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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 #2

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This manuscript presents results of finite element viscoelastic modeling to investigate the role of different geometric and rheologic parameters on postseismic surface deformation after mega-earthquakes. This study addresses relevant scientific questions within the scope of Solid Earth. The authors show that their modeling approach is useful to constrain the controlling parameters that need to be taken into account in detailed 3D models of postseismic deformation. Moreover, although this is not sufficiently emphasized in the text, comparison between model predictions and observations of surface deformation provide useful constraints to determine lithospheric and asthenospheric thicknesses and the presence of low viscosity channels and low viscosity wedges. The presentation of results is clear (also figures) and well structured,

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although some explanations are too short, as I will justify below. My main concerns with this manuscript are that it is not clear at all the novelty or originality of this contribution, and that the methods and assumption are not described in an acceptable way. In this sense I find very unsatisfactory sections 1 (Introduction) and 3 (finite element model). I consider that the authors do not give proper credit to related work and, consequently, the list of references is incomplete. I recommend publication after moderate/major revision, including complete rewriting of the introduction section and providing more detailed explanations of the methods used. I further elaborate these and further ideas in the following notes.

Main comments:

1. The authors refer to previous studies of postseismic deformation related to viscoelastic deformation dating from the early 80's (lines 18-19). Then they simply state that the impact of different rheologic and geometric parameters 'has not been studied thoroughly'. A revision of the state of the art of the topic is completely missing. This revision should be added to clarify the novelty of and significance of this contribution. The two following paragraphs (from line 24 in page 429 to line 21 in page 430) the authors introduce a discussion about the advantages and limitations of the adopted 2D approach, and discuss the effect of other simplifying assumptions such as not imposing postseismic slip in the subduction zone. I suggest moving these paragraphs to section 3 (finite element model), as they are really hard to follow by the reader at this stage. Instead, the authors should use the introduction section to give credit to previous work on the topic, referring to a complete and updated bibliography and, in this context, express more clearly the purpose and expected contribution of this study.

2. In section 3 the authors simply mention that they use a recently published 2D finite element model, without any explanation about the method or assumptions. In contrast, they add a quite confusing discussion about the choice of a Maxwell rheology discussion instead of Burgers or Kelvin-Voigt models. I strongly recommend that they add some paragraphs to summarize the modeling approach.

Minor comments:

1. Page 429, lines 11-12. I recommend adding the date and magnitude (moment magnitude M_w) of the three earthquakes, as usually done in seismology.
2. Page 431, lines 8-9. Use the entire names of institutions accompanying the first time that acronyms are used (e.g. GSI, JPL).
3. Page 431, line 19. Fig. 5 is mentioned before Fig. 4. This also occurs with Fig. 18 and Fig. 19. Please, correct this.
4. Page 432, lines 6 and 23. Clarify which are the earthquake/s studied by Satirapod et al., (2013)
5. In section 2, the authors show deformation data of three giant earthquakes of the last decade and point out the similarity in the postseismic deformation pattern for the regions of the three earthquakes (page 432, lines 26-29). In order to understand if this is a global feature, it would be useful if the authors could compare with previous studies of postseismic deformation in other subduction zones.
6. Page 433 line 15. Replace 'Maxwell' by 'Maxwell model'
7. Page 433 line 24. Replace 'in (Trubienko et al., 2013)' by 'in Trubienko et al. (2013)'
8. Add some length scale in Fig. 7, as for instance the 670 km discontinuity. There are some inconsistencies between text in Section 3 and Fig. 7. The label H_{mantle} (page 433, line 17) is not shown in Fig. 7. Define clearly D_{lock} the first time it is used (page 434, line 17) and indicate this depth in Fig. 7. I do not understand why negative values are given for this parameter (page 435 line 2 and page 438 line 9), instead of the classical positive values used for depths (positive values are given in page 435, line 6). In the text (line 19) the symbol H_{litho} is defined as the thickness of subducting elastic plate, while in Fig. 7 corresponds to the overriding plate.
9. Page 434 lines 12-15. I simply do not follow the logic of the sentence 'One can

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easily rescale. . .’ Please, clarify.

10. Page 435 lines 14-16. In the sentence ‘ .. existing models based on spectral methods. . .’, please provide a reference for this statement.

11. Page 435 lines 23-25. .. the authors mention that the influence of the elastic slab is discussed by Trubienko and coauthors. Please, provide a brief explanation of this discussion here.

12. Provide a physical interpretation of some results: e.g. ‘ the steeper the slab the larger the tendency for uplift’ (page 436, line 16).

13. Page 436, line 24. Remove the first sentence, as it is repetitive. The beginning of the following sentence ‘Now we want to study. . .’ is too informal.

14. Page 437, lines 14-15. Provide references for the three values given for parameter L. An explanation on the method used to obtain these values is also useful. This parameter is mentioned before it is defined (with different definitions) in several places: lines 25-26 in page 437 and lines 3-4 in page 438

15. Move the sentence ‘ The curves of horizontal displacement .. (page 438, lines 18-19) to line 13, before ‘The deformation rates). Is

16. Page 438 lines 16-18. .. the authors state that the dependence of the vertical velocity on the locking depth was already discussed by Melosh (1983). Please, provide a brief explanation of this discussion here. This is needed to enrich the discussion and to clarify the novelty of this study.

17. Page 440 lines 2-3. The authors obtain that the LVW induces a broad zone of subsidence on the continent side of the uplift peak. This is on my opinion an important result that deserves further discussion. Do the authors observe this subsidence in the areas of these earthquakes (or other areas)?

18. Page 441, lines 18-19. Replace ‘. . . maximum to the curve figuring’ by ‘. . . maxi-

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mum in the curve representing..'

Comments about figures:

1. Place labels a) and b) in Figs. 10, 12, 14, 15. Remove small labels a and b located at 'strange' locations within these and other figures. Authors should choose between either using 'top' and 'bottom' in figure captions or labels 'a' and 'b', but not mixing them.
2. Indicate in all figures which horizontal velocities that negative values indicate trenchward velocity
3. Vertical green dashed line is missing in the bottom panel of Fig. 17

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

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6, C100–C104, 2014

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