

## ***Interactive comment on “Evidence of preliminary prognosis of appearance of catastrophic earthquake and strong tsunami in the region of Tarapacá, Chile” by Raissa K. Mazova et al.***

**Raissa K. Mazova et al.**

jorge.vandenbosch@uantof.cl

Received and published: 4 February 2020

Dear Editor, I sent you our author comments to comments from both Referees. Referee 1 examined our work in detail and made comments, with which we largely agree. The Referee 1, obviously, well represents the computed method described in our paper. Therefore, his comments were “line by line”, i.e. detailed, which can also be answered essentially. But, with a number of comments by Referee 1, we do not agree, as I write in detail in my answers in Attachment 2. Referee 2 did a great work comparing our paper with existing publications on this event. However, unfortunately, he is obviously had little to do with the work on the application of the

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keyboard model of the earthquake to the description of catastrophic earthquakes, its justification, and the further widespread use of this technique to model complex catastrophic events accompanied by tsunamis. This model was successfully applied to the Kuril-Kamchatka subduction zone, which almost accurately indicated (first in the world!) the location of the epicenters of the three strongest tsunamigenic earthquakes, September 30, 2006, November 15, 2006, and January 13, 2007 that followed in the same region six months after the work was published (Lobkovsky, L.I., Mazova, R. Kh. et al., (Doklady RAS, 2006)). Since the paper (Mazova R.Kh. and Ramirez J.F., 1999) provides conclusions of the authors on the similarity of the continental slope of the deep sea trench near of northwestern part of the Chilean coast and Kuril-Kamchatka area, in this paper the keyboard model of the earthquake was also used. Since this model is widely published (see, e.g. [1,2] below), we did not see the need to go into its details, but considered only its use for modeling this earthquake. Unfortunately, the Referee 2 also “did not notice” the study of this process by the method of spectral analysis, which is rarely enough, in such a volume, used to analyze the wave characteristics of the tsunami process. In our answers, we tried to briefly explain some of the errancies of our colleague (see Attachment 3). I do hope that our answers to comments from Referees will be convincing for you to consider the revised version of our paper for possible publication. 1. L.Lobkovsky, I.Garagash, B.Baranov, R.Mazova, N.Baranova. Modeling Features of Both the Rupture Process and the Local Tsunami Wave Field from the 2011 Tohoku Earthquake // Pure Appl. Geophys. (2017). V.174, p. 3919-3938, doi:10.1007/s00024-017-1539-5 (March 2017) pp.1-20. 2. L.I.Lobkovsky, I.A.Garagash, R.Kh.Mazova, Numerical simulation of tsunami waves generated by the underwater landslide for the Northern Coast of the Black Sea (Dzhubga Area) // Geophysical J. Int., V.218, Iss.2, Aug 2019, p. 1298–1306, <http://doi.org/10.1093/gji/ggz221>. Sincerely yours, on behalf of all co-authors, Raissa Mazova

Please also note the supplement to this comment:

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<https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2019-278/nhess-2019-278-AC2-supplement.pdf>

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Interactive comment on *Nat. Hazards Earth Syst. Sci. Discuss.*, <https://doi.org/10.5194/nhess-2019-278>, 2019.

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