Dear authors and editor,
the presented study is quite interesting and well fit the scope of the issue. The overall idea, set up, acquisition and processing are good and well-presented and the text is mostly well written. Respect to the proposed interpretation and conclusion instead I have some major concerns. The interpretation appears in some aspect too optimistic on trusting the model results and it will benefit of more detailed information regarding the used geological constraints. Part of the conclusions instead are based on a comparison with a legacy data that may not be a proper reference for this study. I would suggest a major revision, not for the amount of needed work, but for the importance of the requested updates that, if not properly addressed, may invalidate the main results and conclusions.
Following you can read my specific comments regarding the main encountered issues and suggestions for more discussions, and technical comments mostly regarding the form of the text.

Specific comments

Almost all figures require bigger labels font size, it is too small and difficult to read. In general, many figures are too small and is difficult to see the details that you point out, consider adding zoom windows when needed and insert the figures as big as possible in the manuscript.

Results and interpretation chapter will benefit of a rewording to make the text clearer, sharper and to the point.

In figure 4c it looks like you got some DCs with particularly low phase velocity in P1 respect to the average results, can you map it in the data and in the results?

How confident are you with the velocity reversal? Did you tested if it is data driven or if it is some reminiscence from the starting model? In general, discuss how much the starting model is affecting your results.

The interpretation and results shown in figure 10 present different issues and they need to be updated and revised after consideration of each of them:
(1) Your inverted model seems to be too deep, you said you have a good coverage up to 350 m of wavelength and a reasonable one up to 550 m of wavelength. This means that your maximum reliable depth is approximately 150 to 200 m, without considering its possible reduction related to the velocity reversal and the issues related to the lower resolution in different areas as you state in the text. So, everything deeper than 200 m should be removed and definitely not considered for interpretation (you are interpreting down to 360 m that is almost double the depth). If you have some explanation to justify the reliability at this depth, then clearly state it in the text.
(2) The S-wave velocities in the upper part of the model are mostly reasonable, but 4500-5000 m/s (mostly from the bottom part of the model) are extremely high even for magmatic rocks, and you are attributing them to sedimentary rocks that should have lower ones. You can also see that most of these velocities stop at 5000 m/s indicating that they would go even higher if allowed to. These numbers are not reasonable for what I know and I suspect that they are a result of the starting model, but if you have some particular case in the area that justify these numbers, then state it in the text and indicate some reference. If you have sonic logs from the boreholes it will be very useful to compare them, if they are not available then you should report some velocity from literature for these formations or for similar lithologies.
(3) You talk about an internal distinction in the Hekpoort Formation between weathered and not weathered basalt, do you know if this distinction is reported also in the boreholes? If yes it will be

nice to show in the figure. This contact some time shows a strong velocity contrast while other times it is almost not visible. Is this due to the data quality changes, is it a real feature with internal differences on this contact, or are they different contacts (maybe with the clays reported on top of each borehole)? Discuss it better and use references when possible.
(4) You state that when the basalt is thicker than 200 m, the contact with the Timeball Formation is not visible in the inverted model. I think this is not due to the thickness of the basalt itself but to the depth of the contact. As I said in point (1) of this comment, I will not expect to see anything deeper than 200 m from this data, and therefore if the basalt is already 200 m thick you will simply not see its base contact because you do not have data from it.
(5) what is your expected resolution of the model? what is the minimum thickness you expect to resolve? how much accurate is the location at depth of a contact (5, 10, 20 m)? write it in the text and consider it during your interpretation.
Figure 12 confirms what I said for figure 10, almost everything below 200 m of depth looks unrealistic and a result of the inversion. If this is the case cut the model and show it down to 200 m of depth, you will also have a lower range of velocities that will highlights better your low- and high-velocity features while still keeping most of them.

In the comparison with the 3D seismic legacy data (Figure 13) you state that some of the modeled low-velocity zones are correlated with dikes detected at the mine level (3 km deep).
(1) How constrained are the geometries of these dikes? What is known about them? What is their thickness, dip and strike? Are they constant along the whole length of the dike? Do you have any data showing that they are expected to reach the near surface and do not stop at deeper levels? Do you have a surface expression of them? Just assuming that they move vertically for 3 km is not enough, you need to explain why these geometries are likely to be real and how much confidence we have on them.
(2) Why they are not visible in the seismic sections? You give some possibility but they need to be based on more concrete information. It is ok that vertical and thin structures cannot be imaged, but why the surrounding structures and crossed contacts are not affected and shows linear reflectivity? You may get some strong related diffraction in the unmigrated data, did you check it?
(3) Are this low-velocity zones matching with the dikes strike geometries (if known) also in 3 dimensions? In the figure you show only the crossing with one section and in the text you do not specify it.
(4) You state that the velocity in the dike can be lower than the surrounding sedimentary rocks under weathering conditions. But you never state how deep do you expect this weathering conditions in this area, I assume that at some point the dykes will not be weathered and their seismic velocity will be higher than the sedimentary surrounding rocks. Where is this crossing point? Is it deeper than your model?
In general, to can relate the low-velocity zones with the dikes you have to add more information and constraints about the dikes and where this information come from. Especially when you use them for one of the main conclusions of your work.

