Comments on Behm et al., “Passive Processing… alpine valley infill”

It seems to me that the topic is well suited for the journal. The study presents new data on the seismic structure of a 200-500-m-deep Alpine valley, and it uses a novel combination of active and passive data analysis. The paper is well structured and, for the most part, clearly written and in good English. However, there are a few unclear points, that require a revision of the paper.

(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?

(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 V_P might not be indicative for the GWT but not in this case of unconsolidated sand, where one would expect a sudden increase of V_P at the GWT from maybe 800-1200 to ~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 ~1500-1800 m/s, and then check, if such a contour line coincides with the interpretation of the GWT from V_P/V_S-ratios.

(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.)

Minor remarks:

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

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

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

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

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

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.

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?

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.

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?

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

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

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?

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

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

Fig. 1: A reference to the original author of the geological map may be missing?

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.

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