JOURNAL OF COASTAL AND HYDRAULIC STRUCTURES

Review and rebuttal of the paper

Physical modelling of tsunami barrier and debris interaction

Schlesier et al.

Editor handling the paper: Bas Hofland

Subject: [JCHS] Editor Decision

Hauke Günther Schlesier, Hajo von Häfen, Nils Goseberg:

We have reached a decision regarding your submission to Journal of Coastal and Hydraulic Structures, " Physical modelling of tsunami barrier and debris interaction".

Our decision is to ask for moderate revisions.

The three reviews are rather well in agreement: this is novel research that is worth publishing. However, modifications are required before the manuscript can be accepted. The main items I can distill are the following. The number of cases investigated is rather low, one or two extra would really improve the general validity of the results. Moreover, the grammer needs at least one more revision to become journal quality. And some more careful discussion of model assumptions and practical issues is required.

Please use all points raised by the reviewers to improve the manuscript. Please resubmit the paper in two versions, with and without 'track changes'. Add a clear numbered rebuttal where you treat discuss for all points raised, one-by-one, where you improved the manuscript, or why you chose not to.

I am looking forward to receiving the improved manuscript.

Regards,

Bas Hofland
JCHS

Subject: Responses to reviewers' comments and suggestions

Manuscript Number: 5516

Title: Physical modelling of tsunami barrier and debris interaction

Dear Editor,

The authors would like to express their appreciation for the constructive comments raised by the respected reviewers and yourself - addressing them will have increased the overall quality of the revised manuscript. A thorough revision of the manuscript was conducted.

Pleased find enclosed below detailed answers to reviewers' comments, as well as the corresponding actions performed to the original manuscript submission. The actions are linked to the marked copy by line numbers.

Sincerely,

H. Schlesier, H. von Häfen and N. Goseberg

Reviewer B

This paper presents an experimental study of a novel tsunami barrier, and its interactions with a tsunami bore and (more importantly) the debris carried by this bore. Experiments were undertaken carefully, and observations and measurements were synthesised well in their discussion. I believe that this paper will be of significant interest to the readership of the Journal of Coastal and Hydraulic Structures, and that it meets the requirements for data quality and novelty. However, I cannot yet recommend its publication without (at least) minor revisions.

From a technical perspective, the content of this paper is excellent. Many of my comments on technical matters are out of my own interest and may be used by the authors to inform future work on the topic if they wish. However, the quantity of experiments and amount of discussion is rather lacking considering some of the stated aims of the paper. Additionally, the level of written English is not yet appropriate for publication. The parameter space should be increased (although I'm happy for the authors to respond to this point if they do not agree), while the discussion should be extended in some areas and the quality of written English in the manuscript should be improved before the paper can be published.

I provide general questions and comments on the key areas of potential improvement, while specific comments pertain to grammatical/spelling errors (not an exhaustive list, but highlighting key errors that the authors should improve upon in their own proof-reading) and some items for the authors to consider in their future research efforts.

Comment 1.1

One of my only concerns from a technical perspective is that the period of the tsunami bore is very short compared to most observed tsunami events (seems to be about 5 minutes at prototype scale). I believe that the authors can address this easily in the paper, on the grounds that the experiment had reached a quasi-steady regime and that the behaviour would not change significantly were the period to be increased. However, it is not acceptable to simply ignore the issue of the scaled tsunami period. At least some discussion should be added on this point.

Answer to Comment 1.1

The authors agree with the comment of the reviewer and his/her argument. Some aspects of tsunami interaction with structures requires long duration flow events whereas other will specifically require the initial short duration where the water rises to obtain meaningful results. The short duration flows are e.g. important where large acceleration of debris may influence impact loading. Longer durations are required to better understand debris damming. In this work, the transient phase of tsunami is rather important, and little will change with longer durations of the approach flow. The authors have thus added a paragraph in the discussion section to reflect the suggested argument (line 388 – 396).

Comment 1.2

The strength of the membrane and cables have been considered, albeit lacking a discussion of the scale effects (I mention this below). However, in Section 1.1 I think that a bit more discussion should be included about the realistic failure modes of such barriers. Abrasion from trapped debris may be worse than hydrodynamic forces (for both barrier and cables), while the foundations will experience significant loads that may be exacerbated by tsunami-induced scour. Additionally, creating a continuous barrier over a meaningful length of coastline will perhaps be the most difficult challenge of all. I encourage the authors to consider these effects in future experimental studies, but would like to see a bit more critical discussion of the realistic issues (and the appropriateness of a hydrostatic framework) in Section 1.1. This can also emphasise the strengths of the *barriers, for instance that the barrier remains functional during overflow (unlike some other coastal structures).*

Answer to Comment 1.2

We agree with this helpful comment and added a short discussion on the appropriateness of the hydrostatic guideline approach and possible failure mechanisms as well as practical challenges that have not been addressed yet in line 55 – 67.

