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

Anonymous Referee #1

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General comments

In this paper the Authors carry out an extensive set of synthetic models of postseismic relxation using a 2D finite-element approach in order to assess the impact of several parameters on the predicted observables. While no new data or modelistic result is presented, I think that their investigations could be a valuable reference for researchers working on numerical models of postseismic relaxation. I therefore reccomend its publication after some further discussion is added, as specified below.

Specific comments

1. I feel that the abstract is too dense, and it turns out quite difficult to read. I suggest

to simplify it by just outlining the main results.

2. (p. 429, lines 17-23). It is not true that existing studies mostly considered a fault with 30 degrees dip embodied in two-layer half-space. Many semi-analytical models of postseismic relaxation have been published in the last decades, both in spherical geometry (see e.g. the papers by F. Pollitz or A. Piersanti or Sun & Okubo in the 90s, or more recent papers by Tanaka et al.) or in a multilayered half-space (e.g. Wang et al., 2006). In many cases, these authors have explored the effects of some model parameters on observables, even within the limitations of an analytical approach. These results should be accounted in the introduction, and a discussion of their results in comparison with the results of the paper would be of great interest.

3. It is not surprising at all that the simple 2D model employed in the paper is not able to reproduce observations of real postseismic motions in many cases, given the 3D structural complexity of subduction zones, as correctly stated by the Authors. The three megathrusts that are cited (Sumatra 2004, Chile 2010, Japan 2011) have been the subject of many studies in literature, most of which employ detailed 3D models (see e.g. Pollitz et al., 2008; Masterlark and Hughes, 2008; Muto et al., 2013; etc.). I think that discussing the presented results in relation to the most recent literature on the three megathrusts would strongly improve the manuscript.

Technical corrections

- p. 433, lines 15 and below: why do you choose this specific value for viscosity? - p. 433, line 17: the symbol H_mantle has not been defined, and is not present in fig. 7. - p. 433, lines 20-23. This period is hard to follow, I suggest trying to rewrite it. - p. 434, line 1: "Burger" -> "Burgers" - p. 434, lines 3-4: remove the comma after "here", and move it after "years" - p. 434, line 5: "Maxwell viscosity" -> "Maxwell rheology" - p. 434, lines 28. Maybe a "We set" is missing before D_lock - p. 435, line 17: "non dimensionalyzed" -> "non-dimensionalized" - p. 435, lines 17-18: why do you say that the angle of the elastic slav at depth affects mainly the vertical motion? From the

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curves, it seems to me that the horizontal motions are affected as well. - Figs 7, 8, 11: I suggest to annotate the three block diagrams with all the relevant symbols (H_litho, H_asthe, H_mantle, the dip angles, etc).

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