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Interactive comment on "Seismic signature of turbulence during the 2017 Oroville Dam spillway erosion crisis" *by* Phillip J. Goodling et al.

Anonymous Referee #2

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The manuscript presents seismic analysis of a high discharge event that deeply eroded the flood control spillway at Oroville Dam. Investigation of frequency-dependent 3-component particle motion at the broadband seismometer near the dam allowed continuous estimation of the location of the dominant seismic source and discrimination of the dominant wave type. The circumstance provides an interesting opportunity to investigate the seismic signal of turbulent flow in a channel that initially has a well-known simple shape and lack of bedload. Changes in the seismic signal through the high discharge event are observed and interpreted by the authors in the context of changing turbulent flow conditions in the newly incised spillway channel. Overall the manuscript is well written and effectively presents seismic results relevant to monitoring dams and observing naturally occurring turbulent flows from a safe distance. I think it is suitable

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Discussion paper

for publication with only minor revisions.

Page 2, Line 28. In this case are the authors referring to changes in channel geometry with time and/or spatially within the channel?

Fig. 1 The bifurcation of the flood control spillway is clear, but the location and type of damage to the emergency spillway is not easy to see. Is the emergency spillway damage meant to refer to the few meters of erosion that appear to be almost uniform along it in the elevation difference map?

Page 11 and Fig. 6. Confidence intervals for the discharge exponent values 'pre' and 'post-chasm' would be useful information. There appears to be a compelling difference, but an attempt to quantify the uncertainty would be an improvement.

Fig. 7. The authors might consider labeling the azimuth corridor that corresponds to the spillway as a handy visual reference. But I understand that it may not be ideal if it obstructs other information.

Section 4.7. This is a good attempt at estimating the effect of topography on the polarization results, and the authors acknowledge some of the limitations of the 2D simulation. I would suggest a bit of additional caution regarding the simple velocity model because the frequency dependent polarization of surface waves could be strongly affected by depth-dependent (and spatially variable) velocity structure likely including anisotropy. I agree that the modeling effort presented provides a viable explanation for some of the deviation from idealized surface wave propagation without topography, I'm just encouraging clear description of its limitations.

Section 4.7. and Fig. 9. Is the oscillating VH angle in Figure 9 because only one point source is considered? Would it be more realistic to sum the seismograms with staggered time shifts to simulate a temporally continuous and spatially distributed source process?

Discussion. The difference in exponent 'pre' and 'post-chasm' is interesting, and even though there is not a clear explanation for it I think the higher exponent is a useful target for future studies. In regard to comparison with the Gimbert et al. model I wonder if the extreme steepening of the channel to essentially a waterfall into the ‘chasm’ is beyond the limits of the model formulated by Gimbert et al or if they actually thought the model assumptions would still be reasonably well justified in that setting?

The supplementary material is used appropriately and will be valuable to researchers in the field.

Continuous line numbering would be more helpful for review, but maybe that’s a journal policy.

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