Characterization of spatial coseismic response of groundwater levels in shallow and deep parts of an alluvial plain to different earthquakes by Parvin et al.

The paper investigates the hydrologic responses of aquifers of different depth to earthquakes of different distance and magnitude. A network of 56 groundwater wells registered rising and/or falling groundwater level responses to earthquakes. The impact of different geological settings (alluvial vs. andesite lava) on the hydrological responses has been addressed. The hydrologic response is preferably related to changes in hydraulic heads. Finally, a conceptual model has been developed in order to explain the spatial pattern of the observations.
Though the topic of the paper is clearly within the wider scope of HESS and thus potentially suitable for publication, it shows some major flaws. Some of these flaws are substantial.

General comments:

- What is the most important outcome of this study? How does this work explicitly contribute to the knowledge of earthquake related hydrological phenomena? What is the newly gained knowledge? This should be better pointed out by the authors.
- Some sections should be reorganized in order to clarify the paper's outline:
    - Abstract: what is the main outcome of this study? Neither the results, methods, interpretation/discussion nor conclusions do explicitly appear. I suggest reshaping the abstract completely.
    - Introduction: It is mentioned that further investigation of groundwater response to earthquake is needed. However, the authors do not explain why their study is important to address these issues and how they contribute to further understanding of seismo-hydrological processes? This should be complemented in order to point out the study's relevance.
    - Results: Results, observations and discussion are merged into one section. From my point of view, this makes the interpretation complicated. It is very hard to differentiate between the results/findings and the authors' personal interpretation. I'd suggest separating the distinct sections clearly in order to avoid mixing of observations, results which may lead to misinterpretation. Moreover, it facilitates the reader to follow the thoughts of the authors. Even though the interpretation seems to be reasonable in cases, the interpretation remains unsupported by any proof (e.g., page 5328, lines 4-8).

    - The distinct hydrological responses to the earthquakes and their magnitude are mentioned in section 3.2. However, they rather belong to the results section where they should be removed to. In general, I think, the results section should be reshaped up to a substantial extent and a discussion section should be added.

- The distinct mechanism (communicating aquifers; increasing hydraulic head vs. indicated permeability change by coseismic (dilitant?) fissures) are presented over simplistic and a critical assessment is missing. The anisotropic permeability change, as proposed for the Chi-Chi earthquake response in Taiwan is not considered as a potential mechanisms though the geological/topographical setting of the greater study area here seems to be comparable.
- The written English should be improved
- Important and recent references are missing: e.g., latest overview: Wang and Manga, (2010): Earthquakes and water; Permeability: Elkhoury et al. (2006) Nature, Alluvial fan response: Wang et al., (2001) Geology; Anisotropic permeability change: Wang et al., (2004) Geology; Groundwater of different depth: Wang et al., (2012) Geology.
- The authors stress the lack of studies focussing on multiple earthquake responses and to groundwater tables of different depth though studies of comparable settings exist. However, the hydrological responses and their spatial patterns of the Chi-Chi earthquake may provide a valuable comparison for this study here. In addition, Montgomery et al, 2003 does also deal with hydrologic effects of an earthquake on an alluvial fan.
- The authors often refer to “patterns” but do not explicitly explain what kind of patterns they refer to? Spatial? Temporal? Please clarify this consistently.
- From my personal opinion, the geological impact of Togawa lava is the most interesting feature of this study and should be expanded in analysis and discussion.

Scientific issues and questions:

1.) What is the exactly the underlying process? Is it a co-seismic change in hydraulic head, modified connectivity or permeability of the geological units? In the abstract, the importance of hydraulic head increase is mentioned. In the conceptual model, however, the impact of permeability change due to fissuring is also indicated. Is it a mix of both processes? In order to facilitate the discussion, the present day's understanding of seismo-hydrological processes can be shortly reviewed in the introduction/model section.

2.) What is the accuracy of the groundwater level measurements? By what means has the groundwater levels been measured? How are the uncertainties? Uncertainties are not quantified or even mentioned in this manuscript. Please be more critical about the measurements in terms of assessing the quality of the measurements (In fact, an increase of ~1 cm is hard to measure). Does water temperature data exist in order to support your interpretation?

3.) The Magnitude of the earthquakes is mentioned and seismic energy appears several times throughout the manuscript. The local seismic energy (density) can be estimated according to Wang and Manga (2010), Geofluids, or Wang (2007), SRL, which could be used to evaluate concurring mechanisms within the near- and far- field as different hydro-seismological mechanisms are related to threshold values of seismic energy density. The earthquake mechanism is not mentioned. Are all earthquakes comparable in terms of rupture mechanisms? Moreover, the ground shaking can be assessed by available ground velocity/ acceleration data (e.g., see http://earthquake.usgs.gov/) in order to compare the impact of each earthquake within the study area. Finally, the duration of the distinct earthquakes should be considered since they may be crucial for some processes, e.g. undrained consolidation (probably up to liquefaction).

4.) Several times throughout the manuscript, "large earthquake" is mentioned. However, how are they defined in this case?

5.) The applied interpolation technique seems to be suitable for such a kind of data set. However, the geological setting differs substantially across the study site and I am wondering if spline-based interpolation does account for that? Moreover, please specify how many samples are included into the spatial analysis. Are all wells (n=56) included into this spatial analysis? What is the uncertainty of the spline-interpolation?

6.) The andesite seems to differ from the alluvial deposits mostly in terms of porosity. Can the porosity of both geological units be quantified?

7.) Short-term dilatation is mentioned in the introduction section. However, dilatation/dilatancy excludes increased hydraulic head as a potential mechanism since dilatation/dilatancy increases the porosity by secondary dilatation cracks/fissures which in turn decrease the hydraulic head.

8.) Are there any significant tectonic faults crossing the area? And if so: Is there a spatial relation between responses and the tectonic setting?

9.) The more detailed analyzed wells (2 each geological unit/ earthquake) are all located in the recharge area of the flats, right? Does data from the discharge area of the foothills exist?

10.) The conceptual model postulates a strong compressibility of groundwater. However, compressibility of water is very small, isn't it?

Technical corrections/ suggestions

- What is the undrained Poisson's ratio (page 5322; line: 4)? What describes the Kronecker delta (page: 5322; line: 5) and the Skempton's coefficient (page 5322; line: 18)? A short explanation would benefit to the understanding of the study.
- Poroelastic theory should be explained more clearly if mentioned, probably by adding 1-2 sentences.
- Table 1: longitude of SCE earthquake is incorrect. This table could be expanded with additional earthquake features (e.g., type of earthquake mechanism, duration, surface velocity, …)
- In Figure 2, 5, 9, 10 and 11appears a red line crossing the area. What is that red line exactly?
- Figure 13: quite speculative and the model should be better explained
- Page 5331; Lines23-25: This is an interesting finding and should be included into the abstract.
- Page 5332; Line 10: where is mentioned twice in this line. The second should be changed to "were"
- Page 5334: line 8: R^2 is here 0.63 but 0.62 in figure 12. Moreover, I was wondering if this relation is really resilient.