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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Tsunami run-up estimation based on a hybrid numerical flume and a parameterization of real topobathymetric profiles" *by* Íñigo Aniel-Quiroga et al.

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Anonymous Referee #2 Received and published: 29 January 2018 General comments: This paper presents a method for quickly assessing tsunami runup for different tsunami wave shapes and bathymetries. The method is built on a hybrid approach, where the Non-Linear Shallow water model COMCOT is used for the deep-water propagation and a RANS models is used for the near shore processes. The results from this hybrid model, then enters an interpolation model, which can be used to assess run-up. The approach is novel and innovative, and I especially like adding a fast interpolation model. I have however, a few major concerns. The actual implementation of the hy-

brid model is not well described and I think the coupling between the two models can pose big problems. Further, the hybrid model is not validated on its own in a controlled environment.

REPLY: We thank Reviewer 2 for their careful review and for the positive comments regarding the developed interpolation modeling. We have considered their modifications and suggestions, what has increased its overall significance. Changes included following Reviewer 2 comments are in yellow color in the attached revised version. We respond in detail to their comments below. We especially thank their comments towards explaining more deeply the hybrid model implementation. Initially, we included some more details regarding this specific part of the work, that concentrated our efforts, like a specific validation. However, in the end, we decided to skip some of this data to keep the focus of the paper on the run-up calculation. Following Reviewer 2 comments we have reconsidered this aspect, and we have added more information to make it easier all the process to follow. This incorporation provides a substantially improved approach to the hybrid model.

Major comments: 1) Parts of implementation and usage of the hybrid model is not well described. a. What are typical grid sizes in the RANS model? Are these sufficient to handle the processes, which NLSW models cannot handle? Like wave breaking.

REPLY: The design of the domain of the RANs model (IH2VOF) followed 2 criteria.

First, there is a limitation of the model that do not allow grid more than 5499 cells in X dimension ($nx<5499$), and the ratio between dimensions must be constant ($r=\Delta x/\Delta z=constant$). In this case, and due to the different scale necessities on each dimension, the applied ratio was $r=5/1$. Therefore, the maximum length covered with RANS model was $Lx=nx*\Delta x$.

And second, to control and avoid false wave breaking, the Z dimension of the RANS model grid must be discretized in a minimum number of cells, satisfying the expression:

$\Delta z=[(K*H\_COMCOT)/(10*0.05)]*0.05$

Where K is a safety margin of the model=1.08 and Δz is defined in the range ($0.05 < \Delta z < 1$).

To sum up, in this sense, the model itself limits the length Lx and the grid size Δx. First, Δz is calculated with Hcomcot, then Δx is obtained $\Delta x = \Delta z * r$, and finally, $Lx = nx * \Delta x$. This approach results in values of Lx, depending on the Tsunami wave height of the COMCOT model. For Hcomcot=0.5, then Lx=1400 m. For Hcomcot =4.5m, then Lx=12400m.

Following this process, the grid size is enough to handle the processes that the LSWE model cannot.

b. What are typical Lx lengths?

REPLY: $Lx=nx*\Delta x$, and the value of Δx depends on Hcomcot. In the same way.as it has been described, the RANS model limits in terms of number of cells drive the generation of the grid, and also the value of Lx. ., that is calculated following two rules: "Maximize the area" means to use all the available cells to cover the maximum length. And, since the number of cells is limited, we did not want to lose cells onshore far away from the flooded area. Thus, we pre-calculate a rough value of the run-up using COMCOT and approach a first value of the run-up. Taking this into account, typical values of the RANS model length are from 500 to 25000 m.

c) How are the boundary conditions for the turbulence mode?

REPLY: In the case of turbulent flows, IH2VOF numerical model uses smooth wall, log-law distribution for the mean tangential velocity. This aspect has been included in page 4 of the new version of the manuscript.

d) It is unclear how xcut is determined. In the paper two criteria is given. One is to maximize the area of the IH2VOF domain and the other is to ensure that flooding does not exceed the inland end. Regarding the first criteria, letting IH2VOF cover

the entire numerical flume would achieve that, but that is clearly not what is being done. Regarding the latter, I cannot see how the end position of the IH2VOF domain influences the position of xcut.

