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Interactive comment on "Passive processing of active nodal seismic data: Estimation of V_P / V_S-ratios to characterize structure and hydrology of an alpine valley infill" *by* Michael Behm et al.

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We thank the reviewer for the detailed and very useful comments. Please find our response below.

(1) The authors note a big difference between the two sensor types they use: 4.5-Hz 1-C-geophones of unknown making that "appear to have a better response than the ZLand 5-Hz 3-C-stations". It's unclear what exactly "better response" means (the frequency response functions are not shown) but the ZLand data in Figure 5 simply

shows no useful signal. I am quite surprised by that, and I know colleagues who equipped themselves with ZLand nodes because of their supposedly great sensitivity especially at low frequencies. To claim that those instruments perform poorly has a potentially large impact on the manufacturer, and such a claim must be well founded: The authors must do more to reveal the source of the apparently poor data quality of the ZLands! Is it possible that no instrument simulation was performed? The authors don't write about it. It might explain the apparent differences. (It would also mean that the cross-correlations and the subsequent MASW would have to be re-done with the corrected data.) Or could it be a problem with the time-base?

» This section was indeed poorly presented. There are no issues with the nodes, but the (apparently) "better" response of the Texans is mostly due to Geology and acquisition geometry. The nodes have been deployed in the northern part, where the basement comes close to the surface and the sedimentary cover represents a presumably thick layer of heavily weathered basement rocks mixed with young soil/debris, which tend to scatter surface waves. Additionally, most of the noise sources (including the shown examples) are situated in the south, so there is an overall decay in signal strength towards the north. It is further noted that it was easier to tightly ground-couple the small 1C geophones compared to the more bulky and more heavy ZLand stations. We chose different data examples and simply increase the gain to avoid the wrong impression of faulty node data acquisition. We also adapted the description in the text.

(2) I find the interpretation section difficult to follow both how it is written and the conclusions it reaches. This paragraph should be rewritten for more clarity. I suggest the authors start this section summarizing all geologic information they have on the lithologies to be expected in this valley, including the geologic map and the well. More importantly, I find the lithologic description (clay/mud below ~1720 m in Figure 11) to be inconsistent with the reported P-wave velocity of 2700-3300 m/s. In their book chapter "Rock physics principles for Near Surface Geophysics" (In: SEG-Investigations in Geophysics No. 13, 2005), Knight and Enders report P-wave velocities for clay to

be at most 2200 m/s. Indeed, all technical literature I am aware of specifies P-wave velocities for loose materials significantly below 2700-3200 m/s. If real, such velocity indicates lithified rocks, which might be an important finding. If not real, and if the material is indeed clay, it contradicts the statement that it is part of a 400-m-thick aquifer (13/25). Also, I find the interpretation of the GWT difficult. I fully understand that there are many reasons why VP might not be indicative for the GWT but not in this case of unconsolidated sand, where one would expect a sudden increase of VP at the GWT from maybe 800-1200 to $\sim$1700-1800 (as seen in the well). It would be appropriate to make a first interpretation of the GWT from a contour line in the range $\sim$1500-1800 m/s, and then check, if such a contour line coincides with the interpretation of the GWT from VP/VS-ratios.

» We agree that the structure of the interpretation section is poor and adds more confusion than clarity. We tried to rearrange accordingly to the remarks above. The conclusions on water saturation, GWT interpretation, and potential aquifer properties have also been revised. In particular the comment on high Vp-velocities has been addressed more clearly, also in the context of the reviewer's comment (3) below. We point out that the interpretation of "clay" below the sand describes the core for a few meters only, as drilling was stopped below the lacustrine sands. It has also been made more clear that the well is 5 km away, and as such cannot be used to exactly validate the results at the seismic location. Figure 11 has been updated as well.

(3) Please add some more detail about how the reflection-processing-based Vp-velocities were derived. (At an angle of 45°, as can be seen in the reflection section, NMO velocities would be 30% increased just from the dip, not to mention the potential distortions from the Dix formula.)

» The NMO and interval velocities were derived from conventional CMP velocity analysis, with additional manual editing accounting for the U-shape. The extracted velocity profile is located at the center of the U-shaped structure, where reflections from the flat part of the U are actually observed. For the extracted vertical velocity profile, Vnmo

velocity reductions larger than 20% for the bottom layers lead to unrealistic interval velocities (e.g. bouncing back to 1100 m/s at 250 m depth). We take this as a suggestion that the influence of the steep valley walls is not significant at this central location, but we are aware that this is a rather qualitative statement. There are also strong indications for out-of-plane reflections which might introduce non-physical layering in the velocity profile. As a result of all these uncertainties, we refrain from a detailed discussion of the lower section of the sediment fill. The text has been adapted accordingly.

