

Interactive comment on “1997 Kronotsky earthquake and tsunami and their predecessors, Kamchatka, Russia” by Joanne Bourgeois and Tatiana K. Pinegina

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Authors' responses to reviewer #2

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Response to review of Dr. Rob Witter

Response to general comments

"... the introduction does not clearly articulate the relevant scientific questions. One

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way to improve the introduction would be to specifically lay out the central questions addressed by the research, and how the study addresses them." Response: we will write a short introductory paragraph BEFORE the other paragraphs laying out the basic questions: How do tsunamis inform earthquake interpretations? The 1997 tsunami requires a different earthquake source region than geophysically interpreted: we address the (previously unrecognized) significance of the 1997 tsunami to that interpretation. Do the historic earthquakes in the northern part of the KSZ characterize it as rupturing in shorter segments than southern part? How does the prehistoric record inform the question? What are the strengths and limitations of reconstructing prehistoric tsunamis, even with strong age control from well-dated and well-mapped tephra?

"For the paper to meet international standards of practice in tsunami science, the authors should place greater emphasis on the methods used in the study. In fact, the authors are leaders in this field and pioneered many of the methods used today. However, the brief presentation of these methods obscures important details and raises questions about some of the conclusions of the study. For example, the authors describe tsunami deposits generally as sand sheets that become thinner and finer-grained in a landward direction (line 245). But was this used in the study?" [Response: "Yes, in the cases of historic events and the general trends in prehistoric deposits]

"In detail, the deposits mapped in profile 110 are thickest in exposure 45, the farthest site from the sea;" [Response: "reviewer is wrong about this" for example, the historic deposits do not even occur in excavation 45; the deposits he is talking about are older and cannot be correlated from excavation to excavation; the fact that the deposits are more numerous in excavation 45 is a matter of preservation and identification, NOT thickness or grain size]

" and no particle size data are included in the results." [Response: "Only relevant for historical deposits except for general trends" would the reviewer like to have copies of all our field notes? These are general trends" On one hand he wants us to write more, but we feel he is looking for contradictions where there are none.]

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"What differentiates a sand layer on the coast of Kamchatsky Bay from a flood deposit of the Chazhma River," [Response: "we can add a sentence that flood deposits here are muddy, and most of our sites are above flood level] " or a sandy fan deposit produced by storm wave wash over? In 2013 Typhoon Haiyan's storm surge produced sheet-like overwash deposits up to 8 cm thick that extended over 1000 m inland (Pilarczyk et al., 2016)". [Response: "(there is a 2017 paper on Haiyan deposit as well) we can repeat as in prior studies that tropical storms & their surges do not occur at these latitudes, and our elevations are high enough to preclude regional storms; we don't see a reason to repeat what is in prior papers, unless more than one reviewer requests it. But if the editor asks, we will comply, in the Supplement, not in the main text.]

" In some of the figures, the predominant sediment is sand –how do tsunami deposits stand out in this sedimentary environment? Do aeolian processes deposit sheet-like sand layers along Russian coasts?" [Response: "if the editor requests this, we can copy and paste our discussions from prior published papers, in the supplement.]

"The authors should provide additional explanation on the methods used to determine paleo-elevation and shoreline positions used to estimate past inundation and runup." [Response: "see Figure S5 as well as the paper that actually uses this information in detail (Pinegina et al., 2013). What more does the reviewer want? Why repeat what is published?]