I do not understand what is the benefit you get from comparing the SW model with the seismic legacy data. The only deep structure you use in your interpretation are the dykes, that are not imaged, and the geological model, that seems to be derived from regional knowledge and not from the seismic sections. At the shallow depth where you have the SW model the seismic section has no good data available for any comparison. So, what is the scope of comparing them? State it clearer in the text.

You should compare the obtained SW model with the section resulting from the new active seismic data. At least the 2D profiles should have a better quality in the shallow part respect to the legacy 3D seismic data and they will allow you to better constrain and understand your results. You could also try some first break traveltime tomography and compare the two models, low- and high-velocity zones should mostly correspond for P and S waves in these cases.

I like your interpretation in figure 14 and it sounds quite likely, but you need to fix the previous model interpretations and clearly express what you can see from the data, what your results are adding to previous knowledge of the area, and what instead is just a suggestion.

One of your main conclusions is that the faults (together with the dykes) are one of the main pathways for water migration in the studied area and that this information is very important for safe mining operations. I agree with all of this, but I would like to see the whole results from your model and not only 2 examples from figure 11b and c. How many of these faults were you able to detect? Can you see their 3D geometries? Can you map them in the whole area? How deep can you map them? What are the relations between faults and formations seen in the data (and not known from the overall geology)? What are the limitations of the method? Were you able to detect them independently from the data coverage, dip angle and offset? Or there are preferential geometries that are emphasized from the data acquisition (a certain strike and deep for example)? Discuss all of this in the text.

Finally, you are using a new reflection active seismic data (3D and 2D), processing it for SW analysis and comparing with results from an older acquisition with different parameters not better specified, to show that you get information in the shallow area that you do not have in the 3D imaging. I agree with the statement but this comparison is not fair due to the different acquisition and of 20 years of evolution in the seismic industry, instead, you should compare the results from the same acquisition and with the same parameters for a proper comparison. You can then suggest at which depth it is better the SW analysis and at which the seismic reflection imaging. And you should distinguish between the 2D and 3D cases, since they have different resolution and coverage, to distinguish in which cases this method is useful (maybe all of them but you need to show it). At the end, the main question is “Why should someone do SW analysis instead of the more traditional seismic reflection processing, since the acquired data is the same?”, it is not faster nor cheaper, so you have to show that you get better results or extra information from the same acquisition.

Technical comments

Line 68 – “Malehmir et al, 2015”

Line 98 – Write “(Fig. 1) and not “(Fig. 1a and b)”

Line 98-101 – Rewrite the phrase more clearly, consider to split it in 2 phrases. State clearly that the following phrases are a general setting and later state that you are going into detail of the interested area.

Line 102 – It misses a “and” after the coma.

Line 130 – Remove the symbol “~” from the age since you are already giving an error range. The use of the age unit in “Ga” for line 118 and “Ma” in this and the next line for the same order of magnitude is slightly confusing, I suggest to be consistent all along the text.

Line 132 – “(Dorland, 2004)”

Figure 2 – This figure needs some update. (1) a and b are too small, the text is not readable and the profiles are not distinguishable. (2) the legend in a is only for the geology while the seismic sensors are described in the caption, but then in b the legend is referred to the seismic sensors, and in none you are referring to the boreholes. You may try to combine a and b on a single figure with a single complete legend or remove the satellite view, it is interesting but not of high relevance.

Line 167 – I think you mean “slip-sweep” and not “sleep-weep”. Check all along the manuscript.

Line 177 – You can define the distorted layout as crooked or moderately crooked profiles. Specify this especially for the 2D profiles.

Table 1 – Fix slip-sweep and slip time. Only the sweep frequencies for the 3D grid are linear?

Line 186 – Remove “(Fig. 2b; Table 1)”. It is just confusing.

