

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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Dear F. P. Lucente

We are very grateful for your friendly and constructive comments. We took into consideration most of them which required considerable work to be done including some new calculations.

The revised manuscript is given in supplement. All the corrected parts in the manuscript are highlighted with red.

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1) I believe that the advertised 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.

REP1: Example of comparison of our results with other ones (Miller et al., 2005) is shown below (Fig1). We think that comparing our results in each section (horizontal or vertical) in this way with all others is not reasonable. Surely the results of different authors are really different in the details, so we compared the main features, character of subductions and suggested geometric models. The main differences, that we thought are worth, are indicated in the paper. And it should be noted, that in comparison with other works, we paid more attention to the verification tests, that makes our results are more reliable.

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.

REP2: The problem of representation of velocity anomalies with the seismicity along the slab is that the velocity anomalies would strongly variable depending on the lo-

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cation of the profile, whereas the seismicity is taken from a wide band and includes all seismicity. Certainly one might play with the position of the profile and obtain nice correlation features. However this would be not a really scientific way. Nevertheless, which have produced a section, as requested by the reviewer, and we show it below (Fig2). We do not believe that this picture will be useful for the paper.

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.

REP3: We have made the test according to the reviewer’s advices. The results of reconstructions corresponding to two data subsets for the periods of 1964-1985 and 1986-2007 are presented below (Fig3, Fig4, Fig5, Fig6). Indeed, the reconstructed features appear to be similar in two data subsets, although the older subset contains much smaller data amount of much lower quality than the newer subset. However we decided not to include these pictures to the main manuscript because there is a similar test with odd and even events, an additional picture may overcharge the paper.

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

Corrected.

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

Corrected.

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

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wrongly referred to

Corrected.

Page 826, line 25: withdraw “fact of”

Corrected.

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

Corrected.

Page 827, line20: withdraw “It is important that”

Corrected.

Page 828, line13: substitute “to one” with “to the one”

Corrected.

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)

REP4: Number of earthquakes that we selected for tomographic inversion approximately 2 times larger (Our: 17827, Engdahl et al.(1998):8178) than one selected in Engdahl et al.(1998). It can also be compared in figures below (Fig7, Fig8), where distribution of earthquakes in map view is shown. Colored dots depict earthquake hypocenters.

Page 828, line 23: withdraw “implying”

Corrected.

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

Corrected.

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

Corrected.

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

Corrected.

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

REP5: We agree that this statement was not sufficiently supported by the existing data and we have removed this sentence from the text.

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

REP6: We find that the existing structure of the paper is quite appropriate. Just after the describing the tomographic images, we start to interpret them. If we include the seismicity section in between, this would produce a confusing gap.

Page 831, line 22: withdraw “which is”

Corrected.

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

Corrected.

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

Corrected.

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

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here in supporting this statement

Referring to figure 4 was added.

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

Rearranged.

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

Corrected.

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

Corrected.

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

Added.

Please also note the supplement to this comment:

<http://www.solid-earth-discuss.net/4/C667/2012/sed-4-C667-2012-supplement.zip>

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

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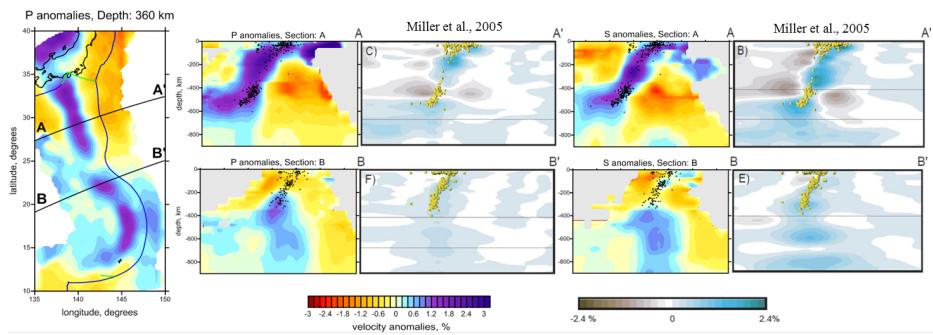


Fig. 1. Comparison (see REP1)