"One example that needs clarification is application of the method to deduce long-term uplift and subsidence. For example, the authors present contrasting topographic profiles in southern Kamtchatsky Bay that they interpret as evidence for opposite senses of tectonic deformation over distances of what looks like less than about 50 km." [Response: The reviewer's question does not have to do with method but with cause (tectonic segmentation, not the point of this paper). Reviewer appears to have read Pinegina et al., 2013, which discussed tectonics more than this paper, for which it is not the point; further discussion below]

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"Profile 001 in Figure 7 shows the low-lying Bistraya River valley that flanks higher coastal deposits, yet the authors do not present clear evidence for tectonic subsidence." [Response: In general, we rule out uplift here; see next response] "Does the evidence preclude coastal erosion that removed tephra deposits seaward of the Bistraya River that drape the lower valley topography?" [Response: "yes, some tephra are eroded because the profile became lower (subsided) –there are older tephra that are essentially at sea level, whereas tephra are not preserved on the coast below the general storm limit (see our discussion and also Figure S5)].

"In Figure 8, the authors interpret Chazhma profile 110 as evidence for uplift based on reconstructions using seaward termination of tephra deposits" [no –based on elevation of oldest preserved tephra]. "If there is a marked change in tectonic deformation between the Bistraya and Chazhma Rivers, what mechanism accommodates the opposite senses of motion?" [Response: The reviewer does not dispute our observations but wants a tectonic explanation –see our response above –reviewer appears to have read Pinegina et al., 2013, which discussed tectonics more than this paper, for which it is not the point. We do note in this paper that Kronotsky is where the Emperor Seamount chain impinges on Kamchatka]

"A more complete explanation of methods, and presentation of the evidence will help substantiate the authors' interpretations here." [Response: "we can expand our methods section in the supplement if requested by the editor. However, reviewer does not dispute our data or our interpretations, as far as we can tell.]

"The figures are well designed, readable, and present important observations. However, an additional figure might be added to demonstrate how tephra stratigraphy is applied to deduce tectonic subsidence at Bistraya" [Response: "see Figure S5 –Storozh and Bistraya are along the same coastal plain, as noted in our introduction to the area.]

"Additional improvements to Figure 1, suggested in comments in the reviewed

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manuscript copy, could help the reader place the study sites into the overall tectonic and geographic setting." [Response – will do.]

"Finally, I want to see representative photographs of the tsunami deposits to help show how they are distinguished from sandy soils, fluvial deposits and sand deposited by storm waves and aeolian activity." [Response – this can be shown in text, but reviewer should know well that photographs will not show it, and that is true even when we make meter-long excavations, not cores, as most other workers do most of the time, for example, in Japan, at lower elevations than in Kamchatka.]

"The paper would be incomplete without the supplementary material. More detailed explanation of the methods, and tables showing the reconstructions of paleotsunami deposit elevations based on tephra stratigraphy help substantiate the authors' interpretations. To help readers understand the paper without relying on the supplement, some of these details should be included in the main paper." [Response – which? We differentiate our data and interpretations based on those data – in the main body of the text – from additional information on methods and other peoples' observations, e.g., of tide-gage records. If we expand our discussion of methods with regard to identifying tsunami deposits, it would be in the supplement because it is already published, as is our method for reconstructing shorelines. Note that in this field location we have several historical deposits to use as interpretive guides. Should we say that more specifically?]

Additional responses to Reviewer 2, Dr. Witter:

General comment about distinguishing tsunami deposits. We can add a few lines/sentences but do not feel the need to repeat material in prior publications. We would put it in the supplement. Note that tropical cyclones like Haiyan do not occur at these latitudes.

General comment about sea level history. We will add a sentence about late Holocene sea level stability in this region; our analysis goes back only about 2000 years and thus does not require a repeat of material discussed in detail in a prior publication (Pinegina

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et al., 2013) that covers more time and where the amounts of relative sea level change are more relevant.

General comment about tectonics: They are not the focus of this paper. We do note that the profiles indicating uplift are close to Kronotsky Peninsula, which is going up. It is not the purpose of this paper to discuss tectonics, as we did in a prior paper. We do not have a long enough record here to do the same kind of analysis. Nor has this region been examined for active faults, as had the Pinegina et al. 2013 paper (and companion Pedoja et al.).