Line 192 – “Clearbout and Green, 2008”

Line 195 – “Clearbout and Green, 2008”

Line 206 – “(Socco et al., 2009)” is in between two dots. Remove it or remove the first dot.

Line 228 – “CR gathers” write it as previously in the text (CRGs).

Line 220-230 – You state that the used spatial windows are mostly “relatively large”, can you add what would have been the optimal or desired window size for the analyzed datasets?

Line 233 – With the provided figure no ground roll is “clearly visible”. The picture is too small and the resolution low, maybe zoom the shot gather in the near offset if you do not need the far offset traces. In general, you do not use the term “clearly”, it is not a very scientific term.

Line 234 – “after transforming the data” You may specify “the data in the red dashed rectangle” to do not confuse the reader.

Figure 4 – The figure 4a is too small and low resolution, consider showing only part of it or adding a zoomed window. In figure 4d and e you use the Easting as x axes, but if I understood correctly they represent profile 1 and profile 2 that are perpendicular to each other and none of them parallel to the Easting direction. Did you consider to plot them versus the profile distance or one the easting and the other one the northing instead? It may be more representative of the real situation and easier to read in this case. Unless what you are showing is a projection of the profiles on EW direction, but in this case, you should explain it better in the text (same for figures 9a, 10 and 11).

Line 249 – In figure 5 caption you may add “plotted as a function of wavelength and spatial location” as stated in the caption of figure 4 (remove depth), to keep consistent wording along the manuscript and make it easier for the reader to follow. Make the labels bigger in the figure.

Line 272 – Add a reference or explanation for “(considering approximately depth = wavelength/2.5).”

Figure 7 – Center “Phase velocity (m/s)” label.

I suggest to consider the LCI chapter as a subchapter of data processing (4.1 instead than 5).

Figure 8 – What is the inset shown in 8b? state it in the caption or show it in the figure (or both), I think it is a zoom of the Global misfit but it is not clear.

Line 295 – In this study you used a laterally invariant initial model for the whole area. Since the area is quite big and heterogeneous, did you test how a more detailed initial model affects the results? and more in general, did you tested different initial models to discriminate dependencies of the results from the starting model itself? Discuss it

Line 311 – Fix “Figs. 9a and b)”.

Line 312 – What do you mean by “variations in the study area are resolved”? be more specific.

Line 315 – Fix “Figs. 9a and b)”.

Line 316 – Remove the dash and use parenthesis instead.

Line 315-320 – Try to be clearer and more concise on what you want to highlight. What do you mean by “low- and high-velocity zones”? It is normal to have low and high velocity unless you are in a homogeneous media. Remove terms as “Evident” and “prominent” and use more scientific and quantitative terms (i.e., in figure 9c is visible a S-wave velocity reversal zone at around xx m of depths).

Line 320 – Remove “(Fig. 9c)” if you already stated “In figure 9c” at the beginning of the phrase.

Figure 9c – The 3D grid plot is very interesting with the full data view, but if you want to use it for highlighting the correlations between datasets it is not useful. The correlations you indicate in the circles are not visible and just take time for the reader to search for them. Or you modify the figure to make these areas more visible (remove some of the data, use zoom in windows, etc.) or you remove it from the figure and just state in the text and the reader will have to trust you. It is also very difficult to understand which data is from the 2D profiles and which one is from the 3D acquisition.

Figure 10 – In the legend write “inferred variations within the formations”, using Hekpoort-Timeball Formation is confusing.

Line 398 – How did you do the interpolation? Did you consider the different coverage and the different resolution of the datasets? Write it in the text.

Line 398-404 – Rephrase it to be clearer.

Line 412-419 – Move to the “Site and data” chapter.

Line 424 – If you “already stated” it there is no need to repeat. Give all needed information at the same point.

Figure 13 – You do not state in the caption what MT stands for (I assume mine tunnels?).

Figure 14 – You draw the dyke all the way to the surface, do you have geological evidences of it at the surface?

Line 518 – From what you showed and said so far, the S-wave velocity model does not show any dyke. It shows some low-velocity zone that you suggest may be related to dykes, but you need to provide more information as detailed in the general comments if you want to state this. The low-velocity zones could be just due to faults and local fracturing.

Line 538 – Compromised by what? Specify with some example.

Line 539-541 – This statement is subjected to the replies and additional information you will provide respect to my specific comments.

Line 592 – I cannot find this reference in the text. Check and in case remove it.

Line 604 – This reference is a copy of the one just above, remove it.

Line 639 – I cannot find this reference in the text. Check and in case remove it.

Line 657 – I cannot find this reference in the text. Check and in case remove it.