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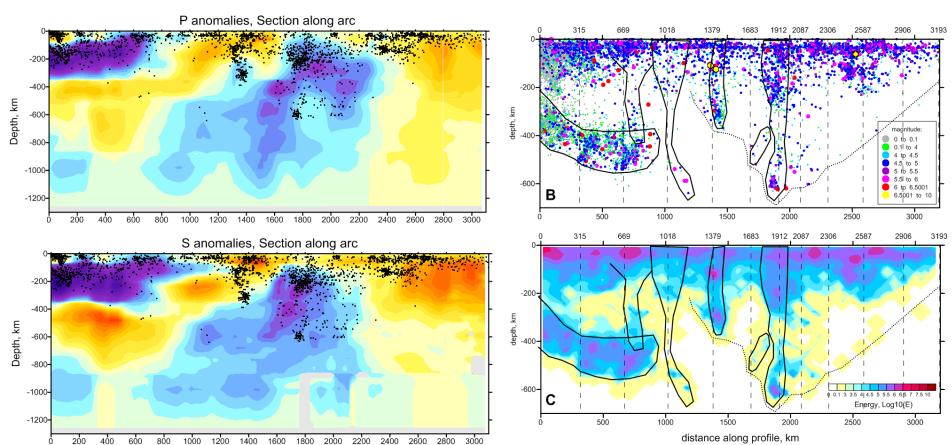


Fig. 2. Section along arc (see REP2)

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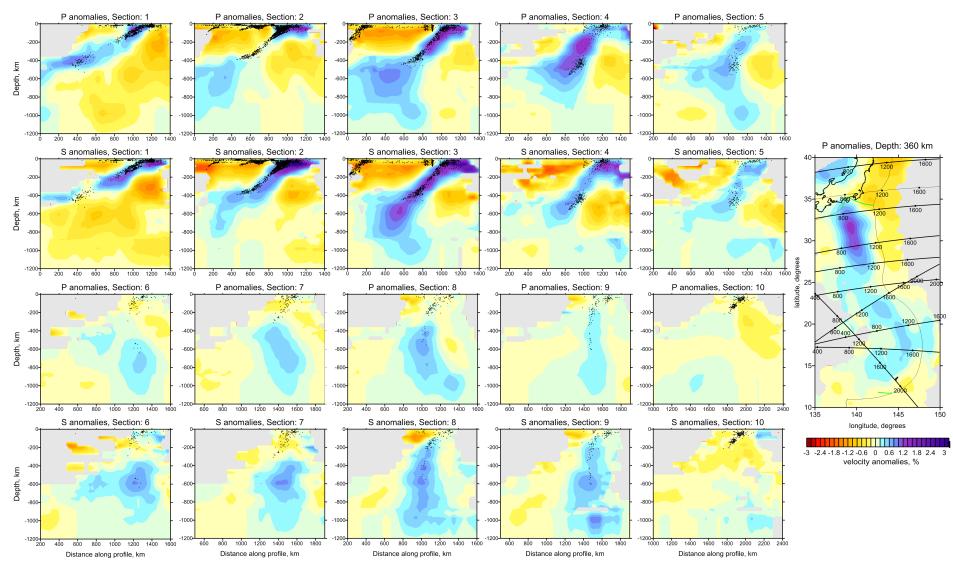


Fig. 3. Vertical sections (1964-1985) (see REP3)

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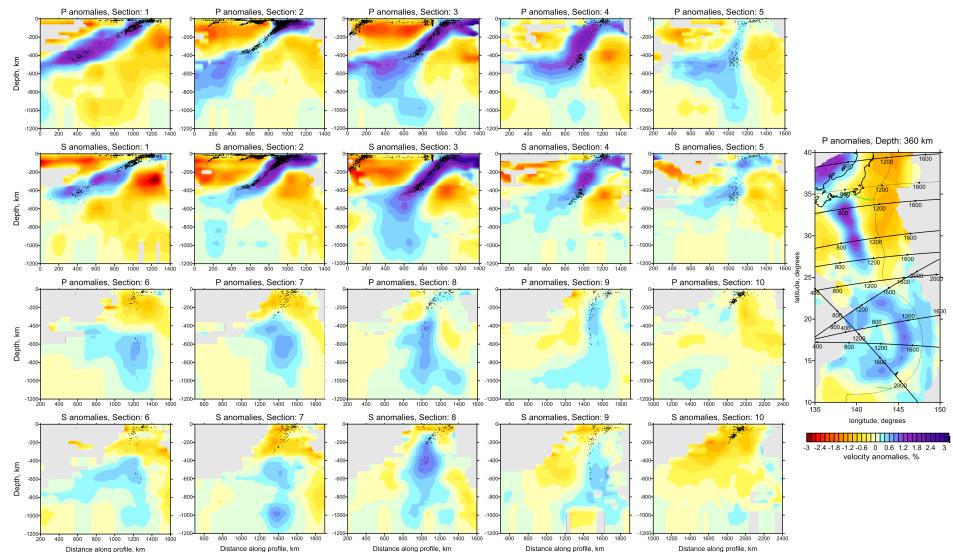