…Marissen et al. (2013) proposed design guidelines based on the pressure distribution along the membrane in a hydrostatic equilibrium state. However, this is a highly simplified scenario since kinetic energy of the impacting tsunami wave will lead to different pressure distributions and overall higher pressure. Moreover, membrane and cable forces are in a dynamic balance during tsunami loading leading to various parameters being design relevant. Thus, barrier parameters likely need to be altered when hydrodynamic forces are considered. Hofland et al. (2015) investigated the dynamic behavior of the barrier with a 1:100 scaled model and found that the principle of a self-lifting membrane barrier (SLMB) is basically applicable. For flow depths larger than the barrier height, the barrier stayed lifted, even while overflow occurred. It is still unclear if the barrier is susceptible to other failure mechanisms such as extensive scouring or abrasion of cables and membrane due to debris. Hofland et al. (2015) noted that a huge lack of knowledge concerning design and behavior of the barrier exists. Understanding local and irregular load concentrations, foundation loading, barrier side end design, varying wave parameters and debris impact would need more research. Concerning debris impact, Hofland et al. (2015) hypothesized that most debris impact would be stopped by the barrier. Furthermore, the acceptance among the local population of constructing such a barrier over multiple kilometers remains to be answered. ….

Comment 1.3

Although 20 ft shipping containers are common at the coast, it would be useful to include discussion of which debris are the most damaging from tsunami field observations. One weakness in the current paper is that there is little discussion about the dependence of the debris results on the composition of debris used in the experiments. Although additional experiments are not required, I believe that the results should be discussed in the context of published works wherein multiple debris types have been considered.

Answer to Comment 1.3

We thank the reviewer for this constructive comment. We took up the suggestion of the reviewer to discuss the dependence of the debris composition in greater detail and added a paragraph in the discussion. Although we agree that experiments with varying debris characteristics would have been useful, it is not uncommon in similar studies to only deal with one type of debris.

Line 415 – 421:

…The mentioned benefit together with unimpaired performance under occurrences of overflow represent a strong argument in favor of the SLMB.

Only a single type of debris is used in this study. It is expected that debris types with less draft like boards, hydro poles, beams, or logs more likely pass the barrier. However, 20 ft shipping containers are utilized frequently in previous studies due to their simple geometry and widespread existence in tsunami-prone areas world-wide (Cheng Chen et al. 2020; Derschum et al. 2017; Häfen et al. 2021; Khan et al. 2000; Nouri et al. 2010; Shafiei et al. 2016; Yao et al. 2014; Nistor et al. 2017a). Hence, their usage in this study allows for a high level of comparability with respect to previous studies. Secondly, considering multiple types of debris is beyond the scope of this preliminary study, since a much larger experimental program would be necessary to quantify statistical variations.

As this is a preliminary study, the number of tests for each case was limited to three. This is however within the range of many studies about tsunami debris transport.

Comment 1.4

I was surprised that an evaluation of the forces exerted on the barrier and cables was not included within the objectives in Section 1.3, and in the study overall. I'm happy for the authors to defer these forces to future studies, but they should say so in this section. However, in lines 170-181, the authors should discuss the scaling and scale effects of the tensile strength of the barrier and cables (only cable diameter is discussed in Section 3.5). Simply deferring this consideration in lines 429-430 is not appropriate – at least some discussion should be given.

Answer to Comment 1.4

Thank you for this helpful comment. We added a statement in the objective and scope section (line 133 – 136) to highlight that forces on barrier and cable are outside the scope of this work and are therefore not included.

…The specific objectives of this study are:

[…]

- *To analyze the performance of the barrier to mitigate debris transport.*

As this preliminary study primes for extensive investigations, the parameter space is limited. The study focuses on a commonly utilized type of tsunami debris and a limited variation of wave conditions. Although understanding hydrodynamic and debris forces exerted on the barrier and cables is vital when it comes to application, the quantification of these forces is outside the scope of this study….

Also, additional discussion about scale effects and scaling of tensile strength has been added in line 369 – 375.

