs to be clarified by showing the shot gathers both before (after crosscorrelation, only AGC) and after processing (With all processing applied) so that the processing effect can be clearly seen on the data.

Author: I apologize but this comment is highly speculative. It can cause, but I'm sure that all the processing parameters are carefully tested and adjusted in a proper manner not to include filter and/or migration artifacts. To proof this all for this data set as suggested would result in a pure processing paper, which is not the intention of the manuscript. If you are interested in more processing details please read Polom et al. 2013. I included now Fig. 7 to demonstrate the the shot gather processing applied (only crosscorrelation and AGC and all shot gather processing applied) where Love waves disturbed the data and the estimated depth penetration. The „worm-looking" effect is generally due to the dense lateral spacing used for S-wave acquisition, and the nature of S-wave propagation in the matrix only, this is quite different to P-wave data.

310
The main data processing flow of the first iteration consisted of Vibroseis Correlation, Vertical Stacking of records, Geometry Installation, Amplitude Scaling, Bandpass Filter, Frequency-Wavenumber (FK) Filter, Interactive Velocity Analysis, Common Mid Point (CMP) Stacking, parallel applied Frequency-Space (FX) Deconvolution and Finite-Difference (FD) Migration. Why was the residual statics not applied? Unless a rather simple velocity model is used, the residual statics should further enhance the reflections. Also, the shot gathers shown in Figures 5

and 6 show clear first breaks as indicated by black arrows. Why were the first breaks not picked and used for refraction static correction, given their strong presence in the data? Why were the first breaks not surgically removed from the shot gathers? Skipping this step may result in them being stacked in the top parts of the seismic section. Please elaborate on this.

Author: Written above is „main data processing". Residual statics do not improve the result, because the resulting shifts of +- 3 ms max are too small to get a significant effect. This was tested at a lot of shear wave data sets and also proofed by other autors operation with shear wave reflection data (e.g. André Pugin, GSC). First break picking for refraction statics calculation does not work above an inverse velocity function situation due to operation on a paved road. This first breaks in the records are not common refractions, picking and inverting them to derive a refractor model would fail. Clearly the first breaks are removed by Top Muting (not Sugical Muting) prior to CMP stacking, I added this detail in the processing sequence now and it is shown in the new Fig.7.

333
Resulting depth section of profile 1b after post stack FD time migration (top) and interpretation of the main structure elements (bottom).
The reflectivity seen in the upper 100 m of the stacked section seems realistic. However, below 100 m, it needs to be justified by showing the events on the processed shot gathers.

Author: The nature of the few reflectors below 100 m in Fig. 8 remain speculative due to the missing borehole proof. They result from the statistic of 24-fold CMP stacking in mean, not from single shot gathers. Again the desire to proof in detail would result in a special processing paper. Fig 7 and Fig 9 clearly shows that events below 100 m are structural imaging instead of processing artifacts.

349
Finally, the reflection amplitude responses throughout all seismic sections represent reflection coefficients of nearly 0.1 or less, indicating materials with relatively low contrasts in seismically-sensitive material properties (elastic parameters and density) and hence in shear wave seismic velocity.
In the processing part you mention "Amplitude scaling" and in here you interpret the relative reflection amplitudes. Please clarify if AGC, trace equalization or spherical divergence correction was applied. If the AGC was applied, interpreting the relative amplitude strength is dubious.

Author: True Amplitude processing was not applied until yet, this would be again stuff for a specialized processing paper. Even only AGC 220 ms is applied for amplitude scaling, the sections show detailed amplitude dynamics resulting from constructive 24-fold CMP stacking. This is common also in hydrocarbon exploration for structure imaging. True Amplitude processing would be required if the target would be AVO analysis or inversion etc..

381
Resulting depth sections of combined profiles 2 and 2b-2 after post-stack FD time migration (top) and interpretation of the main structure elements (bottom).
Again, looking at the depths of ca. 100 m and below, the data seems to show migration artifacts. Have different migration algorithms been used and why was FD migration chosen?

