AGU Advances

Peer Review History of

The Chicxulub Impact Produced a Powerful Global Tsunami

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Author Response to Reviewers

Peer Review Comments on 2021AV000627

Reviewer #1

Review of the paper entitled "The Chicxulub impact produced a powerful global tsunami"

This paper provides one of the first models for the global propagation of the Chicxulub impact tsunami. It is clearly of both high scientific and more broad interest to understand how efficiently this impact tsunami could have propagated both regionally and globally. To this end, I find that the present study provides a good first pass study, but there are unfortunately several potential shortcomings that would need to be clarified and improved. There is modelling uncertainty that may lead to bias or large uncertainties, as some essential elements of the physics have not yet been investigated. This needs to be highlighted more in the paper. Hence, the discussion and conclusion need to be presented with more focus on the uncertainty, with the possibility that the wave behaved differently from a shallow water model. Moreover, several parts the analysis are not presented with sufficient detail, including supporting investigations such as mesh refinement tests, details of the models used etc. Consequently, the manuscript would need substantial revision and cannot be accepted in its present form. The main aspects of these shortcomings are briefly discussed below. In my review, I've mainly focussed on the hydrodynamic modelling aspects related to the transition phase from the hydrocode modelling and subsequent global tsunami propagation.

The first aspect relates to resolving and coupling the near field hydrocode tsunami modelling to the far-field tsunami modelling. Inspecting the hydrocode simulations, it is evident from the figures and supporting videos that the thin tsunami layer is relatively coarsely resolved. A question is to which extent the sensitivity of this mesh resolution has been investigated. The results of such mesh resolution test should be shown, at least in the supplements. As the wave front is relatively coarsely resolved, breaking and dissipation may not be fully resolved. To this end, the authors claim that wave breaking is terminated when the wave has reached the periphery of the iSALE2 simulations. I do not find this claim fully convincing, as I could not find any evidence in the paper proving this explicitly. At this point, the wave has amplitude to water depth ratios of more than unity which by itself would point to possible wave breaking. The sensitivity to this could be investigated with mesh refinement studies. Related to this, I find that SI Figure 2 is key for visualising the transition from the impact area to the far field, it should really be included and discussed in the main body of the paper.

A related issue is how this wave field information is conveyed and propagated to the far-field. While there are large wavelengths involved, the amplitudes are also large, and there are several short undulations, and the wave front can be steep. Hence, a detailed treatment of the frontal wave breaking can be of key importance for understanding how effective the global wave propagation is. The authors rightfully claim that the propagation of this tsunami was more dissipative than the 2004 IoT tsunami. However, it is possible that the modelled dissipation in this paper is still underestimated, and this can lead to different conclusions:

A related paper on the smaller Mjølnir impact shows several similarities (Glimsdal et al., 2007). This paper, despite its direct importance here is not discussed. Initial wave amplitudes (some hundreds of meters high), initial water depth (about 500 m), and wave lengths (~20-50 km) show some similarities. Although being a smaller event, the normalised wave characteristics are still similar to Chicxulub, in particular related to the key dimensionless properties (amplitude/water depth and wavelength/water depth). Glimsdal et al. provided a more detailed analysis of the evolution of the wave front and showed that it likely developed into a series of undular bores. This indicates that using a shallow water model, although being useful for providing a first pass analysis, may carry significant shortcomings that may be of major importance. The undular bores, which are solitary waves developing in the front of the wave, can splitting into a series of shorter waves that are more prone to dissipation than a single shock front in a shallow water system. The non-linearity is important in this fission process, and could transfer energy from the long wave component into the shorter undular bores. Modelling Mjølnir, this resulted in highly altered wave system compared to using a shallow water model. In the present paper, a sufficiently highly resolved simulation of the phase where such undular bores can develop are not carried out (this may be done with a dispersive wave model, several are available open source), and it is hence hard to tell to which degree the shallow water assumption is adequate. If this undular bore fission process was present in the wave induced by Chicxulub impact was present, it is likely that the near field tsunami could have been locally higher, but as the undular bores are more unstable, they could also break more rapidly and eventually lead to higher coastal dissipation rates, implying that the onshore impact of the tsunami was dented. A discussion of this aspect (that could be of first order importance) is lacking here, although the van Dorn effect (discussed in the paper) would contribute to this dissipation. Hence, I consider the discussion of the validity of the shallow water approximation too rudimentary, and the same follows for the discussion part in section 5. The uncertainties related to not investigating more complex hydrodynamics therefore needs more attention. The very real possibility that the far-field propagation mechanisms for such a violent impact are different from an earthquake tsunami deserves to be considered in the discussions, including possible effects of using more sophisticated dispersive tsunami propagation models.