… Out of practicability considerations, membrane and cable thickness were not modeled according to scale. It is unclear whether the relatively thick model membrane might lead to high stiffness that in turn could influence barrier behavior. Cables can be scaled only once the forces loading them are known. Since these forces are unknown, unscaled cables are used. However, tensile stress decreases with increased cable cross-sectional area, which in turn reduces strain. In application, a 40 m long cable could potentially stretch up to 1.2 m if a fracture strain of 3% is applied (Hofland et al. 2015). Reducing strain might also reduce push-back of the FM. This might increase impact forces since horizontal forces cannot result in horizontal movement…

A reference to the discussion about scale effects and scaling of tensile strength was added in line 183.

…The triple-welded membrane fabric possesses a specific weight of 0.14 kg/m^2 and a tensile strength of 150 N/cm (for explanation of scaling of tensile strength see section 3.5) …

Comment 1.5

Given the stated results of the paper, I would have liked to see more experiments undertaken. The question of whether 3 experiments is sufficient for some of the highly variable behaviour of the turbulent bore and its interaction with debris/barrier is valid, and should be considered. More importantly, the discussion of the results is weakened by the lack of cases considered. A linear trend is not remarkable if it is the only trend that can be determined from 2 data points. This is the one instance where I recommend additional experiments. If the effect of impoundment depth (or, more appropriately, bore properties) is of interest in the current study, the parameter space should be widened.

Answer to Comment 1.5

Thank you for this comment, which you and the other two reviewers raise several times. We fully agree that additional cases and widening of the parameter space would reduce some uncertainty. Generally, the larger the test program and the more tests conducted, the more information can be extracted and the smaller statistical uncertainties due to randomness. However, we intended this work as a preliminary experiment, firstly shedding light on the physical processes of debris-barrier interaction. Therefore, the tests are conducted in small scale and with a limited amount of tests and repetitions. In addition, the circumstances of the current pandemic resulted in deferral of research projects, which makes it impossible to conduct additional short-term experiments. We are looking forward building on the findings of this study and conduct experiments in larger scale in near future. At the moment, due to the current difficult situation, we kindly ask you to be lenient that we cannot meet your request.

Comment 1.6

One of the very interesting findings of the current study is that the reversal of flow direction at the barrier (due to its shape and height) prevents (or at least mitigates) the formation of a debris dam. This is a strong argument in favour of such a flexible protection structure, and contrast with a sloping revetment would be nice if the authors have the capacity to undertake such experiments in the future (or simply a comparison with published studies). It would be good to confirm that this behaviour is not simply a function of the chosen debris composition in these experiments, but either way I think that the authors could emphasise some of these potential advantages. This is appropriate even if additional critical discussion of implementation challenges is included.

Answer to Comment 1.6

We thank the reviewer for this comment. We improved on stating the potential advantages for debris dam mitigation more clearly (line 411 – 414):

…However, the flow conditions governing the barrier-debris-interaction (Fig. 7) are highly transient, preventing the formation of a debris dam. This is an advantage of such a barrier as additional loads due to debris damming are mitigated. The mentioned benefit together with unimpaired performance under occurrences of overflow represent a strong argument in favor of the SLMB.…

Comment 1.7

The characterization of the overtopped, trapped and blocked debris in Figure 9 is interesting, if limited by the lack of cases considered (discussed above). Here I'd be interested to see whether the composition of blocked, trapped and overtopped debris changed as a function of time. A time series approach similar to Figure 7 would be very informative here. The consideration of time dependence and the inclusion of additional impoundment depths would help to revise the comment in lines 370-371 that "no clear effect can be observed".

Answer to Comment 1.7

We thank the reviewer for this constructive comment. The question how the composition of debris positioning modes changes as a function of time is indeed interesting. However, the perspective of only a single camera observing the flow in side view prevents such an analysis. To enable temporal resolution in future studies multiple cameras will be used. Regarding the inclusion of additional impoundment depths, we would refer to the answer to Comment 1.5.

Comment 1.8

Lines 9-10 contain a slight tense inconsistency "destroying… claimed…"

Answer to Comment 1.8

We thank the reviewer for this remark. The text was revised accordingly (line 7 – 9).

… Following the earthquake, a strong tsunami and its devastating impact particularly hit the prefectures of Iwate, Miyagi and Fukushima. As a consequence, the earthquake and tsunami damaged over 250,000 buildings, destroyed 140,000 of them completely and claimed almost 20,000 human lives (Dunbar et al. 2011; Fraser et al. 2013; Harbitz et al. 2016) …

Comment 1.9

Line 14 contains a singular/plural error "measures… is…"

Answer to Comment 1.9

We thank the reviewer for this remark. The text was revised accordingly (line 14).