REPLY: AS it has been described, the limit of the RANS model in terms of number of cells drives the generation of the grid. The position where the models are coupled, xcut, is given then by the value of Lx. As commented previously, Lx is set trying to maximize the cells that are effectively used in the simulation. This important aspect of the hybrid model was not included in the first submitted version of the manuscript, but following reviewer 2 comments we have added it. Regarding specifically the first criteria, since the flume is non-scaled, it was not possible to cover the whole domain with RANS model due to computational restrictions, i.e., we cannot calculate the generation-propagation and inundation areas without assuming other limitations of scale. Moreover, offshore generation and propagation is well solved by LSWE model, where non-linearities are not relevant in the calculation.

e) One of the advantages of the hybrid model is that the RANS model can handle processes that the simpler COMCOT model cannot. One of such processes is the wave splitting into an undular bore, which can happen when the wave travels long in shallow water and this has been witnessed in many real life tsunamis. To be able to capture this effect xcut needs to be positioned sufficiently off shore. How is this ensured?

REPLY: Due to the exposed characteristics of the model it is not possible to extend the RANS model grid seawards more than what this limitation allows. Nevertheless, to avoid loss of processes on coupling, several coupling tests were performed. These tests were conducted reproducing the flume on scale, so a target simulation was performed using only the IH2VOF model. Lately, simulations of coupling between IH2VOF + IH2VOF and COMCOT + IH2VOF were performed and compared to single IH2VOF simulations. In both cases, IH2VOF cases coupling methods reproduce evolution of single IH2VOF simulations adequately. In the attached figure one of the conducted tests is given. In this figure, it can be observed that, when the last part of the flume is calculated by means of IH2VOF model, the comparison between the different combinations of models is accurate in terms of run-up.

AS a consequence, the part of the processes that are not covered by LSWE model is incorporated in the last part of the flume, where the RANS model works.

f) To avoid reflection, the numerical flume of COMCOT is altered, to properly access the incoming wave. I have a problem with this approach. In reality, especially in cases with steep slopes, there will be significant reflection from the beach which will and should affect the incoming wave. This effect cannot be captured with the current approach. Further it is also unclear what would happen when the reflected wave from the IH2VOF domain meets the hard boundary between the two models. Will this cause additional reflections in IH2VOF domain?

REPLY: The artifice that has been applied to avoid reflection effects on the input wave focus precisely on avoiding that the reflected wave affects the tsunami wave between borders incident one. This unaltered wave is used to force the IH2VOF domain, in which simulation reflection on the beach is of course observed and considered for the runup calculations. Figure 6 of the manuscript tries to explain how this artifice corrects the possible reflection effect. Fig 6a shows the reflected wave and Fig 6b shows how the artifice works and the reflective effect is almost imperceptible. This figure quote has been improved in order to make clear the artifice intention.

g) The calculations of L does not match Fig. 4. E.g. Li is given as Li=1/50 tan(β0). This will result in a very low Li. Further $Loff$ is given as Lf +x2, but according to Fig. 4 it should be Loff = Lf +x2 +x1. Finally, there is no need for δX in the calculations of Lf as it is present both in the denominator and the numerator.

REPLY: Regarding the calculation of Li, the expression in the manuscript is incorrect, as describe by the reviewer. It should be: L_i=50/(tanβ_0) Regarding Calculation of Loff, again, we appreciate reviewer correction. It should be: L_off=L_f+x_2+x_1

Finally, regarding the Lf calculation, we used “ceiling brackets” which is the largest integer less than or equal to X, commonly used in mathematics and computer science. Then, using Δx both in the denominator and numerator, allows us rounding values to the order of Δx.

h) How is the run-up height determined in the IH2VOF model? In a VOF computation,the interface can span across several cells.

REPLY: The numerical model calculates for the last flooded cell the ratio that is actually flooded and provides the run-up in accordance. Indeed, due to the fact that the RANS model does not calculate directly the free surface but it tracks the changes in cell density, there are mainly 2 ways to tackle this calculation: Assessing the iso-surface, determining the contour where VOF function is 0.5 Calculating the water contained, accumulated in a column of the grid. In order to avoid diffusion, the quantity of water is added. In this case, after several “trial and error” tests, the second method has been applied, although in the end the difference were not serious. None of the methods is perfect but both of them provided a good approach. This aspect is included in page 4 of the new version of the manuscript.

Responses to comments regarding models coupling have been included in the paper section regarding the characteristics of the numerical flume.

2) The first validation case is performed by comparing the interpolation model to the hybrid model. This is an important and satisfying validation case, but I am lacking validation of the actual hybrid model. How will the model perform using the approach outlined in the paper e.g. in cases with both breaking and non-breaking waves running up a constant slope.