Despite the modelling uncertainties outlined above, the shallow water model still shed some light on the global propagation. However, the grid resolution used in the modelling is extremely coarse (1/10 degree, e.g. >10 km). The authors claim that mesh resolutions studies are carried out, but they do not show explicit comparison of the same wave field using different mesh resolution. This should be demonstrated, at least in the supplementary materials. The ~10km resolution might be appropriate (but still be coarse) for the 2004 IoT tsunami where the amplitude in the oceanic propagation is small and the wave behaves linearly, but this is not the case in the "vicinity" (i.e. the region surrounding the area modelled by the hydrocode) of the impact for Chicxulub where non-linearities are still pronounced. Despite the grid resolutions tests carried out, it is unclear whether this modelling would be capable of properly resolving wave breaking and dissipation at this resolution. It is likely necessary to model the wave propagation at much higher resolution in this near-field area to investigate mesh effects on wave breaking and more closely. This is also sensitive to the choice water depth, which is an uncertain parameter.

The paper also lacks a more general discussion related to hydrodynamics asteroid impacts, and several key paper in the literature are not cited. Despite that several of them are dated some time back (it is very likely that there are newer references I am not aware of) and deal with deep ocean impacts, a more general introduction to the topic showing the breadth of examples are lacking. These papers also clearly show that the propagation of most such impact tsunamis are modelled using dispersive wave models. Given the importance of frequency dispersion in almost all previous assessments, it is surprising that this is not discussed in the present paper. Examples of relevant references are given in the reference list below:

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References - examples where offshore tsunami propagation is discussed:

Asphaug, E., Korycansky, D., and Ward, S. (2003). Exploring ocean waves from asteroid impacts. EOS Transactions 84, 35

Glimsdal, S., Pedersen, G. K., Langtangen, H. P., Shuvalov, V., and Dypvik, H. (2007). Tsunami generation and propagation from the Mjølnir asteroid impact. Meteoritics & Planetary Science, 42(9), 1473-1493.

Korycansky, D. G., and Lynett, P. J. (2005). Offshore breaking of impact tsunami: The Van Dorn effect revisited. Geophysical Research Letters, 32(10).

Ward, S. N., and Asphaug, E. (2000). Asteroid impact tsunami: a probabilistic hazard assessment. Icarus, 145(1), 64-78.

Ward, S. N., and Asphaug, E. (2002). Impact tsunami-Eltanin. Deep Sea Research Part II: Topical Studies in Oceanography, 49(6), 1073-1079.

Weiss, R., Wünnemann, K., and Bahlburg, H. (2006). Numerical modelling of generation, propagation and run-up of tsunamis caused by oceanic impacts: model strategy and technical solutions. Geophysical Journal International, 167(1), 77-88.

Weiss, R., and Wünnemann, K. (2007). Large waves caused by oceanic impacts of meteorites. In Tsunami and Nonlinear Waves (pp. 237-261). Springer, Berlin, Heidelberg.

Wünnemann, K., Collins, G. S., and Weiss, R. (2010). Impact of a cosmic body into Earth's ocean and the generation of large tsunami waves: insight from numerical modeling. Reviews of Geophysics, 48(4).

Reviewer #2

This is a very interesting paper with interesting conclusions. It is rather short for the breadth of field that is covered. In particular it appears that thermal effects at the impact are completely ignored. This may be reasonable, but it should be stated since these effects have been shown to have some significance in other studies. In particular, the work of Galen Gisler and co-authors is never referred to, even though they have made significant contributions in a rather small field. My second issue is somewhat "unfair". Given the recent Tonga event, which happened after the original deadline for this review, the effect of atmospheric pressure waves on tsunami generation has really illustrated that this is a very significant process. The importance of this paper would be significantly increased if the authors somehow managed to include some ballpark estimates of this effect in the paper. I realize that this means more work for a paper that otherwise would be close to publication, but I think the authors are perfectly positioned to write a more comprehensive and state-of-the-art paper if they did.