General comment about limitations to methods. We believe we are very clear that our methods have limitations. Reviewer tends to point these out, also, but we cannot find examples where we haven't pointed the same out ourselves, with a few minor examples noted below. For example, p. 2 line 4-5 of supplement, our sentences actually describe specific cases where inundation will be underestimated or overestimated, in two sentences. We feel as if the reviewer did not read the sentences with their qualifications. There is no general rule, and we are also clear throughout that our estimates are minima, within the constraints of the methods. Same on that page with lines 15-16. However, we will do some rewriting of that sentence to make it clearer.

Dr. Witter makes some other important but completely addressable points in the pdf review of the manuscript, which we address below:

Line 7 (abstract) – We will change this word to "portion" and used "segment" when speaking specifically of segmentation, which is the more technical term.

Lines 35ff. We think this is an appropriate sentence for an introduction, which is setting up our study.

Figure 1. We will add scale. The addition of boxes will obscure information. Olga Bay and Kozlov Cape are in tables. Cape Africa was in the table but has been removed, thus we can remove that, but there is no real reason to remove information, this is a

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locator map.

Line 74-75. Reviewer's questions are already addressed in the existing manuscript. This is an introduction.

Line 79. Reviewer's questions are already addressed in the existing manuscript. This is an introduction. We wish the reviewer should have gone back and re-read his comments/questions after reading the paper.

Figure 2. Will address small corrections. Points above hypocenters can be mapped.

Figure 3. We will expand the caption. However, clearly the reviewer did not read the caption carefully because it states that the photos were all taken on the same day. Figure S3, previously published and hence not in the main body of the text, shows a sketch. We can label sand and sea foam.

Line 201. We do not use the word predecessors because it implies the other events were similar to 1997, and they were not.

Line 206-207. Why crossed out?

Table 1. Questions will be addressed, table will be easily clarified.

Line 213. Will clarify – it's the bolded column on the table.

Lines 211-222. Will correct labels and shorten text.

Lines 224-225. We disagree with deletion. It is important to note that catalogues existence does not mean that all events are recorded.

Figure 4 left. Will remove asl and note that the lower right corner of each profile is 0.

Lines 251-255. Reviewer is wrong; this has nothing to do with 3-D tsunami behavior. We are here defining NOT tsunami elevations, but rather deposit elevations and distances. Even IF the tsunami did not overtop some point but came from the side, its sediment (minimum, as we note) runup and inundation on THAT PROFILE would what

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it is. If the tsunami got there and left a deposit, that distance and elevation had to have been reached.

Figure 5 caption. We will add a note about this being a simple 2-D profile. HOWEVER, if the tsunami came from the side rather than over the beach ridge, it had to carry the sand even farther, so its elevation would almost certainly be greater than as shown, and its inundation at least as much. The sketch is based on data from this profile, but also on our knowledge of the regional topography – beach ridges do not just appear on 2-D profiles. See Figure 4.

Line 266. We wish the reviewer would read on and then correct his question. There are three most consistently present and one that is not as extensive, as stated in the next sentence.

Table 2. We are sorry there is more than one designation for these tephra, such information needs to remain in the table. We will use AD dates in the text.

Line 279 – insert “and” between “past” and “must”

Line 282 – we can elaborate a bit, but here we are dealing only with the last 2000 years or so, we don't need to review the last glaciation. Insert “relative to sea level” after changes in elevation. [reviewer later notes, as we have, that some profiles have gone up and others down, not expected if from eustasy]

Line 287. The name Chazhma is just our shorthand. Details of the rivers is not necessary. The table lists latitudes and longitudes. We cannot provide topographic maps as they are proprietary. Google Earth is available to reader.

Line 309. If the tide was low at the time of the survey, we would get a maximum, whereas we want a minimum, so we correct to high tide. We could add this note to methods.

Line 316-317. Reword: We also report the maximum height the tsunami had to exceed IF it traveled ACROSS the profile. Reviewers concern is also addressed in the next

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sentence.