2/32 – In their model, resistivity is increased for the aquifer.

» Has been changed.

3/28 – I cannot see that road on the map.

» This is because the narrow road in the map is masked by the signatures for receivers and shots. A description has been added to the map.

5/9 – Over-deepening is an effect along the river-bed. How can you identify it from a cross-section?

» This interpretation has been expanded on in the text.

7/28 – 8/2 Your explanation for the observation would still require that the reflections were stronger than the incident waves.

» This discussion has been changed/expanded.

8/10 – "refrain" – you mean this cancels out through stacking?

» "refrain" has been changed to "attenuate", as stacking(averaging) with limited data will not achieve total cancellation.

8/23 – Please clarify what you need density for. Maybe it's not so important but for unconsolidated saturated sediments, Gardner's relation tends to significantly overestimate the density.

» We need density as one of the model parameters (vp/vs/rho/thickness) for surface wave inversion, since the Rayleigh wave velocity is a function of Vp, Vs, and rho. Many studies have shown that the phase velocity has low sensitivity to density (e.g., Xia et al., 1999), therefore usually just constant densities like (2.0 g/cmˆ3) are chosen for surface wave inversion. Recent research show that the use of constant density can lead to Vs overestimation as well as create inaccurate model structures, such as a low-velocity layer (Ivanov et al., 2016). Thus, we prefer a meaningful density model which could be associated with the earth model like vp. Gardner's relation, even though it might overestimates densities, is already a significant improvement to commonly used and accepted constant densities.

9/13 – I agree that the ratio profile length to wavelength should be at least 1.5-2. But I fail to see how you can then say that it's supposedly okay to use a ratio of less than 0.5. How does the overall length of your profile change the length of your subprofiles to which you apply MASW?

» As for passive MASW, there is no exact numerical relationship to indicate the maximum wavelength in relation to the linear array length. There's no clear maximum wavelength criterion, but only commonly accepted rules of thumb which will also change with the data quality, dispersion measurement, source-receiver configuration and chosen processing techniques etc. We chose the minimum frequency as 3.5 Hz due to the high-quality data and dispersion measurements (continued dispersion spectra extend as low as 2Hz) in our case. Depending on the velocity, this results in minimum wavelength-profile length factors between 0.3 and 0.7. The text has been changed accordingly.

6/25ff (Interpretation) – Overall, I don't understand what the authors want to say in this paragraph: In the beginning the argue that there is a systematic trend regarding Vp/Vs and pore fill, and then they discuss examples that all appear to contradict those trends. Also, references to lower crustal studies, or studies where the GWT is in fractured granite, should be avoided. It's not enough for a general overview, and too much for

loose sand.

» Despite a detailed literature search, we find very few papers which report measured Vp/Vs ratios in exploration depths corresponding to our study (e.g. below soil / weathering zone and above deep crustal targets), and in particular with relation to hydrology. Industry is expected to possess a lot of data on the reservoir level, but it is very rare that these get published. The cited studies on shallow soil structures are still considered as relevant, as they at least refer to similar material (sand, gravel). We also point out that sand represents only one among other materials (weathered granitic basement, alluvium, colluvium) which we interpret along our entire profile, so we further think that the reference to weathered granite is useful as well. References to deep crustal studies have been eliminated. It is not totally clear to us what the reviewer means by "[cited] examples that all appear to contradict those trends". All of the cited examples show an increase of the Vp/Vs ratio with increase of the degree of water saturation (or with the switch from dry to water-saturated materials). There was, however, a profound misphrasing in our description: Uyanik defines "saturation" as 100% water saturation of the pore space, and uses "water content" as the total amount of water in a volume of 100% saturated soil (defined via the weight ratio). We wrongly described the "10%-50% water content" as "10% to 50% water saturation". This error has been corrected

10/7ff – If Vp/Vs-ratios greater than 3.3 indicate "saturation" (100%, I assume), how can Vp/Vs of 5 indicate only 10% saturation?

» This error has been corrected, see above.

12/23 – "Largely insensitive": Not if you undershoot. "less sensitive" might be more sensitive.

» Has been changed

12/23ff – is a discussion of the geophysical approach and could be a separate section.

» We reorganized the discussion / interpretation section accordingly.

13/14 – I believe it is standard in the earthquake community that you remove events before X-correlation. Could you comment on why they/you do things differently?