Fig. 4. Vertical sections (1986-2007) (see REP3)

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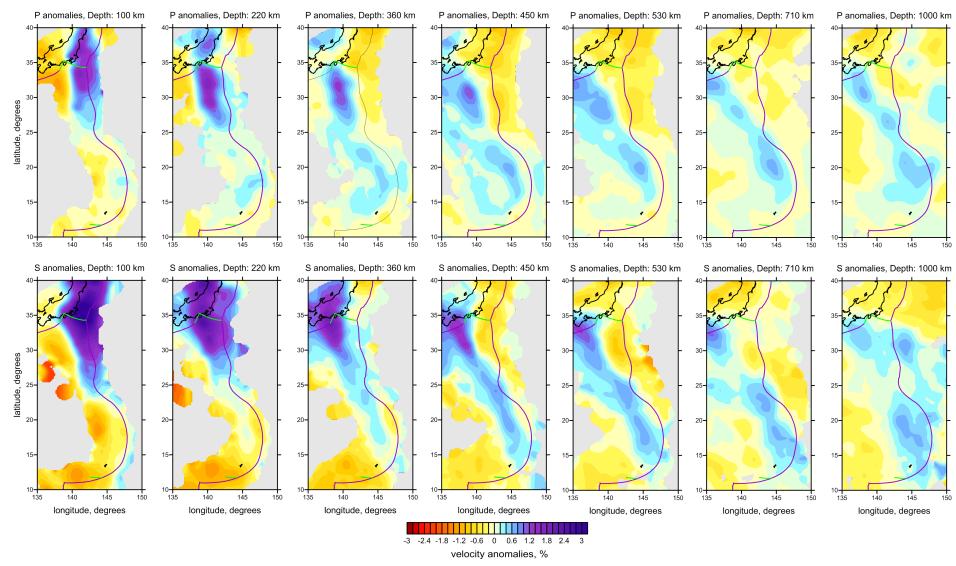


Fig. 5. Horizontal sections (1964-1985) (see REP3)

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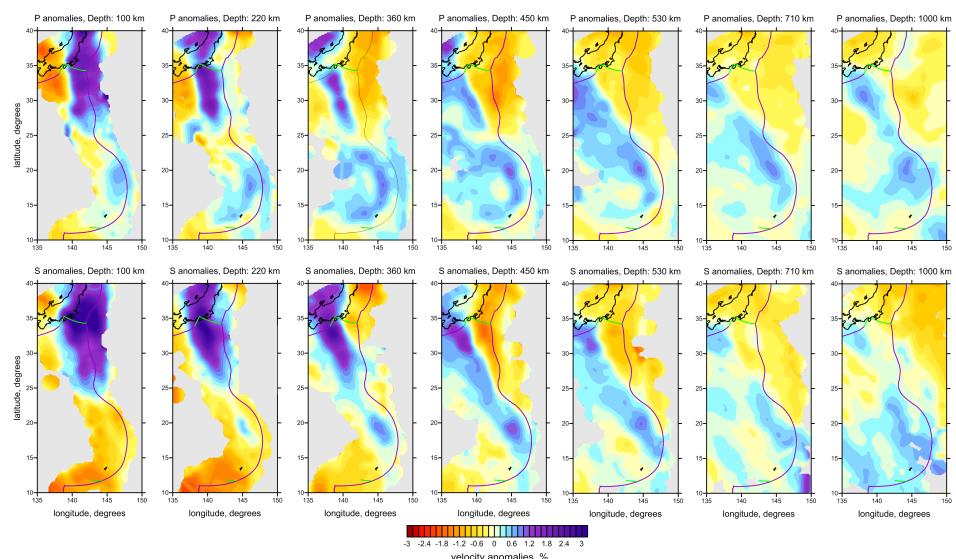