Author: From my opinion again speculative, and again the kind of processing artifacts in charge is not specified. Except profile 2b-2 at the left (East) border, where some migration smilies disturb the imaging, FD migration did a good job. Different migration methods applied to shear wave reflection data were extensively tested since more than 10 years (see e.g. Polom et al. 2010 and 2013), and FD migration (67 degrees algorithm) was found to be a sufficient tool, superior to Kirchhoff post- and prestack time migration methods.

483
The results presented here show the advantageous imaging capabilities of the method regarding high-resolution structural analysis and depth penetration compared to common refraction seismic methods or common P-wave reflection analysis.
Unless referring to previous reports using the P-wave reflection method, this sentence and the entire comparison is not valid. Please reformulate the sentence by referring to P-wave reflection studies at the sites vicinity (if any), or remove it. It is not valid to compare S-wave reflection surveys with P-wave refraction tomography or MASW.

Author: The sentence has been modified by referring a P-wave reflection result shown in Ezersky and Frumkin (2013). In particular, the message of the sentence does not compare S-wave reflection with P-wave refraction tomography or MASW, no wording about this. Why this is valid or not is never stated in a paper until yet from my point of knowledge, the proof of this reviewer statement remains missing. From my opinion, each subsurface imaging method can compared by another.

567
The favourable velocity-frequency relationship of the resulting wavelets and the absence of pore fluid effects enabled a meter-scale resolution and a nearly 200 m penetration depth.
The results seen on the stacked sections seem reliable down to ca. 100 m. Without any deeper borehole, the strong reflectivity below 100 m needs to justified by either referring to reported studies in the site's vicinity indicating the nature of these reflections and confirming their presence in the processed shot gathers of this study to show that they are likely a real feature and not a processing artifact.

Author: I agree this depth estimation needs improved verification. Due to the absence of a reference borehole of sufficient depth in the surrounding area I added Figure 7 b) of processed shot records from Profile 2.

575
Combining the reflection amplitude responses
Have the true amplitudes been preserved during processing?

Author: see comment to 349

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This paper describes a good example of using S-wave reflection seismics in a setting where classical P-wave reflection would not have resulted in high resolution images.
The review criteria are assessed below. After that, suggestions for improvement of the paper are listed.
The review criteria are assessed as follows:
1. Does the paper address relevant scientific questions within the scope of SE? Answer: yes
2. Does the paper present novel concepts, ideas, tools, or data? Answer: yes, application of S-wave reflection seismics in difficult setting to image
3. Are substantial conclusions reached? Answer: yes
4. Are the scientific methods and assumptions valid and clearly outlined? Answer: partly, see comments below.
5. Are the results sufficient to support the interpretations and conclusions? Answer: yes
6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Partly, see comments below.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Answer: yes

8. Does the title clearly reflect the contents of the paper? Answer: yes

9. Does the abstract provide a concise and complete summary? Answer: yes

10. Is the overall presentation well structured and clear? Answer: yes

11. Is the language fluent and precise? Answer: yes

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? If present: yes

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Partly, see comments below

14. Are the number and quality of references appropriate? Answer: yes

15. Is the amount and quality of supplementary material appropriate? Not applicable, no supplementary material.

Author: Many thanks for the suggestions!

Suggested improvements for the paper: Section 2.2:
1. Line 183: typo 'experimentss'

Author: done

Section 3:
2. The target depth for imaging is not stated. Please explain why the chosen setup is suitable for the target depth.

Author: Proposed target depth 36-60 m is now included.

Section 4:
3. The readability of the paper would be improved if a table with processing steps and results of those steps were provided.