Line 319: Add: note that for most profiles, the ridge crossed continues laterally (e.g., see Figure 4 profiles)

Line 330 – this concern is already addressed in our methods text on sediment inundation and runup.

Table 3. Will write out m.a.s.l. The depth to these 20th century deposits is insignificant. For prehistoric deposits, it is recalculated.

Line 370. Add “and other historical deposits”.

Line 388 resolves to most slip

Line 410. Disagree with deletion.

Figure 7. Reviewers question is answered in the figure text. Figure 7 caption – older tephra are preserved below sea level, thus subsidence –clarify in caption.

Figure 8. We will add to the caption – “sand” undifferentiated means that the section was too sandy to identify individual deposits.

Line 456. First, we do not use cores, we use excavations. Still, correlating individual beds is fraught with potential errors. We would argue with anyone who says otherwise, based on historical examples (closely paired events, e.g.) and extensive field work.

Figure 9. Profile 140. Yes, this section has older material, which we report here but do not use in our analysis. The older tephra identifications are tentative, but done in consultation with Vera Ponomareva who has mapped them regionally. Both profiles – when deposits of tephra or sand are more patchy, they are shown as not extending across the section; we will add that information here and note in the Key in Figure 4.

Line 464 (figure caption) will add that there is not a river nearby.

Line 470. Soil between sand layers was not distinct enough. Will add to text.

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Line 475-476. Yes, in this case runup could be different, but not for the other excavations on this profile. We are being honest about cases where water might have come a different way, though we think it unlikely. Such uncertainty does not affect the overall analysis.

Line 480. Deposits cannot be correlated. We never count more than the maximum number of deposits. However, the deposit with the greatest sediment inundation may be on a low profile, whereas the deposit with the highest sediment runup may be on a steeper, shorter profile. We are likely undercounting.

Line 489-490. Reviewers comment is exactly why the lack of higher paleo-runups is an artefact of the sites used. Yes, the higher profiles were lower in the past, so we cannot “get” a record of high runups.

Lines 505-507. Add to caption. One would expect smaller events to be more frequent, which in general is the case (clustering in the lower front), whereas larger events are more scattered. However, the 1923 deposit is about as large as any, and it's within a short time interval. If we removed that axis, information would be lost. But we want to point out there is some time bias.

Line 523 and Figure 11. The red line is runup and does not go below 5 m. We are considering only the largest tsunamis, and by staying about 5 m, as clearly stated.

Lines 552-553. We are pointing out that even modern survey data have limitations. In our case of studying deposits where there are no survey data, we still for the historical record pair runup and inundation. However, for the prehistoric record it is simply not possible. Reviewer does not seem to contest that. We are thus recording typical recurrences of sediment runups and inundations over the last 2000 years, while being careful not to overinterpret.

Line 556. Please keep reading the paragraph, it explains why in this case, with our data.

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Line 561-562. Add: While we cannot determine the tide level for “1923” because it is one of two events, the 1997 earthquake occurred just after local high tide, so tide cannot explain the higher record from 1923.

Line 568. change wording from “asperity” to “locked or continuously slipping zone” because it could be locked (an asperity) or continuously slipping (as interpreted by some of the gravity people)

Line 584-585. The issue is illustrated by Hayes’ more westerly location, which cannot explain the tsunami, even though his rupture focus is more northerly than others’ interpretations. We are not sure what the reviewers point/question is.

Line 588. Seismic gap is a well-defined and oft-used term. It completely describes the 1997 location in our interpretation (and earlier Russian publications)

Line 590. An asperity is defined by IRIS and others as a locked zone with POTENTIALLY high slip. We are not sure where “elsewhere” the reviewer means.

Line 605-606. (as shown in our this paper, our papers, and papers on Japan—need we repeated here?) A large part of the paper relies on tephra for reconstructions. As have been used in Japan, as well. Is reviewer disputing this statement?

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