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Interactive comment on "Numerical simulations of tsunami generated by underwater volcanic explosions at Karymskoye lake (Kamchatka, Russia) and Kolumbo volcano (Aegean Sea, Greece)" by M. Ulvrová et al.

Anonymous Referee #2

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Overall this paper is challenging to understand tsunami generation and propagation processes and related hazards caused by underwater explosions at Karymskoye lake and Kolumbo submarine volcano. Wave characteristics are important to constrain source conditions and hazards, but they are only recorded geologically, or at gages along shorelines near active submarine or littoral volcanoes. Numerical simulation is therefore useful to constrain them; however, it has been examined only for limited examples because of difficulties to determine the initial explosion processes. This work is valuable in the field of tsunami sciences, volcanology and natural hazards. I would

judge the paper publishable after some modifications. Comments and suggestions are listed below.

Major issues:

In section 3, authors stated that the Manning coefficient n is set to be 0.025. However, it seems that this value was not used in the following applications. In application to Karymskoye lake, n = 0.02 and 0 were used. In application to Kolumbo volcano, the Manning coefficient is not mentioned at all, although only one friction condition (no bottom friction) is shown in the caption of Fig. 7. Was n = 0.025 examined in application to Kolumbo volcano? How was the effect of the Manning coefficient in this case? The values of Manning coefficient and its effect should be described.

Moreover, in application to Karymskoye lake, authors concluded that the numerical results without bottom friction explained observations better than the results with n = 0.02. However, RMS errors in Fig. 4 show that the results using n = 0.02 explain better than no friction for initial wave height > 50 m. Why were the results using the Manning coefficient rejected? I think that zero bottom friction may be unrealistic in natural system. That's why the results from different friction conditions should be evaluated in application to Kolumbo volcano as well.

In section 5.1 and Fig. 5, there are some locations that have large discrepancies between observation and simulation results. Why did these mismatches occur? I suggest that locations where runup was measured should be indicated in Fig. 5, because it is difficult to find which locations (in Fig. 1 and 3) had mismatches.

Other minor suggestions are the followings:

6408-L16: I recommend that released energy from larger volcanic explosions should be compared as described in Sato and Taniguchi (1997) rather than the Tohoku earthquake.

6409-L19-21: It seems that the model with n = 0.02 explains observation better than

the model with no bottom friction for $\eta 0 > 50$ m, as written above.

6410-L13-16: Was the water rim height calculated using Eq. 3? This should be stated clearly here.

6411-L23: Figs. 9 and 10 might be Figs. 9 and 11.

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