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Interactive comment

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

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In their manuscript, the authors show that the Oroville Dam produced seismic ground motions that can be characterized and which changed with flow characteristics. Overall, the work is reasonably well described, and is a novel application of seismology to observing a process of significant scientific and societal interest. Some of the text and figures can be improved, with more detailed suggestions and comments below, and after such improvements are made, the manuscript should be a nice contribution to the literature.

P1L15,18: See later comments about clarifying discharge scaling and upslope propagation.

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P2L17: Run-on sentence.

P2L28: Not clear what is implied by 'geometry variations'.

P3L20: It is not true that the Tsai and Gimbert models assume only Rayleigh waves are excited. In the Tsai model, it is true that a Rayleigh-wave Green's function is used to approximate the response since the force is assumed to be close to vertical, but it is not a limitation of the general modeling framework. In the Gimbert model, a similar approximation is made, but again Love waves could be included in the most generic version of the model.

Figure 1: Panel c needs better labeling. First, it should be clarified where exactly the label 'emergency spillway damage' is referring to. Second, the same names for labels should be used as in panel b. Labels should also be larger, and generally easier to read. Finally, since the distance from the signal to the station is an important parameter, it would be useful to mention somewhere (either in the text or figure) what those distances are. (It can be estimated using the scale bar, but a definite number would be useful.)

P6L1: Listed as 38-hour here but 26-hour in Figure 2. Please clarify.

P6L18: "complied" should be "compiled"

P7L3: "causally" would not be clear to non-seismologists. Either explain in more detail or remove.

Section 3.2 (P8-9): It is not clear that this description is very useful. It is technical, and not that well explained. It might be more straightforward to just describe the statistics used and refer to the references for details, rather than put in a technical section that is challenging to read. Alternatively, the section could be clarified. I believe I understand roughly what the authors did, but this understanding is not from reading the section. As just one example, on P8L7, it is not clear what dominant eigenvector is being discussed. Eigenvector of what?

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L10P6: "Complex" should be described more.

Figure 5: It is difficult to tell how much of the differences between 2017 and the other years are just due to the difference in range, and how much of the hysteretic behavior is due to something else. In particular, the low-flow part of 2017 does not appear to have strong hysteresis, and is therefore appears quite similar to the other years, and perhaps not distinguishable if the higher flow segments were not there. Incidentally, the color scale chosen for this figure is poor. Please modify to make the times more distinguishable. Potentially larger symbol sizes are needed, or the black edges could be removed to make clearer.

P11L9: Again, first eigenvector of what? Not clear what it is an eigenvector of.

P11L12: Break in slope is not clear. Please clarify.

P12L2-3: This statement needs better explanation. How is the change in scaling relationship consistent with a change in turbulent intensity? Why should the scaling exponent be expected to change in this way, rather than just changing the scale factor, for example (but not the exponent). Somewhere here, it would also be worth commenting on whether the raw signal (without doing a polarization analysis) shows the same behavior or not. Is it necessary to do a polarization analysis? Or is it just clearer using the decomposed polarities? What about the vertical?

Figure 7: Zero discharge azimuths are actually somewhat well determined at a wide range of frequencies. It is true that azimuths are better determined for other times, but only relatively so. So, some discussion should be modified.

P14L24: m/s Units are incorrect

P15L1: m/s Again units are incorrect

P16L26: Do these simulations use uniform velocities? If so, this might yield misleading results, since a more realistic structure in which velocities increase with depth naturally have stronger trapping of waves near the surface, and thus stronger surface waves. (If

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simulations use non-uniform velocities, that should be clarified as well.) Partly for this reason, it is not clear how much of this section's analysis really explains the deviations discussed.

P17L9: In a uniform velocity medium with a slope, surface waves simply travel along the slope, rather than horizontally. Part of the complexity shown and cited is due to the non-uniformity of the slope, not just the existence of a slope. This should be clarified.

P18L23: Again, why does greater turbulence imply a change in exponent? This argument needs to be fleshed out, and would add significantly to the conclusions if it can be done quantitatively. It is interesting that the Gimbert model appears to work better during pre-crisis times, but the reason it does not work later should be more specific than the generic 'greater turbulence' statement, since greater turbulence would also just be expected at higher flow rates within the same model.

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