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## Comment on nhess-2021-62

Anonymous Referee #1

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Referee comment on "Identifying plausible historical scenarios for coupled lake level and seismicity rate changes: the case for the Dead Sea during the last 2 millennia" by Mariana Belferman et al., Nat. Hazards Earth Syst. Sci. Discuss.,  
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In this paper, the authors apply a constant tectonic shear loading on a theoretical 1D vertical strike slip fault along with water lake forcing at the surface and calculate nucleation of seismic events. They use different water level history curves and choose the best fit water level that predict synthetic seismicity similar to historical records. They conclude that based on the similarities between their synthetic seismicity and the historical seismicity they can constrain water level history of the Dead Sea – Lisan lake.

I find this work to be misleading for several reasons:

- There is no obvious correlation between lake level and seismicity. Seismicity occurs equally at high and low levels (Fig. 1 c and d). If at all, the water level has a minor effect on seismicity. The major forcing for seismicity is tectonic forcing. The reason that earthquakes do show exact intervals of recurrence could be related to changing shear forcing, damage processes, or viscoelastic relaxation. In my opinion, these processes are more likely to be responsible for the changing recurrence intervals. It is possible to get non-uniform intervals with any diffusion process and lake loading and pore pressure diffusion seems to me to be the least favorable. Furthermore, the model used in this paper use very questionable assumption (see following sections).
- The most questionable assumption is that the pore pressure at hypocentral depth approaches the value at the lake's bed. At the Dead Sea, even shallow boreholes are over pressurized. Water level in Ein Feshkha are artesian. Shallow aquifers <30m do not approach the lake water level. Ein Feshkha site is very close to a fault. Hypocentral depth at the Dead Sea could be 25 km. Between seismic events, faults heal and the permeability of the faults is very low. Deep layers will never approaches the value at the lake's bed. Given a reasonable permeability, it will take millions of years. How can pore pressure at this depth approach the lake pressure? What about compaction and poroelastic loading? Furthermore, the diffusion time depend on the hypocentral depth. Hypocentral depth could be any number between 1 and 30 km. This will have a great effect on the relaxation time. The model does not take this into account because the

hypocentral depth are unknown.

- A second questionable assumption is that the pore pressure throughout the Dead Sea Basin at any depth responds elastically to changes in the lake level. The Dead Sea basin is layered with different rocks and soils. In fact, most of the basin is made up of clay. Clay is very plastic. Any deformation at the surface will plastically deform the clay and will not affect deep layers. The loading efficiency is very low. The rate of the water level changes is much slower than the rate required for plastic flow in the clay to redistribute deformation. I estimate that changes of 25m in water level loading at the surface do not significantly affect the stress at depths below 2-3 km.

To summarize, in my opinion, there are many problems with the simplistic model suggested in this paper. It ignores many processes and use unreasonable assumptions. This model cannot be used to calibrate the water level history of the Dead Sea.