

Dear Editor Prof. Arcady Dyskin,

We are very grateful to you and the two reviewers for encouraging positive evaluation of the manuscript of our paper and constructive and thoughtful comments and suggestions, and also for the recommendations and corrections proposed by the anonymous reviewer to improve the quality of the manuscript. We have addressed all the issues raised in his review and have modified the text of the manuscript accordingly. The authors would like to kindly acknowledge both detailed reviews. Below are responses to the two reviewers and a summary of the changes performed following the anonymous reviewer's comments and recommendations.

Authors' response to Reviewer 1.

We would like to thank Prof. S. Sherman for his encouraging positive evaluation of our manuscript. We are deeply grateful for his careful and meticulous reading of the paper and concrete and insightful comments and deep analytical conclusions on the manuscript which we really highly appreciate. Thank you once again for evaluating our contribution to the solution of the problem of wave geodynamic processes.

Authors' response to Reviewer 2.

The authors' answers to the second reviewer's comments:

1. The part 4 (Seismic effects of slow strain waves. . .) looks as a brief description of the previous papers of the same authors. I suggest to add a reference to the authors' paper published in Journal of Seismology, most of arising questions were clarified there.

We complemented the 2nd paragraph of section 4 by a brief explanation and the reference to the authors' paper: Trofimenko, S.V., Bykov, V.G., and Merkulova, T.V.: Space-time model for migration of weak earthquakes along the northern boundary of the Amurian microplate, Journal of Seismology, 2016a. doi:10.1007/s10950-016-9600-x .

The added sentence, highlighted in blue, is put in the text on lines 177, 178 and 179 of the revised manuscript.

2. The method used by authors to estimate the rate of earthquake epicenter migrations is not suitable for estimation of the values of displacements per day, it is far beyond the approximation accuracy. So, I suppose that authors should use values in km/year, not km/day.

Taking into consideration that in the overview section of the manuscript all the velocities are given in km/yr, we reduced our calculation results to the same dimension and rounded these to integer quantities. The changes are highlighted in blue, see lines 217, 218, 223 and 224 of our revised manuscript.

3. It is not clear from the article text, how the graphs in Fig.5 were obtained from the data shown in Fig.3? Why the line in Fig.3a is called “sinusoid” while the line in Fig.3c is treated as “appreciably different from a sinusoid”?

Following the procedure of the initial GPS time series approximation, processing of the measurement results is performed based on the standard technique for the calculation of pseudo-distances and phase measurements for each observation day (Altamimi, Z., Collilieux, X., Métivier, L., 2011. ITRF2008: an improved solution of the international terrestrial reference frame. *Journal of Geodesy*, 85, 457-473). The annual and semiannual variations are taken into account by the following series: $x(t)=x_0+bt+a\sin(\omega t + \varphi)+\dots$ (Serpelloni et al., 2013).

Because the graph shown in Fig. 3a is plotted in accordance with the above procedure, we decided to verify the extent to which it corresponds with reality. It is shown in Trofimenko et al. (2016b) that real annual site displacement curves for various regions of the world differ considerably from sinusoid and are better described by a nonlinear wave – a breather. In order to explain these dissimilarities, we added two references with brief explanations. The changes in the text are highlighted in blue and put in the text on lines 241, 249, 250, 254, 275, 455-458 and 476-481 of the revised manuscript.

4. The “pendulum” model suggested in the paper looks very artificial. It is unclear from the text, if the authors made the calculation for such a “pendulum” oscillations of tectonic blocks or just took one of the solutions of the sine-Gordon equation. If the calculations were made – what parameters were used?

One of the objectives of the study was to find the adequate mathematical model of the vertical oscillatory movements of crustal blocks with the extraordinary trajectory pattern. In this case, the analytical solution of the sine-Gordon equation in the shape of a breather is of fundamental significance in terms of a qualitative description of recorded elementary characteristics of crustal blocks (the shape of displacement, the profile of displacement rate).

In our case, the sine-Gordon equation applied for the interacting blocks is actually postulated, but the physical interpretation of the summands of the equation is given. The validity of application of the sine-Gordon equation to the chain of blocks is proven by the fact that the implications from this equation are consistent with the results of in-situ experiment, i.e., equation (1) is the generalization of experimental data (a remarkable coincidence of solution (2) with the observation results shown in Fig. 5).

We have shown here that the sine-Gordon equation (the mathematical model, of coupled blocks-pendulums) is *an appropriate tool* for describing the shape of recorded displacements.

We express our sincere thanks to the anonymous reviewer for his careful reading the manuscript of this our paper as well as for familiarizing with our previous papers on the subject and making constructive comments in order to improve the quality of the manuscript.