…Besides soft countermeasures such as tsunami evacuation, structural tsunami prevention measures remain to play an important role for tsunami hazard mitigation and are frequently considered by coastal planning agencies…

Comment 1.10

Line 15 highlights some existing coastal protection structures. However, these are generally designed for very different wave conditions. A short discussion about their effectiveness (or lack thereof) for tsunami protection should be included.

Answer to Comment 1.10

We thank the reviewer for this remark. We included the argument that not all protection structures were designed for tsunami loading and added more details to failure mechanisms of existing structures (line 23 – 29).

…Investigations in the aftermath of the Tohoku tsunami indicate that coastal structures could only provide a limited mitigation effect. Failures of sea walls, breakwaters and concrete revetments due to overflowing of water resulting in fracture of crown armor, overturning due to the forces exerted by withdrawn water and scouring of the dike toe was observed (Kato et al. 2012; Fraser et al. 2013). Although, it should be noted that not all structures were designed for tsunami loadings but for floods, typhoons or wind waves. Overall, breakwaters with a combined length of 8,500 m collapsed during this Tohoku tsunami event (Fraser et al. 2013). Therefore, Takahashi et al. (2011) suggested that emphasis should be placed on stability so that protection infrastructure stays structurally intact even during high impact tsunami…

Comment 1.11

Lines 18-20 should also include a discussion (or at least mention) of the effectiveness of nature-based solutions as a function of length scale, as the required footprint of these solutions may be prohibitive in many coastal contexts.

Answer to Comment 1.11

We thank the reviewer for this comment and revised the text accordingly. We integrated the argument put forward by the reviewer in line 20 – 22:

…Similar observations were made during the Tohoku tsunami in Japan 2011, which led to the design of forested embankments and comparable land use planning (Strusińska-Correia 2017). The use of such nature-

based approaches to inundation mitigation is limited by the required space in inland direction, which is restricted in some coastal areas. For example, Lawrence and Nandasena (2019) showed that forest extending 150 m landwards had no mitigation effect on tsunami inundation….

Comment 1.12

The wording "touching ecosystem aspects" is currently unclear. Although a lengthy digression is unnecessary, this sentence should be reworded for clarity.

Answer to Comment 1.12

Thank you for the comment. The sentence was revised (line 31 – 32).

Coastal structures tend to compete for space and aesthetics within the coastal area while also impacting local ecosystems…

Comment 1.13

Line 78 contains an instance of incorrect comma use. This is fairly prevalent in this article.

Answer to Comment 1.13

We thank the reviewer for bringing this to our attention. The script was thoroughly revised, and mistakes of grammar were taken care of.

Comment 1.14

It would be useful to quantify the reduction of maximum inundation due to debris presence in lines 81-84. Is this significant compared to the errors within which we can estimate inundation?

Answer to Comment 1.14

We thank the reviewer for this comment. Unfortunately, Yao et al. 2014 does not quantify the reduction of maximum inundation. However, they noted uncertainties in the maximum inundation, which we added to the following paragraph (line 88 – 89):

…Yao et al. (2014) investigated motion of debris accelerated onto a sloped beach model by a broken solitary wave in laboratory. They concluded that presence of debris can reduce maximum inundation (although maximum inundation was subject to high uncertainty). A similar effect on maximum inundation was observed for the presence of built environment (Goseberg and Schlurmann 2015). This supported Khan et al. (2000) who observed an increase in surge deceleration and surge height with increasing debris amount…

Comment 1.15

Line 82, "build" should be "built".

Answer to Comment 1.15

Please see Answer to Comment 1.13.

Comment 1.16

The multiple-line dimensioning used in Figure 1 is slightly confusing and does not include all of the gauges. I encourage the authors to revise this part of an otherwise excellent figure.

Answer to Comment 1.16

The authors are not sure if the reviewer intends referring to Figure 1, but to Figure 2 since gauges are depicted only in the latter figure. Also, we are uncertain what the reviewer means by 'multiple-line dimensioning'. Probably there is some lack of clarity concerning the depicted wave gauges (e.g. WG and WG'), which refer to different positions in experimental phase 1 and 2 & 3. This is indicated in the legend of the figure but is not mentioned in text until section 2.2. In sake of clarity the caption of Figure 2 was revised (line 148 – 152).

