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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Numerical modeling of the 2013 meteorite entry in Chebarkul Lake, Russia" *by* Andrey Kozelkov et al.

Andrey Kozelkov et al.

aakurkin@gmail.com

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The authors would like to thank the reviewer for their thoughtful and useful comments on our paper. Below we outline how we could address specific points raised by the reviewer in a revised manuscript.

1. It is mentioned (last line, page 3) that both air and water are considered to be incompressible. Although incompressibility is a good assumption for water, air is a compressible fluid. How is the approximation of incompressibility for air justified? Is this assumption valid for the present application? Some comments on the magnitude of the Mach number might be appropriate.

The incompressibility / compressibility of air will mainly influence the rate of change of the wave parameters in the source during the collapse of the cavity. Qualitatively, the

process will be described correctly, but the quantitative difference can be significant. As rightly noted by the reviewer, this is directly related to the Mach number, determined by the characteristic velocity of the meteorite, which in our calculations is 156 m / s (line 14, page 3), which is approximately 0.5 Mach number. It is known that the compressibility of the medium must be taken into account when the Mach number exceeds 0.3. In our case, the Mach number is slightly higher than the recommended value to take into account the compressibility, and therefore, in the first stage of calculations, the compressibility can be neglected. Of course, this will introduce a certain error in the collapse of the cavern, but it is estimated that it will not exceed 15-20% for speed and pressure. That is why we did not take into account the compressibility of air in this study. After the wave emerged from the source, the effect of compressibility on the process of its propagation is negligible. We introduce this comment in text of revised paper.

2. In figure 4, the time instant t=0.2 sec is presented before the time instant t=0.1 sec.

Will be corrected in revised text.

3. In the case of the ice-covered lake, the authors use the condition 'rigid wall' to account for the ice on the surface. The ice plate has a thickness of the order of magnitude of 1m but extends for hundreds of meters. It can therefore be assigned the attributes of a slender plate. Is the 'rigid wall' approximation justified under these conditions? Flexural waves generated by the impact, propagating as hydroelastic waves, might be significant for the phenomena at the vicinity of the entry area. Several models and methods of solution for hydroelastic interactions have been proposed in the literature. It might be interesting to comment on the applicability of these models, in conjunction to the simulation strategy employed by the authors, for future studies.

To compute combine equations for water and ice requires more computer time. Taking into account that we have no enough observation data concerning this event we decided to simplify the problem and use the "rigid wall" approximation. For future studies,

of course, the “ice” block of hydroelastic equations with adequate breaking conditions should be added in the model.

4. Some minor typos:

Page 9, line 21 ‘counti8ng’

Corrected

Page 2, line 15 ‘Also is made the estimation of the wave heights, the description of the stages of the meteorite collapse and as well as the generation of waves emanating from the source’. This sentence should be rephrased, as it is difficult to follow.

We deleted this sentence.

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