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Interactive comment on “Segmentation of the Izu-Bonin and Mariana plates based on the analysis of the Benioff seismicity distribution and regional tomography results” by K. Jaxybulatov et al.

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The manuscript “Segmentation of the Izu-Bonin and Mariana plates based on the analysis of the Benioff seismicity distribution and regional tomography results” presents new tomographic models of the subduction setting under the Izu-Bonin and Mariana arcs (as correctly pointed out by reviewer W.P. Schellart). Based on the obtained models and on the deep seismicity, the authors propose an evolutionary scenario of subduction in the studied area. On the whole, the tomographic models described are largely comparable to those previously obtained by other authors, with some new detected

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features. As stated in the manuscript “any attempt to compute a new model using independent approaches is an important step to corroborate the existing information”; in this perspective I feel that this work is a valuable contribution, however it needs some amendment to fully hit the mark. In the following I’m going to list my suggestions:

1) I believe that the advertized new modeled features (with respect to previous tomographic model) should be listed and described more clearly in the manuscript-perhaps in a dedicated section-in order to make the comparison with other models easier for the reader, but, most of, all in order to set the measure of advancement in the knowledge of the studied region after the present work.

2) The whole Section 3 “Slab-related seismicity” is somewhat misleading. Apart from the unnecessary redundancy of phrases like “Unlike the traditional way of presentation of the Benioff seismicity across subduction zones.”, with the intent to describe the seismicity along the subduction arcs, this becomes a mostly interpretative section which anticipates concepts that should be more properly expressed after the discussion of the tomographic results. In fact, based on the distribution of seismicity-and seismic energy release-the authors identify seismicity clusters, whose geometry is somewhat arbitrarily drawn, giving them a key role in the discussion section. I would see the data in Figure 2B and 2C compared with tomographic models along the same vertical section, then seismicity clusters can be identified and one can assign them a role. Otherwise the reader is left with the uncomfortable feeling that the terms of the equation “deep seismicity=subducting slab=fast velocity anomaly” are used (or not used) by tomographers at their best convenience.

3) Results of synthetic tests are generally satisfactory, given the well-known limitation imposed by the geographical characteristics of the study area. Other than the odd/even test to estimate the influence of random noise, I would try an “old/new” test, to evaluate the influence of the lower data quality of the initial ISC catalog on the inversion results.

4) About the Discussion section, all my comments are included in the questions raised

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by the reviewer W.P. Schellart, therefore I will not go through it.

Minor issues: I found few typos and some inconsistency in the bibliography; in the following I list them, but I recommend a careful check of the whole manuscript.

Page 824, line 8: substitute “form” with “forms”

Page 824, line 20: substitute “that causes steepening” with “causing the steepening”

Page 826, line 22: references “Gorbatov et al., 2003” and “Miller et al., 2004” are wrongly referred to

Page 826, line 25: withdraw “fact of”

Page 827, line 3: avoid “and it is greatly appreciated by the scientific community”

Page 827, line 20: withdraw “It is important that”

Page 828, line 13: substitute “to one” with “to the one”

Page 828, lines 12-14: the statement “Our approach is less conservative compared to one used by Engdahl et al. (1998), and it keeps more data, which is favorable for tomographic inversion.” is by far too simplistic. It needs to be properly clarified, and possibly quantified (see point 3 above)

Page 828, line 23: withdraw “implying”

Page 829, line 10: substitute “station” with “seismic”

Page 829, line 22: substitute “for the four” with “for four”

Page 830, line 1: The phrase “Unlike...zones” should be avoided

Page 830, line 26-28: The phrase “In this body...in the slab” should to be avoided. The Authors do not discuss anywhere in the manuscript the quality of the events location, nor the relationship of the slab shape with these linear structures

Page 831, lines 1-13: there is quite a bit of confusion here among slab segments

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and seismicity clusters. I suggest to move the whole section 3 after the tomographic images, rearranging it in order to provide a comparison between tomography (both P and S-waves) and seismicity along the section of Figure 2B and C (see point 2 above)

Page 831, line 22: withdraw “which is”

Page 831, line 25: substitute “The correlation of” with “The correlation between”

Page 831, line 26: substitute “the reliability model” with “the reliability of the model”

Page 832, line 1-2: Referring to a particular Figure in the manuscript could be useful here in supporting this statement

Page 834, lines 3-5: the sentence “The anomalies were defined...certain band” is not clear in the present form, please rearrange it

Page 834, line 21: substitute “behavior” with “geometry”

Page 834, line 23: substitute “Figure 10” with “Figure 8”

Page 836, line 2: please add reference(s) after “migrate forward”

Interactive comment on Solid Earth Discuss., 4, 823, 2012.

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