Fig. 6. Horizontal sections (1986-2007) (see REP3)

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Our:

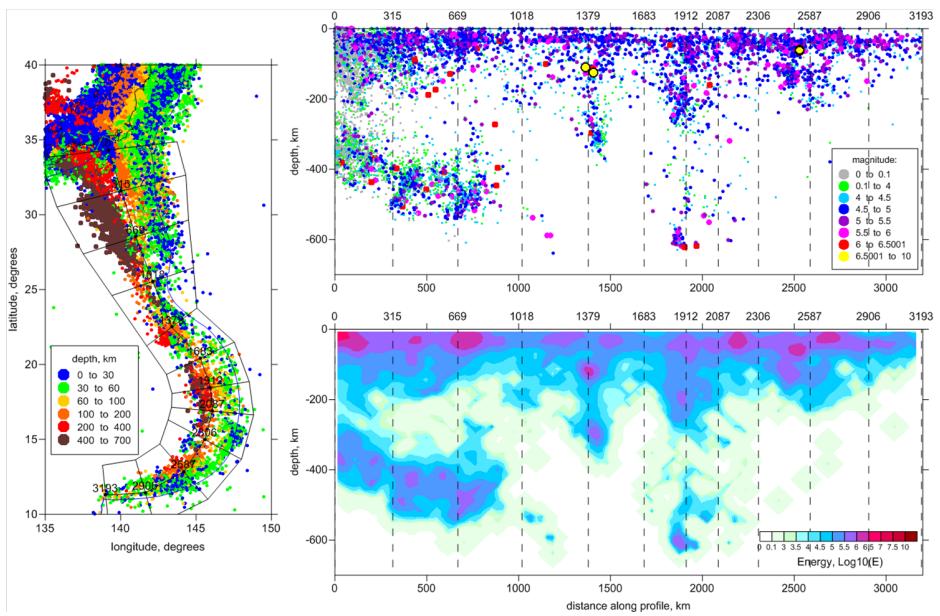


Fig. 7. Distribution of earthquakes (see REP 4)

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Engdahl et al.(1998):

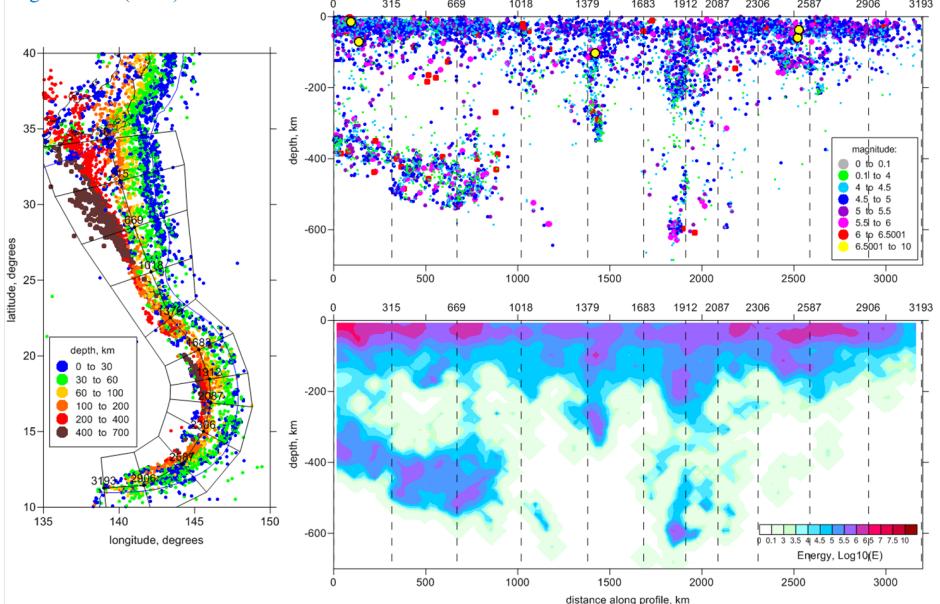


Fig. 8. Distribution of earthquakes (see REP 4)

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