REPLY: Both, COMCOT and IH2VOF models are models that have already been successfully validated in the past. The validation of the numerical flume and the coupling of the models was made by comparing its results with those conducted by Synolakis (1987) and Baldock (2009). The scenarios of these experiments were calculated using

COMCOT, IH2VOF, COMCOT+IH2VOF (the complete numerical flume) and the results were compared to the results of the physical experiments. The attached figure shows the run-up obtained in this comparison and how the results of both series of experiments fit adequately with the numerical flume results. This validation allowed us to continue with the database elaboration, and the database itself was then validated by comparing the results with both numerical models and field work data.

IH2VOF model is, in the hybrid model, the responsible of incorporating non-linearities and breaking effects. In this sense, apart from the validation as part of the numerical flume, it has also been validated and applied in many studies e.g. The shown figure, together with an explanation of the validation has been added to the manuscript, at the end of section 2 (pages 13).

3) The performance of the iterative solver is compared to the Synolakis formula. I do not believe this is a fair comparison, as it is created for the run-up of a solitary wave, which as highlighted by the author does represent a real geophysical tsunami event. A more relevant comparison could be to the analytical model proposed by Madsen and Schäeffer (2010), as also highlighted by the authors in the introduction. (The Synolakis formula require a proper reference).

REPLY: We appreciate this comment. It was also highlighted by Reviewer 1. We have added the results of applying Madsen and Shaffer (2010) to the table where the result of applying the run-up database interpolation are given. AS explained to Reviewer 1 we think that this complementary validation definitely improves the significance of the paper. In addition a proper reference to Synolakis formula has been included. We have decided to leave the Synolakis formula application results as well due to its common use in literature.

4) The periods are estimated as the time between the first two zero crossings for positive heights. Does this mean that the model cannot differentiate between tsunamis having only positive surface displacement and e.g. a leading depression?

REPLY: This criterion (period=time between 2 positive zeros), is the one that IHTRUST, the interpolation tool, and system, uses to automatically calculate the period of a time series. However, the system itself allows to manually edit the period: It shows the time series and the part of it that is going to be considered in the interpolation. If another period, like in the case of a leading depression is to be used, it can be manually corrected. However, several tests were performed, and we did not found serious differences on simulations led by crest or trough regarding the run-up results. Therefore, the tool was scripted following the explained criteria, although as explained it allows its modification.

5) With this approach of estimating period and wave height, I see a potential problem in the case where the leading wave is not the largest. Can you please elaborate on this?

REPLY: This is a real limitation that concerned us during the process of scripting the IHTRUST. It was the main reason why we added the “manual way” to include the height and period. This allows using an ad hoc input data on the interpolation process. Although obviously it cannot take into account several waves of the tsunami, it assures to use the proper part of the time series as input. We have reinforced the IHTRUST explanation on page 20

6) One of the main points off this work is to be able to quickly access tsunami runup without doing long complicated simulations. Therefore for this work to fulfill this, it would be beneficial is the TRD database was made available to engineers. Are there any plans regarding this? REPLY: So far, we are still making the most out of the tsunami run-up database. We are specifically working on a better definition of the influence of each parameter on the final value of the run-up. Once all the analyses are finished, we are planning to release the data, by itself or on a new issue.

Smaller comments: Thanks a lot for this smaller comments. We have included them in the new version of the manuscript. Page 1, line 8: It is stated that Run-up is accurately calculated by means of numerical models. This is a rather strong statement. I would prefer it rewritten as: can be accurately calculated

Page 1, line 14. The models here, and several other places are referred to as schemes. They are however not schemes, but models. Please change the formulations. REPLY: We have changed it.

Fig. 5. It is stated that only the COMCOT model is used with the altered domain. If this is indeed the case, then please remove the IH2VOF domain from the figure, as it is causing confusion.

Fig. 6 units and legends are missing on the colorbars. Please add these.

Page 14, line 2. It is stated that d2 −d1 was always shorter than 2200 m and x1 shorter than 210 km. How does this correspond with table A1 where d2 −d1 is always larger than 2200 m? It should say larger.

Fig 8. Many of the axis are missing units. Please add these.

Fig 10. Please add missing units to the axis.

Page 19, line 6. It is stated that T corresponds to the time between the first two zero crossings for positive heights. However in Fig 11. It looks as if the second zero crossing has not occurred within the shaded area? The IHTRUST tool calculates the best fitting to this criteria. In this case the red shaded area approximates the period. The time series is built with discrete points and the system catches the closest one.

Page 24. It is described how low values of tan(β2) gives lower run-up height due to friction. From Fig 13, it can however also be seen that the run-up heights reduce with large values of tan(β2). Please elaborate on this.