…Figure 2| Flume dimensions and bathymetry of the test facility as well as the Self-Lifting Membrane Barrier (SLMB) and measuring devices: Wave Gauges (WG), Cameras (CAM) and theirs positions within the three phases of the experimental protocol. a) side view, b) top view, c) detail of the lifted SLMB including dimension. Wave gauges positions indicated without index ' (e.g., WG 5) belong to experimental phase 1. The gauge positions were shifted for experimental phase 2 & 3 indicated by index ' (e.g., WG 5' see section 2.2)…

Comment 1.17

Although Froude similitude was applied, I could not find a summary of the Froude numbers of the bores for the different experiments undertaken. This should be included in Table 3, along with a brief discussion of how these Froude numbers compare to published field, laboratory and numerical studies.

Answer to Comment 1.17

The Froude-number of a dam break wave is variable over space and time because it is a function of flow depth and flow velocity. Consequently, the wave front with high velocities and small flow depths has extremely large Froude numbers, which decrease with increasing flow depth over time. Therefore, a comparison of Froude numbers to previous literature is difficult. In general, Froude's similarity is applied to gravity waves, which are gravitationally driven, which is the case for dam break waves. Therefore, the authors believe that Froude's similarity is applicable, although no comparison to existing literature is possible.

Comment 1.18

A 1:100 scale seems a bit excessive and is definitely at the high end of tsunami bores that we might expect. Although I don't have a strong opinion on this point, I question whether a smaller geometric scale might be more appropriate for these exploratory tests.

Answer to Comment 1.18

We thank the reviewer for this comment. The authors see the conducted experiments as preliminary for insights in this complex interaction between barrier and debris. We are looking forward executing refined experiments in larger scale in future studies. In that sense, the information on the length scale is only indicative.

Comment 1.19

The authors rightly discuss the possible effect of debris draft on the results. It would have been useful to vary the density of the model-scale debris to investigate these effects, particularly if lower-density debris are more likely to overtop such a barrier. This can be deferred to future work but would have strengthened the current manuscript.

Answer to Comment 1.19

We agree with the reviewer at this point. For context why we choose not to conduct experiments with extra cases at this point, please see comment 1.5.

Comment 1.20

Line 192 does not provide details on how accuracy of debris positioning was confirmed. This should be mentioned.

Answer to Comment 1.20

We thank the reviewer for bringing this to our attention. We added an explanation in line 200 – 202:

…The debris are orientated with its long side parallel to the flow direction and care was taken to accurately position the debris prior to each experimental run. In order to position debris accurately tags were marked to the according places on the beach model board …

Comment 1.21

Line 198, "experiment configuration" should be "experimental configuration".

Answer to Comment 1.21

Please see Answer to Comment 1.13.

Comment 1.22

Lines 273-274, it would be useful to include some citations to studies that have used (or justified) this linear interpolation approach.

Answer to Comment 1.22

We thank the reviewer for this feedback. We included two studies that have used linear interpolation (line 273 – 275).

… Hence, surface elevation is interpolated linearly between wave gauges and extrapolated beyond the last wave gauge. Linear interpolation has been used in former studies for minimum flow depths and maximum crests amplitudes of landslide-generated tsunami before in laboratory (Jaffe and Gelfenbuam 2007; Lange et al. 2020)…

Comment 1.23

Lines 275-276, I'm not entirely convinced by the "total dissipation of overtopped flow kinetic energy" argument here. Some citations to relevant overtopping literature, and a consistent approach with such studies, would be useful here. Likewise, the "roughly even" water levels downstream of the barrier (Line 280) is difficult to justify with effectively 2 data points relating to impoundment depth. Do you really expect such a trend to continue with increasing impoundment depths?

Answer to Comment 1.23

Thank you for the comment. It is absolutely correct that with two data points only a linear surface elevation can be assumed. In fact, while conducting the tests it was observed that the water level upstream of the barrier was mostly even. Therefore, this assumption is correct in this case. This was added to the discussion and in addition, studies were added which prove that mitigation systems can lead to energy dissipation.

Line 397 – 406:

… Interpolation and extrapolation of maximum surface elevation and propagation velocities are carried out linearly since the gradient in surface elevation is approximately constant when no barrier is present (Fig. 4). Interpolations of maximum surface elevation and propagation velocities close to the barrier are not accurate (see Fig. 5). Assuming an even water level downstream the barrier is appropriate for larger distances to the barrier. Close to the barrier, a stilling basin effect is observed (see Fig. 4b, Fig. 5c, d), where a large amount

of energy is dissipated in turbulent flow. Thus, the linear interpolation is valid in larger distances downstream the barrier. It has been shown both in numerical and physical studies that tsunami breakwaters and other mitigation systems can lead to energy dissipation (Li et al. 2019; Rahman et al. 2021). The significant reduction in downstream propagation velocity (see Fig. 5c and d) indicates that the SLMB can delay tsunami arrival further inland. This potential advantage of the barrier could be detected by using additional wave gauge and velocimetry probes downstream the barrier in future studies. ….