Editorial:

Reimann->Riemann

Reviewer #3

General Comments

For "The Chicxulub Impact Produced a Powerful Global Tsunami", the authors ran simulations in which they propagated an asteroid impact-generated tsunami around the globe. The study aims to replicate the tsunami that was produced during the Cretaceous-Paleogene (K-Pg) mass extinction event. This is a solid piece of work and I support publishing it after minor corrections.

In particular, I value the usage of drill-core samples as a validation method for flow speed predictions of the simulation. This approach not only increases confidence in the results but provides a useful link of the simulation back into the real world and adds meaning to this study. My main comment is connected to this point. A tweak in the presentation of the results would better drive home this aspect of the work (see specific comments below).

Further, the laconic descriptions added when mentioning various modeling concepts greatly improve readability and are generally well-formulated. Thank you for that.

Specific comments

It is good practice to mention run times and computing hardware in simulation-based studies. Please add this information to the manuscript.

Section 1: "...continental positions and boundaries The models do not incorporate..." Full stop missing between two sentences.

Section 2.1: "With a grid resolution of 100 m, the ocean is resolved by 10-30 cells..." The sentence is not well-formulated. I assume it alludes to ocean depth. I recommend changing it to: "With a grid resolution of 100 m, the ocean depth is resolved by 10-30 cells...".

Section 3.2: "...to be over 100-m. Along many North Atlantic coastal regions and some South America Pacific coastal regions the models show over 10-m offshore amplitudes. The simulations predict that most of the world ocean experiences maximum offshore amplitudes above 1-m, with..."

Remove dashes in three occurrences between value and unit (for example 10-m to 10 m).

Section 4.2: The reference list after the first sentence has an entry that reads "correct the inconsistency". This inconsistency should be corrected.

Figure 5: The link between flow speeds predicted by the simulation and findings in the drill core samples is a key element of this study. This figure attempts to quantifies the strength of this link. Resolved by region, it shows the fraction of the core samples that correspond to flow velocities smaller than 20 cm/s. Resolving these data as within/without the 20 cm/s flow velocity global contour region (Figure 4) would provide a simpler, more intuitive presentation and strengthen the message. In essence, the updated figure (or text) would state something along the lines of: "Outside of the 20 cm/s contour, we find that a small amount (xx%) of core samples is consistent with large flow velocities, while we find a significantly larger fraction (yy%) to be consistent with large flow velocities inside the contour".

Section 4.3 "It is well known that most impacts are oblique with 45{degree sign} impact angle being most likely." Citation missing to support this statement. I recommend (but there are other relevant sources as well): Robertson, D., Pokorný, P., Granvik, M., Wheeler, L., & Rumpf, C. (2021). Latitude variation of flux and impact angle of asteroid collisions with earth and the moon. Planetary Science Journal, 2(3), 88. https://doi.org/10.3847/PSJ/abefda

Section 5: The second paragraph discusses energy coupling between the asteroid impact and the generated tsunami. The text mentions relevant literature that discusses energy coupling for Earthquake-generated tsunamis but fails to mention relevant literature for asteroid-generated tsunamis. For example, Ward 2000 assumes ~15% as energy coupling constant. There is probably more up-to-date literature than Ward 2000 and it would be worth mentioning how the simulation results compare to that body of literature.

Ward, S. N., & Asphaug, E. (2000). Asteroid Impact Tsunami: A Probabilistic Hazard Assessment. Icarus, 145, 64-78. https://doi.org/10.1006/icar.1999.6336

Clemens Rumpf

Peer Review Comments on 2021AV000627R

[Review comments begin on the next page.]

Review of the paper entitled "The Chicxulub impact produced a powerful global tsunami"

Below a review of the first revision of the paper is given. While some of my comments were appropriately addressed, it was disappointing to see that the comments considered most important, i.e. related to the wave breaking and wave front fission (after the iSALE simulations are carried out) were not discussed nearly to the detail I expected in the main body of the paper (those related to fission of the wave front were not even mentioned). Furthermore, statements related to the appropriateness of the MOST model for resolving dispersion and undular bore appears to be stretched way beyond the validity of the model. Looking at the track changed paper the revision was indeed very light despite several major points of criticism, and it is necessary to include a much more substantial update of the main body of the paper. To this end, recall also that similar objections were raised by another reviewer in a review to an earlier version of this paper. Hence, I cannot accept the present manuscript in its present form.