It is unclear exactly what Fig 15 is describing. Please rewrite the description for clarity and add units to the axis. REPLY: We have modified this figure in order to make it clear that it is just a scheme of the main "regimes" that have been found. It is the figure 17 in the new version of the manuscript

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2017-445/nhess-2017-445-AC2-supplement.pdf

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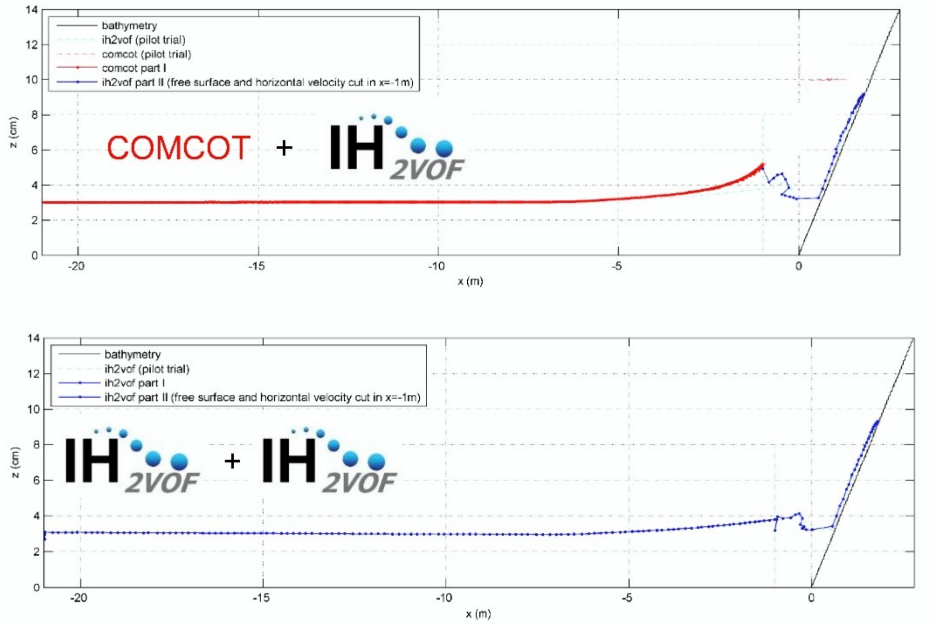
bathymetry
ih2vof (pilot trial)
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ih2vof part II (free surface and horizontal velocity cut in x=-1m)
COMCOT + IH 2VOF
z (cm)
x (m)
bathymetry
ih2vof (pilot trial)
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ih2vof part II (free surface and horizontal velocity cut in x=-1m)
IH 2VOF + IH 2VOF

Fig. 1.

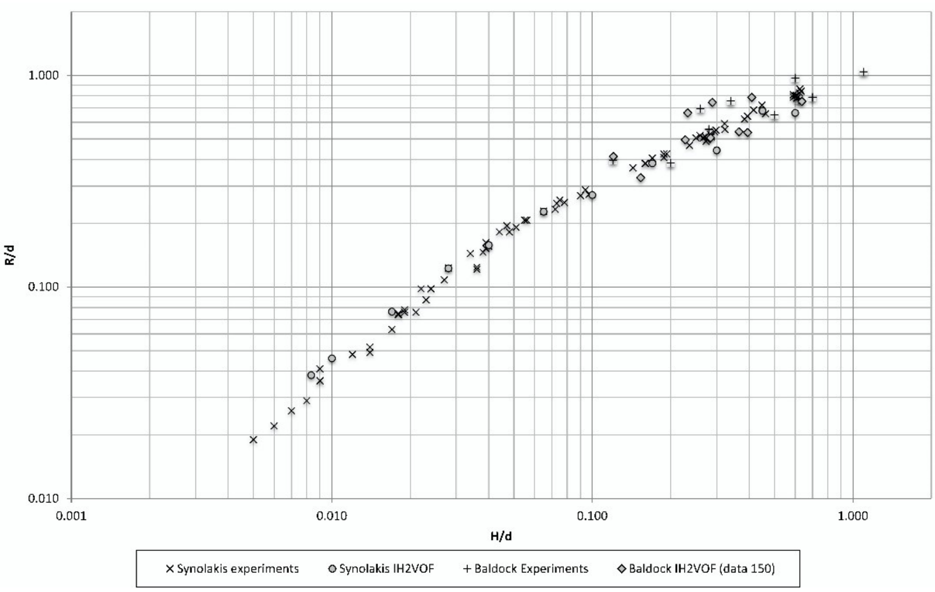
R/d
H/d
Synolakis experiments
Synolakis IH2VOF
Baldock Experiments
Baldock IH2VOF (data 150)

Fig. 2.