» The events (active blasts) comprise clear and strong dispersive surface waves, which we don't want to remove. A statement has been added to section 5.1

13/20 – I don't see how this is a conclusion. You did not use the horizontal components!

» Has been removed.

13/24 – That should go to the interpretation section!

» We have changed the "vertical extent of 400 m" to "100 m", as we no longer interpret the Vp/Vs ratio below the sand. However, we think that this is a summarizing statement which fits into the conclusion section.

Fig.2: I find the colored lines/sidebars very confusing since they do not indicate profiles. It took me some time to realize that. I am not sure they are required but you could at least move them outside of the map. Also, the air photo doesn't really convey any useful information, at least none you refer to, and a simple line-drawing would do it. Perhaps the map is not even necessary at all, and Fig. 1 would suffice. » We like to keep the map as it shows the geometry of the acquisition, which is put into context into some parts of the manuscript (overall: extent of crooked line vs. 2D vertical plots; change of signal on ZLand vs. Texan recorders in relation to geology; variation of active source/signal strength along the profile has an impact of the assessment of the final velocity models). We moved the sidebars to the outside and added more useful information (elevation contours, location of the creek).

Fig 4: Please add a contour line at 1500 m/s, or adjust the color scale such that one can see this contour.

» We tried to do this, but it looks odd to have an isolated contour line since the GWT is not discussed at this stage of the manuscript. However, we added Vp contour lines (1500 m/s, 1800 m/s) to the Vp/Vs plot (Fig.9) where the GWT is discussed.

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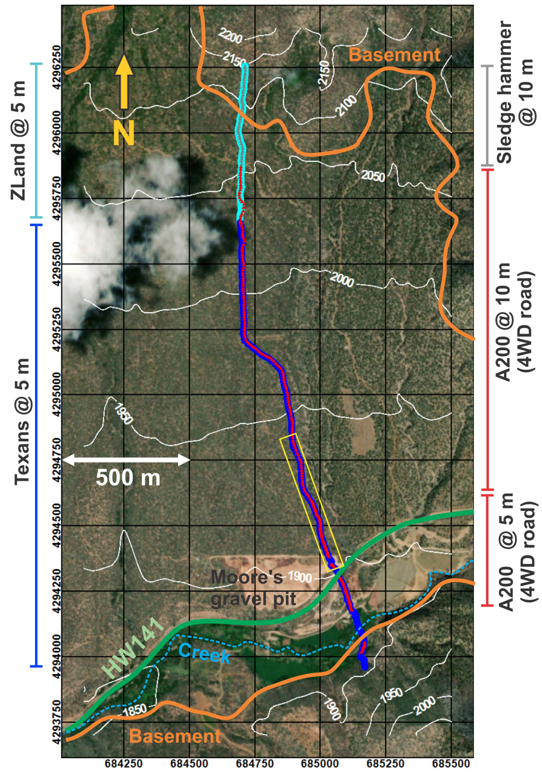
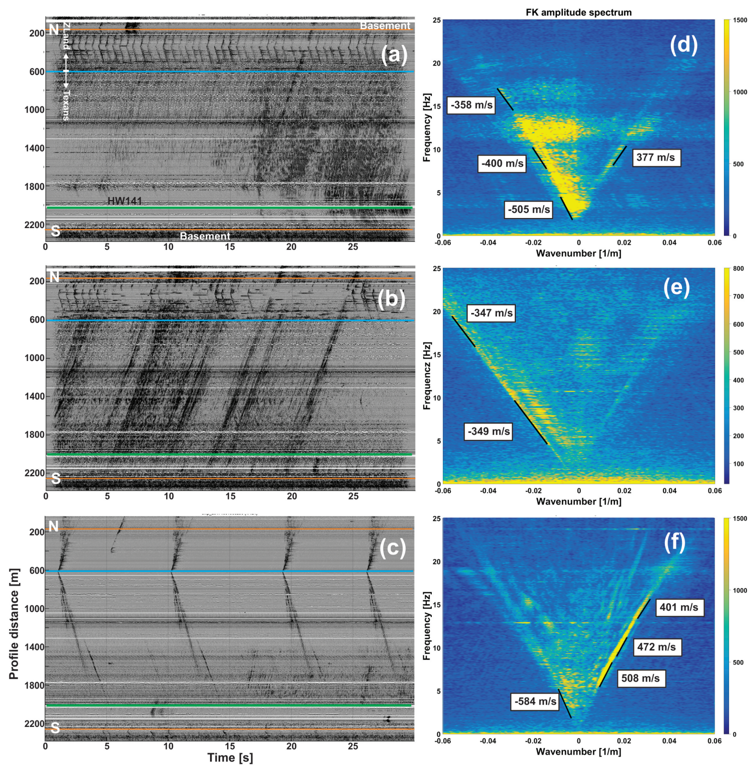


ZLand @ 5 m
Texans @ 5 m
Sledge hammer @ 10 m
A200 @ 10 m (4WD road)
A200 @ 5 m (4WD road)
Basement
N
500 m
Moore's gravel pit
HW141
Creek
Basement

Fig. 1.