A minimum requirement is a more rigorous discussion related to need for more investigations of wave breaking and dissipation outside the iSALE simulation domain. I'm surprised of the authors are reluctant discuss this in the main body of the paper, in particular since including such a discussion that illuminate better the uncertainties in the modelling would not be exhaustive (the actual investigations of wave breaking, and wave fission can be left for further studies). Hence, I see no other choice than reiterating the need to improve the points that are still not adequately addressed. In addition, some new points (related to comparing the two shallow water models) have emerged after reading the paper the second time. I apologise that I did not spot these in the first review. In this review, I first discuss the rebuttal by the authors, focussing mainly on the items that still ne, followed by a point-to-point review. Authors rebuttals are given in *italics (rebuttal)*, while my new responses are given in upright.

Rebuttal: *In response to the general comments above, we would like to (re)emphasize that our study includes the very first peer-reviewed simulation of the global propagation, and estimates of coastal impacts on a global scale, of the tsunami generated by the Chicxulub impact. All previous published studies were focused on regional impacts, mostly around the Gulf of Mexico and North America. (Ward's simulations are either not global and/or are not peer-reviewed publications.)*

Response: Some of the findings from the previous papers may not directly on Chicxulub, but the experience that might be drawn from previous modelling (e.g. Ward and others works) could still be included in discussions for this paper. On the other hand, I see that the several new references are included after the revision, which is positive and sufficient at this point.

Rebuttal: *Furthermore, to the best of our knowledge, our work represents the first time that results from a hydrocode, which is capable of representing the intense impact dynamics, have been handed off to a global propagation model.*

Response: This is acknowledged, but as there still some major modelling uncertainties that has not been investigated it is important to highlight these up-front.

Rebuttal: *The assumed initial condition in other propagation modeling work that we have seen is much too conservative, in our opinion, relative to what is seen in our hydrocode results. While the uncertainties surrounding our simulation are understandably quite large, we consider our quantitative global estimate of the Chicxulub tsunami to be a major contribution to the literature of the K-Pg boundary impact studies. The paper is intended for a fairly wide audience, not just experts in hydrodynamic modeling. Therefore we limited the discussions of model specifics to mostly general principles, without including many technical details. We have added more discussion about the* *uncertainties of our estimates in the revised manuscript and supplementary information section, as suggested by the reviewer, while we understand that these discussions may still be technically nonexhaustive.*

Response: Yes, but this a scientific paper with the main focus on the hydrodynamic modelling, and the conclusions hinges on the modelling assumptions. Hence leaving out details related to the wave propagation physics that is not investigated (and that might be important for conclusions) from the main body of the paper is not acceptable. Most readers do not read supplements. The relevance of the findings of Glimsdal et al (2007) that show a more complex wave pattern than the authors model is not even discussed. This must be fixed in the revision.

Rebuttal: *We agree with the reviewer that the impact-produced wave is still breaking at the time of the hand-over from the hydrocode to the shallow-water code. We thank the reviewer for pointing out the inconsistent description in the supplemental material. What we meant to say was that the wave is done with active plunging breaking, the process that cannot be modeled with the shallow water wave models, once the handoff occurs. However, the wave has clearly reached a virtually steady-state phase of propagation as a bore-like wave. Bore-like waves are processes that can be modeled with the nonlinear shallow water wave approximation well, as the hydrocode simulation that runs out to 1100s shows. We have corrected the description in the supplemental material accordingly. We have also moved SI Figure 2 to the main manuscript, as suggested by the reviewer.*

Response: Thanks for clarifying this item and moving figure 2 into the main body of the paper. Although bores can be modelled with the wave models used here, it is unlikely that these are adequately resolved with the 1/10 degree (order of 10 km, ~100 times larger than in the hydrocode) resolution grid. Investigations using finer grids would likely be needed. As long as this is not done, discussing the need for further investigations on this matter must be mentioned in the main body of the paper.

