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| **Date:** | Jul 8 2013 3:32PM |
| **To:** | "Yan Zhang" eve\_041744@163.com |
| **From:** | "BSSA Editorial Office" BSSAmss@seismosoc.org |
| **Subject:** | Decision on MS# BSSA-D-12-00360R2 |
| Ref.:  Ms. No. BSSA-D-12-00360R2 Studies of mechanism for water level changes induced by teleseismic waves Bulletin of the Seismological Society of America  Dear Yan Zhang,  Your paper has been reviewed for publication in the Bulletin.  I enclose two reviews by anonymous referees who have raised serious concerns.  Reviewer #3 did not review the first draft of the manuscript but has similar criticisms to the two reviewers who reviewed the first draft.  These are appended below. The editorial board has evaluated the reviews and has found the paper to be unacceptable for publication.  First, the English is still poor and the paper lacks structure/organization, although you were given significant input from two reviewers and an associate editor on how to improve the grammar and organization of the manuscript.  Second, you do not appear to addressed the technical concerns of the reviewers and associate editor in your revision.    I believe the Editorial report and reviews adequately explain the reasons for this decision and I hope you find them useful.  Thank you for your interest in the Bulletin.  Sincerely  Diane I. Doser, PhD Editor-in-Chief  Reviewers' and associate editor's comments:  Associate Editor: Please see the comments from the reviewers. Try to incorporate well log data to support your view. You may need to re do this work and resubmit it.  Reviewer #2: Attached. (note this reviewer reviewed version 1 of the manuscript as well as your revised manuscript)    Reviewer #3: The authors of this draft show water level changes due the Wenchuan Earthquake, recorded in several wells in mainland China, at distance too far to attribute these changes to poroelastic response to static stress changes. They claim these changes is due to a variation in Skempton coefficient "B", rather than changes in other poroelastic coefficients or permeability. This change in B is related to a theory of "consolidation/dilation".  The paper is difficult to read, first because of language issues, second because of the poor construction of the discussion. The consolidation/dilation theory is quite unclear, partially because of the lack of equation. Also, during their discussion, it is unclear whether the medium is fractured or porous. The relationship between porosity, elastic modulus and porosity may be quite different in these cases. The author do not take time to discuss their raw data, and comment the order of magnitude of their results. Quality control of data and analysis should be discussed in a first part of the discussion, not left to the discussion at the end of the paper.  There are several points which need to be clarified.  - Does the poroelastic theory used by the authors apply to the formation in their wells? For instance, lithological logs shows shales and crystalline rock. The first rock may display substantial anisotropy or a fractured network rather than a porous network. Previous reviewers asked for more log data to clarify this point, but the authors did not reply to their request.  - The Skempton coefficients are very small for many wells (<0.1). At the recorded depths, we expect fully saturated rocks, and Skempton coefficient are expected to be larger than 0.5 (see final tables of Wang, 2010, citation of l. 585). If the medium is unsaturated, the authors should state that.  - The authors focus on the change in Skempton coefficient, dismissing any change in other coefficients. For instance, as cited in line 141, Berryman and Wang (2001) show a large variation in bulk modulus  Ku in their data. Remember, that the tidal amplitude of water level changes is controlled by B x Ku. I don't understand why the author cite the work done on bone by Theo H Smit, Jacques Huyghe and Stephen C. Cowin (note that the authors cited these authors by their first name): in this paper, they discuss the dependency of the coefficient on porosity. Do the author think that porosity is changing due to shaking? In that case, it should be clarified when discussing the mechanism, because from line 352, I thought it did not. - The description of the consolidation/dilation model is very confusing. To be improved, it would be helpful to get a set of equations and a sketch precising the conceptual model of the medium (is it fractured? porous ?). This would replace the hand waving of lines 199-204. It would provide also an expected range for the linear relationship found between changes in effective pressure and in B. This theoretical framework would be helpful, because they do not provide any citation or evidence for why B would increase with effective pressure (the experiments of Blocher 2009 show a negative trend, but with effective pressure starting at 5MPa, and the apparent B changes in the study may be also contaminated by permeability or Ku changes).  - p 10 and all the discussion on permeability is confusing. Are there permeability changes (as p 10 says) or not (l 350-355)?  - The authors claim there is no issues with hydraulic coupling due to large water storage. But phase lag is not the same before and after the earthquake in some wells. This may be also the sign of change in permeability. Note finally, that your tidal analysis gives only phase with 1 hour of resolution: for M2, that is a phase lag of 30°, which is enormous. Do you have an estimate of permeability and wellbore storage to discard any issue with hydraulic coupling, using directly the equation of Hsieh, WRR, 1987 ? - To show that only B is changing, analyzing M2 may not be enough. One can try to redo the analysis with O1 tidal component, to check that phase is not changing (phase resolution is better with ~24h, the hydraulic coupling should be also better, and the same results should be found). Also the barometric efficiency should change in the same amount as B if the other coefficients are unaffected. This independent analysis would improve the discussion on the cause of the tidal changes, by deciphering the effect of poroelasticity and hydrology in the tidal changes. - You try to apply your model to a variety of geological settings, suggesting a universal behavior. I thought the Chinese Earthquake Administration had a much larger number of monitored wells. Do you have examples of wells not evolving, or with other changes in B than what is expected in your model ? If yes, why does your model not work?   Finally, as a 3rd reviewer, I support the request of the two first reviewers: - the request for logs was to better characterize the aquifers. Are they porous ? Fractured ? Do the wells sample multiple aquifers? What are the constraints (tests on cores, sonic logs) to calibrate the elastic coefficients that are needed to extract correct values of Skempton coefficient ? These questions can be answered more precisely than by stacking raw lithological logs. - the request for seismograms. It seems that other earthquakes, and especially the aftershocks of Wenchuan earthquakes did not trigger any changes. How do they compare ? How much less are the PGA (Peak Ground Acceleration) and PGV (Peak Ground Velocity) ? How did the shaking spectra change ?  To conclude, given the amount of comments from my part and from the other reviewers, I suggest the paper to be rejected, and I encourage resubmission with a major reworking of the paper.   IMPORTANT REVIEWER ATTACHMENTS: If there is a link below then one or more reviewers have uploaded files related to their reviews. To access the file(s), please click on the link below.  These links will expire after 3 uses and so you may also login to the system as an author, go to "Submissions needing revision"  and click the "View Reviewer Attachments" link in the Action column.  \*\*\*\*\*\*\*\* | |