Comment 1.24

Line 329 "resulting it…" is not worded correctly. Please revise.

Answer to Comment 1.24

Please see Answer to Comment 1.13.

Comment 1.25

Line 382, "where" should be "were".

Answer to Comment 1.25

Please see Answer to Comment 1.13

Comment 1.26

Lines 399 and 400 might motivate some basin tests in the future (tricky but interesting).

Answer to Comment 1.26

We fully agree with the reviewer's comment. Future experiments in wider channels or basins would likely eliminate unwished model effects like the error that results from water flow around the barrier's side ends.

Reviewer C

Comment 2.1

The manuscript presented here examines the interaction between debris and a self-lifting membrane barrier in tsunami-like flow conditions. The manuscript describes the complex interaction between debris and the barrier focusing on the influence of the barrier on debris capture and the associated impact on hydrokinetic conditions. The methodology and results are well-written providing lots of information of the experimental methods and physical phenomenon. A quick check of grammar would be helpful to improve the manuscript as well as a check for redundancy as there is a tendency to repeat aspects in different sections, which can sometimes make the manuscript hard to follow. Overall, the manuscript provides a good preliminary description of the interaction between debris and this relatively new design barrier.

Please review the comments (grouped into Major and Minor). I hope that the authors will address them, along with the above mentioned as this is very interesting work.

Comment 2.1

Please do another check for grammar; there are several errors (particularly in the introduction) that should be checked. I noted some of them in the Minor comments but a more thorough check is necessary.

Answer to Comment 2.1

We thank the reviewer for bringing this to our attention. The script was thoroughly revised, and mistakes of grammar were taken care of.

Comment 2.2

Section 1.2: A brief discussion on debris flow (as opposed to individually floating debris) would be interesting, as this would have an influence on the hydrodynamic forces (at least).

Answer to Comment 2.2

We thank the reviewer for this comment. The authors have difficulties to understand this remark. Does the reviewer would like to have the flow of multiple debris discussed as opposed to flow of a single debris piece? If this is the case, the authors would like to refer to the following paragraph in section 1.2 where most of the studies regarding flow of multiple debris pieces are mentioned:

Line 78 – 93:

…Naito et al. (2014) developed a model to estimate potential debris distribution areas, distances and spreading angles based on satellite images made of shipping container terminals before and after the Tohoku tsunami event in 2011. Nistor et al. (2017b) investigated tsunami-driven debris transport over a horizontal apron under laboratory conditions. They found that the maximum longitudinal displacement as well as the spreading angles depends on the amount of debris. Goseberg et al. (2016b) showed by accurately measuring the debris' path (Goseberg et al. 2016a) that obstructions have an influence on the spreading angle. It was shown that a first row of obstructions decreases the maximum longitudinal transport of debris. Stolle et al. (2018a) tested an approach where the final position of displaced debris can be described by a probability-density-function (PDF), allowing to predict the likelihood of debris impact. It was also found that standard deviation of the debris position distribution depends on initial debris orientation. Yao et al. (2014) investigated motion of debris accelerated onto a sloped beach model by a broken solitary wave in laboratory. They concluded that presence of debris can reduce maximum inundation (although maximum inundation was subject to high uncertainty). A similar effect on maximum inundation was observed for presence of built environment (Goseberg and Schlurmann 2015). This supported Khan et al. (2000) who observed an increase

in surge deceleration and surge height with increasing debris amount. Yao et al. 2014 underlined the high uncertainty concerning debris position and maximum inundation. In conclusion, Stolle et al. (2018a), Nistor et al. (2017a), Goseberg et al. (2016a) and Yao et al. (2014) support the finding of Matsutomi (2009) that tsunami debris transport is characterized by a high degree of randomness…

The authors however have added a short sentence on the difference between debris entrained in tsunami waves and classic debris flow that typically occurs in alpine or mountainous areas. The reviewer is correct that these phenomena are sometimes confused with, and in order to limit chances, a brief information is now included in the manuscript.

Line 70 – 72:

…The debris entrained in tsunami flood waves cannot be confused with debris flows (alpine/mountainous conditions) where –unlike to process at hand– an intricate mass and force balance between soil, pore pressure and free surface water flow exists. …

Comment 2.3

Results section: There seems to be some redundancy in the description of the results. For example, Line 298 and Section 3.1. It can get a little confusing what is being described so I would focus exclusively on the phenomenon you are discussing. It would also be helpful to separate into paragraphs when discussing surge velocity and water depth, it can be a little difficult to follow some of the paragraphs.