Author: I agree, processing sequence Is now provided in Table 1

4. There is Love wave energy present in the seismic data. Have you considered inverting these data (MASW) in order to obtain Vs information about the first tens of meters? In the overview of section 2.2 several MASW studies are reported. In the discussion it is stated that Bodet et al. (2010) reported strong lateral Vs heterogeneity. Yet you use only one mean Vs profile to convert the reflection data from time to depth domain. Knowledge about the amount of variability of Vs in the top tens of meters will inform the reader about the reliability of the reported depths.

Author: a) Inversion of Love waves can only provide reliable velocities up to max 30 m using this recording setup (0.3 of the spread length).The geophone frequency of 10 Hz would further reduce this depth to nearly 15 m. b) The Love waves here are propagating as subsurface channel waves (similar to in-seam waves) in the shallow subsurface, not as true surface waves. Using a common MASW inversion designed for Rayleigh waves (totally different propagation behaviour) does not fit this wave type, and the lateral velocity and structure variation (e.g. visible in Fig 5 first breaks) does not fit the requirements for MASW inversion (1D layer case). The reported depths are now supported by a processed record example.
More detailed velocity results are planned for further papers, which also focus on stratigraphic interpretation.

5. Line 326: "to derive final depth sections by using mean 1D velocity-time function". How was that derived? The explanation follows much later (lines 347-349). It Is clearer

when this section is moved up and a couple of more lines are spent on the explanation. It is too short now.

Author: I agree, it's done.

Section 5:
6. Figures 7 – 10: In the text there is reference to certain positions along the lines, but the horizontal distance is not clear in the figures. There seem to be numbers like 200 250 300 in the figure, but rather hidden in the portion above the depth sections and fonts too small. Please add a clear horizontal distance axis in each figure.

Author: I agree, it's done.

7. Figure 9: boreholes BH1 and BH2 are too far away (420 m) from the line. I would not show them in figure 9, no added value. To show them in figure 8 and 10 (200 m away) is already on the limit of preferred. 420 m is really too far off.

Author: I agree, it's done.

8. Blank line needed after line 442. Move the next line about Figure 11 to below Fig 11 to be in the same paragraph as the sentence starting on line 455.

Author: I agree, it's done.

Section 6:
9. Line 505 states that the internal structures such as topsets, foresets and indications of bottom sets are present in the seismic depth sections. It helps the non-geologists reading this geophysical paper if these are indicated in the bottom parts of figures 7-10. And it helps the geologist to recognize these in the geophysical data.

Author: I agree, it's done.

10. Line 548: use of only one 1D Vs profile. Pleas elaborate on why you think this would be a valid approximation even if Bodet et al. (2010) reported strong lateral Vs heterogeneity. Or support this by MASW results for the observed Love waves in your data.

Author: as explained above in 4., Love waves are not helpful in this case. Using a mean 1D velocity profile derived from the data itself prevents to project velocity artifacts (e.g. from irregular ray pathes) onto the depth sections. This is the most reliable time-to-depth conversion if no other velocity information is available. In fact, the strong lateral velocity heterogeneity reported by Bodet et al. (2010) crashes the reliability of his MASW result, since MASW inversion is only valid in a 1D layer case

11. Missing in the discussion section: From you results, would it be possible to indicate areas where future sinkholes might develop? If not, what would be needed to be able to do so in the future?

Author: I agree, I will include words about this. We also aready did some time-lapse experiments there, which are required to monitor the subsurface processes. This will be published in a upcoming paper.

12. You postulate a new combined process model (lines 689-702). What data would you need in order to further support this model? The formation of subsurface channels and loss of cations might be monitored by a combination of time-lapse ERT, IP and SP. The arid environment might pose challenges for these techniques.

Author: Such kind of electric and electromagnetic investigations including GPR were already done by earlier experiments. Arid is right, it would support, but please remember the high salinity and the clay&mud content in the subsurface, the area was former Dead Sea bottom. Results and simple modelling examples show that a reliable penetration is restricted max to the level of the salt water, from a realistic point of view due to the included salt in the soil restricted to 10-20 m. To verify the model, a core borehole below 50 m is required.

Kind regards, dr. P.P. Kruiver