FK amplitude spectrum
(a)
(b)
(c)
(d)
(e)
(f)
Basement
HW141
Profile distance [m]
Time [s]
Frequency [Hz]
Wavenumber [1/m]
-358 m/s
-400 m/s
377 m/s
-505 m/s
-347 m/s
-349 m/s
401 m/s
472 m/s
508 m/s
-584 m/s

Fig. 2.

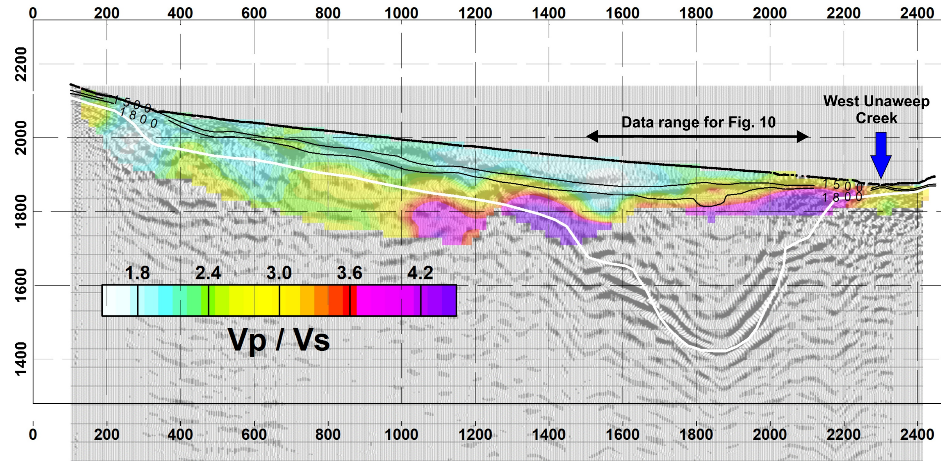

0
200
400
600
800
1000
1200
1400
1600
1800
2000
2200
2400
2200
2000
1800
1600
1400
West Unaweep Creek
Data range for Fig. 10
1500
1800
1.8
2.4
3.0
3.6
4.2
Vp / Vs

Fig. 3.

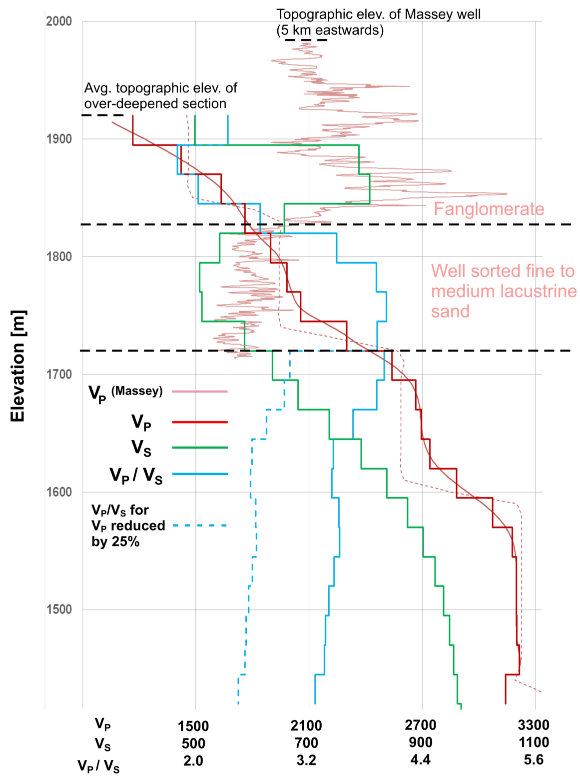

Topographic elev. of Massey well (5 km eastwards)
Avg. topographic elev. of over-deepened section
Fanglomerate
Well sorted fine to medium lacustrine sand
Elevation [m]
2000
1900
1800
1700
1600
1500
V_P (Massey)
V_P
V_S
V_P / V_S
V_P/V_S for V_P reduced by 25%
V_P 1500 2100 2700 3300
V_S 500 700 900 1100
V_P / V_S 2.0 3.2 4.4 5.6

Fig. 4.