Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2017-172-RC2, 2017

Natural Hazards and Earth System Sciences
Discussions

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

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Received and published: 19 September 2017

General comments

This paper reports observations about sand deposits inferred to record historical and prehistoric tsunamis along the northern Kamchatka subduction zone, Russia. The spatial distribution and elevations of the historical tsunami deposits bear on the characteristics of historical earthquake sources, one in 1997 and two in 1923. The authors conclude, based on tsunami deposit runup along southern Kamchatsky Bay, that the 1997 earthquake likely ruptured north of Kronotsky Peninsula, and farther than previous source models contend. The authors also suggest that the April 1923 earthquake

may have attained magnitude 8, despite sketchy evidence supporting this assertion. Because the prehistoric tsunami deposits do not have larger inundations or runups compared to historical deposits, the authors cautiously speculate that the northern Kamchatka subduction zone produces smaller ruptures than the southern Kamchatka subduction zone.

The paper presents new and important findings that may improve assessments of the size and extent of earthquake ruptures in 1997 and 1923. The authors aim to address questions about tsunami recurrence statistics along the northern Kamchatka subduction zone. However, the introduction does not clearly articulate the relevant scientific questions. Instead, it begins with a reference diverting to the 2006 Kuril Islands tsunami, not the subject of the paper, and a plea for more post-tsunami surveys in hard-to-reach places. One way to improve the introduction would be to specifically lay out the central questions addressed by the research, and how the study addresses them.

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? In detail, the deposits mapped in profile 110 are thickest in exposure 45, the farthest site from the sea; and no particle size data are included in the results. What differentiates a sand layer on the coast of Kamchatsky Bay from a flood deposit of the Chazhma River, 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). In some of the figures, the predominant sediment is sandâĂŤhow do tsunami deposits stand out in this sedimentary environment? Do

aeolian processes deposit sheet-like sand layers along Russian coasts?

The authors should provide additional explanation on the methods used to determine paleo-elevation and shoreline positions used to estimate past inundation and runup. Even though these methods are described elsewhere (Pinegina et al., 2013; MacInnes et al., 2016), readers need a brief summary of how the methods are applied here to assess the technique and understand the authors' conclusions. 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 (need a scale bar in site location map). 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. Does the evidence preclude coastal erosion that removed tephra deposits seaward of the Bistraya River that drape the lower valley topography? In Figure 8, the authors interpret Chazhma profile 110 as evidence for uplift based on reconstructions using seaward termination of tephra deposits. If there is a marked change in tectonic deformation between the Bistraya and Chazhma Rivers, what mechanism accommodates the opposite senses of motion? A more complete explanation of methods, and presentation of the evidence will help substantiate the authors' interpretations here.

The authors write well and have put together an interesting paper, not hampered by jargon, understandable, and about the write length. Additional methods and explanation of how they are applied would not make it overly long. The title reflects the meat of the paper and the abstract adequately summarizes the findings.

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. Additional improvements to Figure 1, suggested in comments in the reviewed manuscript copy, could help the reader place the study sites into the overall tectonic and geographic setting. 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.

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.

Also, I agree with the comments submitted by reviewer 1 and found them constructive. If addressed appropriately, the paper could be improved substantially.

Finally, my review also includes detailed comments tracked in the attached copies of the manuscript and supplementary materials.

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2017-172/nhess-2017-172-RC2-supplement.pdf

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