Answer to Comment 2.3

Thank you for bringing this to our attention. We eliminated the redundancy concerning the barrier effects on surge propagation velocity in section 3.1 and 3.2. Additionally, section 3.2 was split into three paragraphs instead of two for better overview.

Line 289 – 300:

…The SLMB needs to be functional under debris loading. Thus, understanding the effect of debris on wave parameters is crucial for estimating changes in hydrostatic and hydrodynamic forces exerted on the barrier in debris versus non-debris loading cases. Figure 5 shows the effect of debris on maximum flow depths and surge propagation velocities for different impoundment depths and debris amounts. An effect of debris on maximum flow depth upstream the barrier cannot be observed (see Fig. 5a, b). In runs with low surge impact, the maximum flow depths downstream the barrier increase with increased debris amount, however no such effect is observed for runs with high surge impact. Presence of the barrier leads to decreased maximum flow depths downstream and increased maximum flow depths upstream the barrier (Fig. 5a, b).

The effect of debris amount and barrier on surge propagation velocity is shown in Figure 5c, d. An increased debris amount leads to slightly decreased upstream surge propagation velocities in barrier proximity. This effect is more pronounced in runs with low impoundment depth. However, the effect of debris on downstream surge propagation velocities is unclear: In the high surge impact scenario, downstream propagation velocities vary greatly with debris amount, however no effect can be observed in the low surge impact scenario. ~~Downstream surge propagation velocities are significantly reduced when the barrier is present (Fig. 5c, d)~~….

Comment 2.4

Section 3.2: I may have missed it, but how was surge velocity calculated?

Answer to Comment 2.4

We thank the reviewer for this question. Surge propagation velocity was calculated by dividing the distance between two wave gauges (Fig. 2) by the corresponding difference in surge arrival time assessed trough methods described in Fig. 3c. We added that piece of information in line 265.

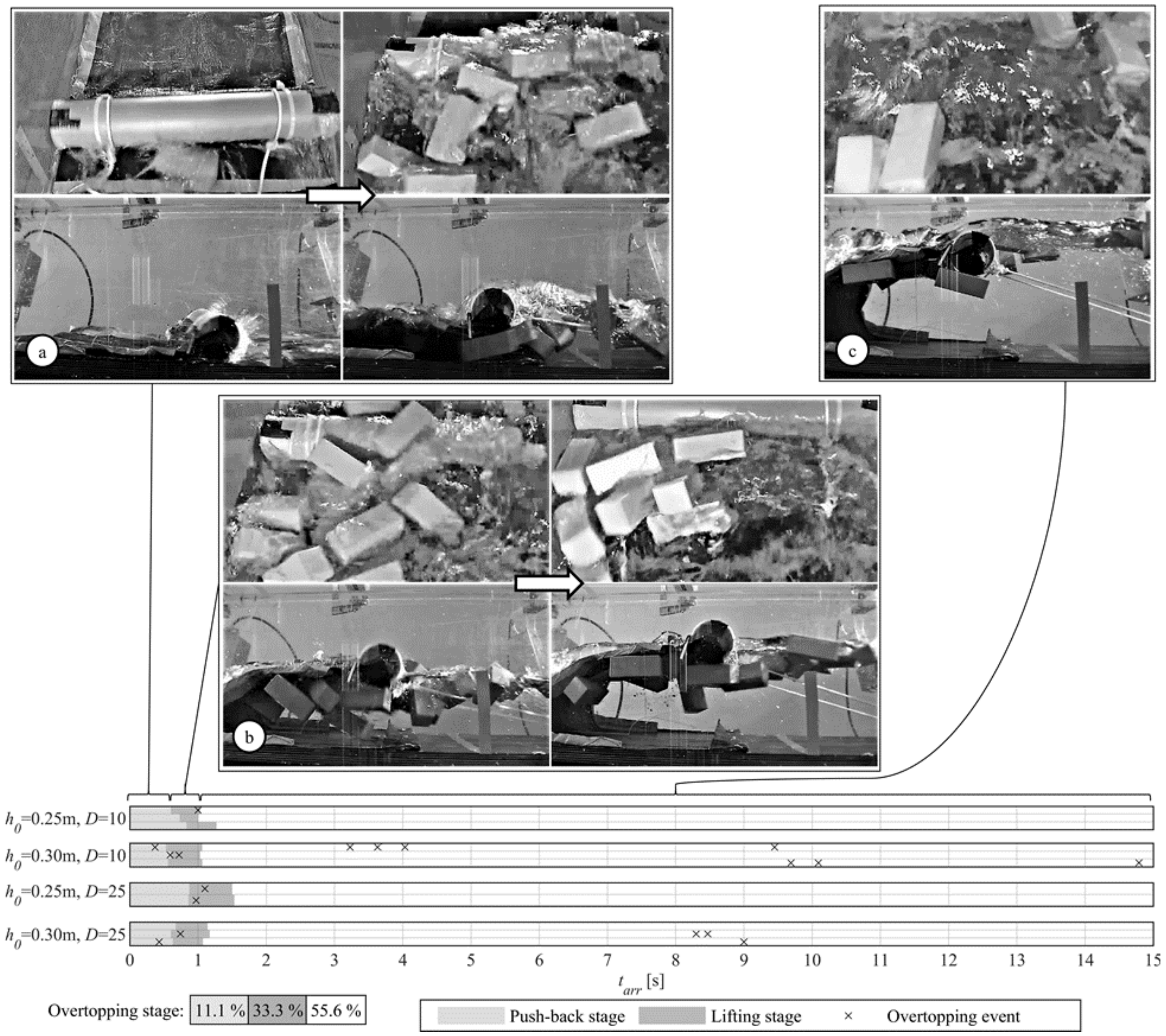
…Maximum flow depths can be derived from the surface elevation time histories by finding the maximum value within the relevant observation period (see Fig. 3b). Surge propagation velocities can be calculated by dividing the distance between two wave gauges (Fig. 2) by the corresponding difference in surge arrival time assessed trough the method shown in Figure 3c....