Rebuttal: *We thank the reviewer for the comment about resolution in the hydrocode simulations. We performed a hydrocode impact simulation at 200 m resolution and the results are in good agreement with our 100 m resolution demonstrating convergence of the solution. Unfortunately we failed to mention this in the first version of the manuscript. We have now added this to the SI section—see the new SI Figure 2 and associated discussion.*

We also wish to point out that there are important trade offs in the use of the hydrocode vs. a global propagation model. One might want to run the hydrocode out longer, so that the nonhydrostatic processes that a shallow-water code cannot handle have fully run their course. However, the assumptions of axisymmetry, constant depth, etc. made in the hydrocode have their own problems– the real world is neither axismmetric nor constant depth–meaning that in order to avoid these problematic assumptions we need to get to the global model as soon as possible. Our hydrocode simulation does not look that different at 1100 vs 850 seconds, and we perform handoffs at both 600 and 850 seconds from the hydrocode to the global model. None of the main results of the shallow-water change qualitatively, suggesting that other assumptions that we make are more important.

Response: Thank you for clarifying these two points, these seems to be adequately addressed now.

Rebuttal: *We thank the reviewer for providing insightful discussion about the dispersive effects and for providing relevant references that we included in the debates of uncertainties in the new manuscript version. We agree with the reviewer that dispersive effects may influence the wave front dynamics as described in the discussion above and illustrated by Glimsdal et al. (2007). However, we consider that additional uncertainties due to these effects would be of higher order, considering other very general* *assumptions of our simulations. Here is our rationale, which, prompted by the reviewer, is now included in the revised supplementary information section:*

Response: You say that "*we consider that additional uncertainties due to these effects would be of higher order*" but how can you tell without doing any investigations on the matter? This is unsubstantiated. Glimsdal et al concluded differently having done deeper modelling investigations. As long as this is not investigated you cannot rule out that it has substantial influence on the conclusions.

Rebuttal: *"The dispersive effects may manifest themselves in the Chicxulub tsunami propagation simulations in two ways: (1) during the long-distance propagation as different wave frequencies separate from a single front; and (2) during the evolution of the initial steep wave front into an undular bore (Glimsdal et al., 2007).*

Response: I agree with the new statement up to this point, but disagree with some of the statements below that hence needs rephrasing:

Rebuttal: *Both of these processes generally lead to the decrease of amplitudes in comparison with the classic shallow-water wave theory estimates. So the nonlinear shallow water approximation provides, in general, a conservative (upper-bound) estimate of potential tsunami amplitudes.*

Response: I understand the authors point here, but this statement is too generalized and hence not correct. Undular bores can given higher amplitudes locally. Also dispersive wave trains can give larger amplitudes. I would suggest revising this sentence.

Rebuttal: *The use of Boussinesq-type models may provide a better resolution of the undular bore feature of the turbulent wave front. However, these effects are confined to a relatively small part of the wavelength near the bore front, and therefore may have very limited effect on the general wave propagation pattern – the main goal of this study.*

Response: The undular bores formation has a potential to changes the wave propagation and not necessarily only the wave front. Moreover, they are more unstable and may possibly accelerate breaking and instability of the wave propagation, and hence make the waves more dissipative. Investigating Glimsdal et al. Figure 14, you see that the whole wave signal is affected by the undular bores, and not just the front.

Rebuttal: *Also, the results of Glimsdal et al. (2007) show that the Boussinesq model appears to overestimate the dispersive front effects in comparison with the full hydro code, which may be attributed to difference in resolution or to the inherent tendency of Boussinesq models to overestimate dispersion.*

Response: This is irrelevant, as there are also higher order Boussinesq models (e.g. FUNWAVE) that can be used to model this more accurately taking into account full non-linearity and dispersion. In any case the Boussinesq model includes much more physics than the shallow water model that cannot be used to model such phenomena (see below), and would be a better alternative at least for the intermediate propagation part.

Rebuttal: *In the case of our modeling, we expect the dispersive effects would be, at least partially, accounted for, since one of the models (MOST) includes the physical process of frequency dispersion approximated by numerical dispersion (Burwell et al., 2007).*

Response: This is misleading. MOSTs inclusion of dispersion is quite rudimentary and, in any case, not suitable for modelling and resolving undular bores. As MOST is using tuned a coarse grid resolution for mimicking dispersion it can only mimic dispersion for some special settings (if the ocean depth is constant for the linear dispersive waves), but not for the undular bores where the physics is completely different.

