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## ***Interactive comment on “Models of postseismic deformation after megaeathquakes: the role of various rheological and geometrical parameters of the subduction zone” by O. Trubienko et al.***

### **Anonymous Referee #3**

Received and published: 19 March 2014

#### General Comments

I have read with interested the paper submitted by Trubienko et al. I do like to state that although I am familiar with the current modelling work being done on post-seismic deformation, my geophysical expertise on this topic is limited. However it does give me a good opportunity to read the manuscript as non-specialist reader and sense if the paper is also accessible to a more general audience. My conclusion is that the paper in its current format appears to be well written and investigates a variety of rheological and geometrical variations/scenario's that are needed to properly model the unique post-seismic deformations that have been observed after these 3 recent mega-earthquakes. I agree with the authors that studying these aspects first in 2D

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can provide a better insight than rather testing them out in a more complex 3D model (which I understood the author's are also working on as I have seen already first results presented/published by them). The paper is also more of a technically nature and does not make any hard claims solely based on 2D modelling of the GNSS data it presents. But it does give good indications on which rheology and fault geometry parameters should be input in a full 3D finite element approach.

### Specific Comments

1. I tend to agree with the anonymous referee #1 that the abstract may bit a bit to dense and overwhelming to a non-specialist reader. Actually the sections that follow are easier to comprehend, as there only one modelling aspect is presented each time. I would suggest the authors to shorten/summarize the abstract, and move some of the detailed information/definitions to the sections they are treated in.

2. Page 434, line 18-23. The authors mention and refer to seismic tomography results in section 2. Can this information not already provide more realistic scenarios on the possible shape of the subducting slab? I have seen examples that show clear structure of the deep-Earth derived from three-dimensional seismic-tomography in SE Asian (e.g. group of Wim Spakman at Utrecht University, Netherlands).

3. Page 438, line 8-11. Please explain why you favour using model 2 to study the effect of the locking depth on the post-seismic deformations.

4. Page 440, line 23-26. Please rephrase and ensure correctness and figure references. You mention that for thin plates the (current/total?) horizontal displacement, divided by the co-seismic displacement is close to ZERO 1200 km away from the trench? I cannot see this in Fig 19b, which also extends to 1000 km and not to 1200 km. Also Figure 19b talks about vertical velocities 2 yr after the earthquake, not displacement 4yr after the earthquake divided by the co-seismic displacement as in Fig. 20. But then shouldn't the ratio be close to ONE, incompatible with geodetic observations (Fig 5.) that show a ratio of 1.5 to 2.0 in the far field?

## Technical Corrections

P 429, line 23: it will be the objective of the paper.

P 429, line 26: after the SE three recent giant earthquakes.

P 431, line 15 (refers to Fig. 3). Please add the station names in Figure 3.

P 437, line 7: I would consider use the term "total horizontal co- and post-seismic" displacement instead of just horizontal displacement. Or consider clearly define the terms used the first time they are used in Fig 9 and 10. To me the term velocities is confusing (as describing a constant plate motion), more appropriate seems using "displacement rates 2/4 yr after the earthquake"

P 438, line 2: I am not familiar with the term abobe, I think it should be above.

P 439, line 19-21: Reverse the order of the Figure description, so first Fig. 17 and then Fig. 18.

P 439, line 27: close-field or near-field? Consider defining these terms at the start of the paper (e.g. near-field: 0-200km, middle-field: 200-500km, far-field 500-1600km) and verify they are used consistently throughout the paper.

P 441, line 10: the smaller (remove: IS) the predicted horizontal postseismic velocity BECOMES in the far-field.

P 441, line 11-13: add reference Fig. 21 (b) in this sentence.

P 441, line 14-15: In which figure can I see this? Fig 21a of 22? Horizontal displacement divided by co-seismic displacement (as in the text) or instead velocities 2 yr after the earthquake? See also my comment 4 in the section above.

P 441, line 22: I would add subducting/overriding plate in Fig. 22 (inside the graph and also in the caption). The discontinuity that can be observed seen at distance 0 km first appeared wrong until it is mentioned later on in the text.

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P 442, line 12: parameters THAT ARE important to

P 442, line 13: gave first ideaS ON geometrical characteristics of models THAT MAY BE REQUIRED to fit the observed deformationS.

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Interactive comment on Solid Earth Discuss., 6, 427, 2014.

**SED**

6, C127–C130, 2014

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