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| **Date:** | Jul 24 2012 12:58PM |
| **To:** | "Yan Zhang" eve\_041744@163.com |
| **From:** | "BSSA Editorial Office" bssa@seismosoc.org |
| **Subject:** | Decision on MS# BSSA-D-12-00178 |
| Ref.:  Ms. No. BSSA-D-12-00178 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.  I also enclose comments by the associate editor.  There is a consensus that your paper has significant technical problems.  The editorial board has evaluated the reviews and decided that the current version of the paper must be rejected for publication.  We have removed it from the review process.  Please note that your manuscript needs significant improvement of its English grammar before it is submitted again.  We urge you to find a colleague who is a native speaker of English to assist you with this process.  We are willing to consider a revised version in which you have addressed the issues raised by the referees and the associate editor.   Any revision will be considered as a new submission but must be accompanied by a letter detailing the changes made in response to these reviews.  The reason for taking this approach, rather than requesting revisions, is that we believe the required changes will result in essentially a new work and that the conclusions may change significantly.  Thank you for your interest in the Bulletin.  Sincerely  Diane I. Doser, PhD Editor-in-Chief  Reviewers' and Editorial comments:  AE's comments: This is a resubmission of the manuscript BSSA-D-12-00070. While it addressed some of the previous raised comments, both reviewers found that the current version still needs significant improvement to improve English grammar, spelling, and organization. In particular, the results and discussions are mixed together in Sections 4 and 5. Instead, the results needed to be presented first without interpretation, followed by a detailed discussion section. In addition, both reviewers have many additional comments that needed to be addressed. Finally, the direct use of figures (Figure 3a) from other recent publications may not be appropriate, and is not really useful. If the authors felt that they are absolutely needed here, proper credit should be given, and in the current paper, the citations are not adequate.  Sentences must be added to both figure captions indicating exactly what material has been obtained (for example, "Figure 3a is reprinted with [or modified with] permission from authors, article title, journal title, volume number, page number(s), year. )  In viewing these difficulties, I would recommend another reject/resubmit so that the authors will have enough time to revise. The authors needed to seriously seek improving the English and structures of this paper before resubmitting it again.   Reviewer #1: The manuscript needs to be edited extensively to improve English grammar, spelling, and organization. There are typographic and grammatical errors in almost every sentence of the paper. The discussion section of the paper currently spans sections 4 and 5. I would like to see this re-organized so that results are presented first with no interpretation, followed by one discussion section. In the discussion, when you compare each well's response with laboratory experiments, you should make this comparison quantitative by comparing your inferred changes in effective stress and B with the experimental results - is there quantitative agreement in addition to qualitative agreement? When you appeal to mechanisms involving increases or decreases in porosity (p. 10 and p. 13 for instance), you can also make quantitative statements about how much change in porosity is needed, and whether these changes are reasonable given that they must occur repeatedly following many earthquakes.   A few specific comments and questions: Table 1: Add the Fuxin well that you refer to repeatedly in the text.   Figure 1: Remove faults so that well locations and letters can be easily read. Add location of Fuxin well.  Figure 3a is directly copied from Blocher 2009. I do not think that this is necessary and in any case the attribution needs to be made more clear and permission must be sought.  Figure 4: Is this well called Fuxing or Fuxin? What is the arrow and label "s.o" in panel (c)?  Page 6, last paragraph: The tidal analysis does not seem correct to me. I think that r should be Earth's radius, not the Earth-moon distance. Furthermore, the formula lambda = omega\*r\*T as you have stated could be simplified as omega = 2\*pi/T, so you are just saying lambda = 2\*pi\*r.  Page 8, section 4.2: "predicted with drilling" does not make sense to me. Are you referring to a theoretical prediction or an observation? A source needs to be cited or the data need to be presented. I do not understand what this sentence means: "The depth of the extra high pressure is usually larger than 3000 m, the pressure will be normal when the depth is less than 3000 m, so we assume those results could be applied to these wells we studied?"  Page 9: Define pc and pf on first line.  You use this phrase several times: "at the beginning of the first pressure cycle" but never explain what it means or its relevance to the problem that you study. I think that if you are going to address Blocher's experiments specifically, you should describe clearly in a couple of sentences what was done.  Page 12: Please use a specific rock type in place of whinstone.  Page 15: <earthquake monitoring records of stations> needs to be removed and whatever you meant to put here needs to go in its place.    Reviewer #2: The authors study the disturbance of the 2008 Wenchuan earthquake on the behavior of water wells scattered in northeastern China.  Though the data seem very interesting, the draft lack convincing analysis for several reasons (see details below). Hence I suggest the authors to operate major revisions.   - The lithology of the rocks is not discussed. Is poroelasticity a valid theory to describe water level changes in granite ?  - The tidal analysis is very surprising: the B coefficient is extremely small (<0.1). For most usual rocks, its value is above 0.5 (see table C1 of Wang, 2000). Either it is related to specific geological features (to be discussed), an important water drainage effect, or it is related to the tidal analysis. It is difficult to assess the quality of the tidal analysis:     o The strain computing program is not accessible. Hence it is difficult to verify its quality and its model. Does it take into account the oceanic tides that has been known to have an effect several tens of kilometers away from the seashore (Berger and Beaumont, 1975)?      o Does the band-pass filter described in figure 6 alter the signal (changing phase and amplitude) ?     o What is the error related to the determination of B coefficient. Put differently, can you show that variation in the B coefficient due to the earthquake is significant relative to the uncertainty in B ?  - Figure 3a and its caption is taken straight ahead from (Blocher, 2009) ! It is better to cite, but not copy-paste information from another paper, especially for ethical and copyright issues. In this case, the figure is complex, from a complex experiment and the information provided is not complete, and it may not support what the authors argue. For instance, Blocher et al 2009 show that the B coefficient is dependent on the modulus of the porous medium (see their figure 8). Hence, it is not clear why the authors discard change in nu\_u in the analysis, whereas it also contributes to the change tidal amplitude (see equation 4). - The discussion is very difficult to follow, as well as the conclusion. This is only partly due to language issues. Parts 4.3 and 4.4 are not well constructed. If I understand well, coseismic water level changes are attributed to permeability changes, that should be reflected by changes in B coefficient. In that case, a cross plot giving water level changes versus B changes would help a lot to summarize the data and support the discussion.  - The authors focus on B variations during few days surrounding the B change. But how does it evolve later ? Does it recover to its initial values ? A study at larger scale should be given.  Typographic comments; - The English is very poor and it needs to be reread by a native english speaker.  - in many cases, number are given by many unsignificant digits (see for instance, page 5: "The wavelength of the M2 wave is about 2,406,329 km") - Figure 2 is difficult to read. The unit of the x-axis is not given (I assume it is day of the month). | |