Rebuttal: *MOST has been benchmarked against laboratory tests with highly dispersive and highly non-linear waves for wave breaking dynamics (Titov and Synolakis, 1995) and compared with dispersive models during the long-distance tsunami propagation (Zhou et al., 2012).*

Response: The comparison in Zhou et al., 2012 concerns linear dispersive waves and not undular bores, this reference is not suitable for this example where possible undular bore is the main issue and the wave propagation is completely different. Please revise and clarify that MOST is not tailored for this.

Rebuttal: *These comparisons showed that MOST provides results closely resembling the dispersive models estimates.*

Response: No this is certainly not true, please delete the above statement for the reasons stated above!

Rebuttal: *Since MOST and MOM6 results are fairly consistent, we consider that the dispersive effect uncertainties are probably not larger than other differences between the two models.*

Response: Looking at Figure 3 it looks like there are clear differences also between these two shallow water models. As discussed below, these issues also need to be clarified further.

Rebuttal: *Other uncertainties of the simulations (such as in the details and size of the impactor, and in the paleo-bathymetry estimates), are significant and we fully expect that our error bars for quantitative assessment may be quite large. In comparison with those major uncertainties the errors due to dispersive effects are probably of much higher order and would not significantly change the main results of this study."*

Response: It might be possible that other issues cause even larger uncertainties, I do not disagree with this. But the authors still need to discuss the uncertainties concerning the scope of their article, here, long wave modelling. And, as long as the issue of wave breaking and dispersion is not investigated, the last statement remains unsubstantiated. A discussion in the main body of the paper is still necessary.

Rebuttal: *We agree that the grid spacing we chose for our global simulation may be a compromise between accuracy and being able to perform full global simulations. The global test simulations with 1/5 degree grid spacing were performed with MOM6 and provided a very similar outcome with respect to global energetics (shown in SI Figure 4) and with respect to maps of global propagation (shown in SI Figure 3). The similarity of 1/5 and 1/10 degree simulations demonstrate approximate convergence of the solution. The details of the breaking front would be smoothed out by the coarse resolution during propagation computations, but we expect the models to provide robust estimates of the general wave propagation dynamics and distribution of maximum amplitudes for the resolution chosen.*

Response: I think for the long wave part far from the impact this is sufficient, although discussion need for more work on breaking modelling for more intermediate propagation distances should be discussed (see above).

Line-by-line comments:

Line 33: "seafloor" should this be "near-shore regions", or is it actually the deep water part (e.g. 4 km)? The statement might give the impression that many deep water seafloors are scoured.

Line 140: "do not depend strongly": There seems to be between 30-70% amplitude difference? Please revise the text and refer explicitly to the differences obtained in the SI.

Line 141: I would suggest to say "moderate dependence of the water depth" and not "weak", as there are clear differences between the amplitudes for 1 and 3 km depths.

Line 161: Please revise and state that dispersion is not included. This is important for modeling the wave fission properly. Please also explain clearly that such effects have not been investigated here, and that lack of including these can possibly influence the wave propagation significantly, but that more investigations would be necessary to quantify this effect. You may add that more research is needed to shed light on this phenomenon.

Line 162: Please include a statement related to resolving bore propagation. With such low resolution, bores will not be properly resolved, and only the long wave propagation properly covered by the model, at least in the intermediate range before bore dissipation has settled.

Line 174: This study was limited mostly to shallow water type models, which, as discussed above might have limitations related to phenomena such as impact tsunamis that have very different characteristics from earthquake induced tsunamis. It might be worthwhile to refer to other investigations that include dispersion, e.g. Pedersen (2008), https://www.worldscientific.com/doi/10.1142/9789812790910_0001, Kirby (2016), https://doi.org/10.1061/(ASCE)WW.1943-5460.0000350.

Line 232: These differences, looking at Figure 3, appears as surprisingly large, given that the same model eq are solved. Please comment on this. You possibly need to change the colour bar to highlight better how large differences are. You should also compare the maxima.

Line 255: In the abstract you indicate that erosion would take place at the sea bottom. Would it be possible to give some examples (e.g. critical water depths at some different locations). This would be more explicit and understandable to the reader.

Line 267: At which water depths, typically, does these above 20-cm/s velocities occur?

Line 285: The above snapshots in figure 3 does not support this claim. Please back this statement up more explicitly, or give examples of differences.

Line 551: But the discussion related to the possible frontal undular bore dissipation which is equally relevant, please add a discussion here.

Line 563: Discussion related to investigations of wave breaking and possible undular bore propagation in the intermediate distance should be added (see above).