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Interactive comment on "The VOLNA-OP2 Tsunami Code (Version 1.0)" *by* Istvan Z. Reguly et al.

Anonymous Referee #1

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This paper present the development of a new generalized hardware version of the Volna tsunami code. The Volna code is one of the recognised Finite Volume codes in the tsunami community capable of simulation tsunami propagation and inundation over dry land. In this paper, the authors have developed a new version of the code, this time the code is embedded into a library (OP2) that allow the model to be compiled and run in different hardware configurations, including single CPU, multiple CPU's with Message Passing Interface, or Graphical Processing Unit (GPU) among others. The author's claim that is the first of its kind of such general interface tsunami models. To my knowledge also, there exists no other comparable tsunami models that are general in this sense. Therefore, the present study may possibly provide a good addition to the present tsunami literature and set of models. However, a set of amendments should

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Discussion paper

be undertaken before publication can be considered.

From my reading of the manuscript, the following main points are found:

This is presumably the first model implementation of its kind, and I find that the implementation should be of interest for the tsunami community, as well as other scientific communities with interest of solving shallow water wave equations or related problems without in depth knowledge of different types of hardware architecture. This part is highly regarded.

The study of the speedup on different types of hardware are also new, and the findings are interesting in their own right. However, if possible, I encourage the authors to see if it is possible to compare the model speedup also with other models (such as HySEA) for inter model comparison.

The validation of the model is entirely missing. I know that the previous VOLNA codes have been benchmarked towards NTHMP tests previously, but this is a new implementation. While one may expect a similar accuracy for this code as well, validation needs to be demonstrated. To emphasise this, the novelty of this paper actually hinges on some kind of proof; i.e. that the model can produce results consistent with previous versions. Moreover, no explicit tsunami results are shown, only results showing the speedup. As a minimum, some results showing that the tsunami code gives a reasonable output needs to be included. I would propose that the authors include one or two of the standard tsunami inundation benchmark tests. I'm sure the authors have some such tests available.

The text and reference list is a bit imbalanced with respect to the authors own work. It would be beneficial if some more external references are added, reference to external work is moved upfront, or alternatively, discussion of the authors own work that are not strictly relevant for this paper are omitted (some parts seems not strictly necessary, see below). In the line-by-line review below some examples are listed. The references in the related work section should be moved upfront (section 2 seems unnecessary).

Some edits to the text and references are needed. See the line by line comments below.

Line-by-line comments:

Page 1, line 14: More references to external work should preferably be placed up front (e.g. here). It makes sense to pay attention to the general literature first, and use this to put the authors own work into a general context thereafter.

Page 1, line 15: The statement "there are only a handful of codes that are suitable for integration into a workflow" is unsubstantiated, please remove.

Page 2, line 1: The science perspective is missing here, but is obvious, for instance the need for running sensitivity analysis (such as varibable slip or uncertainty assessments, e.g. Goda et al., 2014), probabilistic tsunami hazard assessments (e.g. Geist and Parsons, 2006; Davies et al., 2018; Grezio et al., 2017), or for more efficient and informed tsunami early warning (e.g. Oishi et al., 2015, Castro et al., 2015). I think it would strengthen the paper to mention and discuss such examples.

Page 2, lines 19-28: Reading this paragraph, you get the impression that the Volna code is unique with respect to workflow integration, which is not the case (see comment to Page 1, line 15). There are probably more than 10 codes worldwide that can do much of the same analysis. Granted, the new development presented in this paper provide new opportunities wrt hardware independence. This should be the main message.

Page 3: References and discussions in Related Work section should preferably be moved upfront.

Page 3, line 10: Unsubstantiated statement: "Since there is no consensus as to their advantage...". What do the authors mean here? Clarify, or remove statement. Simply, Boussinesq models are needed wherever tsunami dispersion is needed (see e.g. Glimsdal et al., 2013), otherwise the shallow water approximation is sufficient.

Page 3, line 17: The authors should provide a literature search here and add more references, as GPU implementation of shallow water is under rapid development. As a minimum, the authors needs to add reference to the GPU SWE code by Brodtkorp et al. (2010) and the Boussinesq GPU code Celeris (Tavakkol and Lynett, 2017).

Page 4: I could not find anywhere from the discussion whether related OP2 applications have been performed for other (similar) applications of hyperbolic equation. If such implementations exists, it would be of interest to discuss their performance.

Page 5, line 9: Remove "very".

Page 5, lines 7-12: Treatments of shocks and breaking waves are probably the main reason FV are used, so this needs explicit mentioning.

Page 5, line 14: Replace "megatsunami" with either "large tsunami" or "transoceanic tsunami".

Page 5, line 15: Again, we refer to Glimsdal et al. (2013). Because frequency dispersion is a time dependent property, important of dispersion increases with time for a given initial condition, so it is not sufficient to refer to dispersion as weak just based on the properties at a given snapshot. The discussion here seems to merge the effect of dispersion on deep water waves and inundation, which are very different. Either the authors needs to clarify better, however, it would probably be better to omit this discussion here, and rather state that the present implementation is based on the non-linear shallow water model (you do not have to justify that dispersion is not included).

Page 6, first paragraph: This repeated reference to applications of the code seems awkward as it is not needed in this context, beside, this is already discussed in the introduction.

Page 9, lines 14-15: See previous comment.

Page 14, lines 29-31: I cant see that this is more relevant than other and more general applications such as PTHA and tsunami early warning. As said, a more general discussion with references from a broader literature is needed.

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