

## ***Interactive comment on “Characterization of spatial coseismic response of groundwater levels in shallow and deep parts of an alluvial plain to different earthquakes” by M. Parvin et al.***

**Anonymous Referee #2**

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The authors made some interesting observations, such as both coseismic rises and falls were observed in the same well location during the KME, with rises in the shallow wells and falls in deep wells. Also, during the GYE, most shallow wells showed no significant change, but the deep wells showed both coseismic rises and falls.

On the other hand, their claimed correlation between water level change and the Sichuan earthquake (SCE), shown in Figure 7, is not at all convincing. Not only are the signals extremely noisy, but also changes of similar magnitude occurred in the water-level records long before the earthquake.

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More important problems occur in the interpretation of observation. Earthquake hydrology has advanced rapidly in the past decade. However, the authors appear unaware of some of the advances. Much of the cited material is out of date; even the cited review by Manga and Wang (2007) has been superseded by more recent reviews. The exception Parvin et al. (2011) was their own paper.

Another example is the authors' assumption of poroelasticity as the mechanism in their interpretation of the observed water level changes. It has been pointed out time and again that the magnitude of static stress change due to distant earthquake, such as the Sichuan earthquake (SCE), is simply too small to cause any perceptible water level changes in the studied area.

The authors also made some misleading statements: At the beginning of Introduction (line 25-26), for example, they misquoted Roeloffs (1988) in stating that “groundwater fluctuations . . . can contribute a pre-warning system for earthquake disasters.” Roeloffs (1988) was more careful not to make such misleading statement. Also, at the bottom of p. 5333, the authors stated: “. . . decreases in compressive stress were observed . . . increases of compressive stress were observed . . .” In fact, only water-level changes were observed. The supposed change in compressive stress was calculated based upon the unproven hypothesis that poroelasticity was the causing mechanism.

Finally, two minor comments: 1) The authors spent three full pages (p.5321- 5323) discussing poroelasticity, but did not calculate the elastic stress or strain in the studied area for any of the earthquakes. It begs the question why is this repetition of details but omission of the essential? 2) The model presented in section 4.6 is totally speculative without justification. As such, statements such as “For close earthquakes, the deep groundwater is strongly compressed . . .” become meaningless and can only confuse the readers.