

This is yet another article about the atmospheric waves and subsequent ocean waves observed after the explosive eruption of the Hunga Tonga-Hunga Ha'apai volcano. Overall, it is an interesting study with detailed analyses of all available observations (i.e., following the open science policies) during and after the volcano explosion. However, the authors tend to oversell their results: grand statements (e.g., title), movies not providing any information used in the article, lack of reference to other studies related to their results, renaming physical processes already fully documented in the literature (e.g., Lamb waves), etc. Consequently, I recommend major revisions following my comments below.

#### Major comments:

1. I appreciate the efforts made by the authors to find names for physical processes not named before. However, I have several objections to the way this naming was done in the article. First, Lamb waves are already defined and documented and their name should be kept. The authors cannot single handily decide to rename this process named after Lamb research in 1911. Second, “air-shock” seems to me a poor choice as directly invoking (at least to me) “air-shock absorbers” and not any physical process. I propose “sonic boom” related atmospheric waves (which is based on the physical process occurring and heard during the explosion). I am not imposing this name as I am sure some objections can be easily voiced against it. However, I would like to engage the authors to rethink more deeply of the names they give. Third, these “sonic boom” related atmospheric waves (or whatever other name the authors might chose) include, at least, both Lamb waves (barotropic process) and Perkari waves (3D internal waves) and maybe others not known to me. Fourth, once a name is decided, the authors must stick with it. Is it “air shock”, “air-shock”, or “atmospheric” tsunami? Finally, there is already a name for tsunamis driven by atmospheric forcing: “Meteotsunamis”. So why using another name? I suppose one argument is that it is not a “Meteo” event ... But, in this case, should the “meteorological ground stations” be also renamed as they measured the mean-sea level pressure from the Lamb and Perkari waves? I propose to keep meteotsunamis with two categories: “weather” and “sonic boom” related events. This approach keeps the historical naming used for more than 20 years in the scientific community and distinguishes between the atmospheric sources (as generally done in the tsunami community that distinguishes the tsunami sources: earthquake, volcano, landslide, asteroid, etc.). More on the naming, why using “Tonga-Hunga-Ha’apai” when the entire scientific community as well as the site the authors refer to (<https://volcano.si.edu>; line 54 of the article) name the volcano: Hunga Tonga-Hunga Ha'apai. I have no problem to simplify the name to “Tonga event” (and not Tonga-Event; line 483) in the rest of the article but the first mention of the volcano should correspond to the accepted name (to my understanding, the full provided name is not controversial).
2. Title: I personally do not see the “lessons” learnt from this event in this specific article. The fact that warning systems should include “sonic boom” related (or whatever name you wish to give them) meteotsunamis is already published (<https://doi.org/10.1175/BAMS-D-22-0164.1>; <https://doi.org/10.1002/essoar.10511565.1>). Interestingly, the inference of the authors (made with the analysis of the Tonga event observations) that such meteotsunamis (free waves + force waves) can reach 10 m has been demonstrated with numerical models. This leads to my third point.

3. The authors fail to cite a vast majority of the already published literature. I understand that their article is not a review of all the work already done but some “important” references (in the sense of what was already found similar/different to their study) seemed to have been ignored. I encourage the authors to revise their literature review as something cannot be presented as a new result/conclusion if it already has been published. For example, I think the authors should get familiar with the work of Okal and Synolakis (2016) which is nicely related to their free wave analysis and might (or not, I encourage the authors to look at it in depth) be related to the behavior in the Caribbean. Amplification of the meteotsunami waves nearshore depending on the “amplification factor” of given harbors or bays is also a well-known and well-documented characteristic (please review the meteotsunami literature), this might be another reason for the Caribbean response (again to be proven or rejected by the authors).
4. Theories and methodologies presented in the article are what I would call “classic” and, in my opinion, the article tries too hard to sell facts that are already partially known from other studies (on the Tonga event itself but also from the tsunami and meteotsunami communities). Putting together all the pieces of a puzzle is good enough achievement for a scientific article, no need to amplify the novelty and the reach of the results. For example, (1) the additional movies do not add any value to the article, (2) the lengthy description of the Garrett (1976) theory does not particularly add to the analyses (reference and brief summary should be enough), (3) the title is “sexy” but not really related to the true findings of the study, etc. However, last figure summarizing all the ocean processes is, for me, the main achievement of the article (finalized puzzle) and could be even improved by better representing the synchronization of the events (e.g., free wave occurs after the forced wave) and being more specific with the shallow water processes (e.g., shoaling, harbor resonance, etc.) which play a really significant role as documented for weather related meteotsunami events.