Comment 2.5

Section 4: Were there any 3D effects from the debris? Just based on Figure 7, it seems that the debris skewed slightly to one side which could incur some 3D influences. Was that the case for all tests or just the one?

Answer to Comment 2.5

We thank the reviewer for this interesting question. We checked the CAM 2 video data for all twelve runs with debris and concluded that the skewed debris positions depicted in Figure 7 was random. Nevertheless, we decided to replace the camera image shown in Figure 7 for a more representative one.



Comment 2.6

Adding a paragraph regarding the scope of the study (one type of debris, limited wave conditions, etc.) would be helpful to frame the methodology section. Adding that you are focusing on debris capture would be helpful, based on the review, I had an impression there would be something regarding forces.

Answer to Comment 2.6

Thank you for this helpful comment, which is similar to comment 1.4 brought up by one of the other reviewers. An additional paragraph in section 1.3 was included where the scope of this work is outlined. Please see the answer to comment 1.4.

Comment 2.7

- Line 9 of Abstract: The sentence is a little awkward; consider revising.

Answer to Comment 2.7

Thank you for this hint. The sentence was revised for improved phrasing (line 6 – 13 abstract).

…Hence, this study firstly investigates the interaction between a self-lifting membrane barrier and tsunami-induced debris transport. A unique set of laboratory experiments in a wave flume are carried out, in which 20 ft shipping-container models are washed into a membrane barrier by a tsunami-like wave. Varying hydrodynamic boundary conditions and altered amount of debris is used to evaluate different magnitudes of surge and debris loading….

Comment 2.8

- Line 7: "… shook the floor at a distance of 130 km off the northeast coast of Japan."

Answer to Comment 2.8

Thank you for your suggestion for improvement, the manuscript was revised accordingly.

Comment 2.9

- Line 11: Indian Ocean

Answer to Comment 2.9

Please see Answer to Comment 2.1.

Comment 2.10

- Line 25: Level 2 tsunami is not necessarily clear to everyone, either remove the reference or explain a little further.

Answer to Comment 2.10

Thank you for this comment. The reference was removed.

Comment 2.11

- Line 29: The second part of the sentence is a little unclear and I don't think necessary, consider removing or explaining the ecosystem aspect further.

Answer to Comment 2.11

Thank you for your comment. Please see Answer to Comment 1.12.

Comment 2.12

- Line 38: "… tension strength of 3.4 kN/mm to withstand the hydrostatic…"

Answer to Comment 2.12

Thank you for this remark. The manuscript was revised accordingly (line 184 – 185).

…The triple-welded membrane fabric possesses a specific weight of 0.14 kg/m^2 and a tensile strength of 150 N/cm (for explanation of scaling of tensile strength see section 3.5)…

Comment 2.13

- Line 55: Check period spacing.

Answer to Comment 2.13

We thank the reviewer for this hint. The spacing was corrected.

Comment 2.14

- Line 77: The comma is not necessary.

Answer to Comment 2.14

Please see Answer to Comment 2.1.

Comment 2.15

- Line 82: "…a built environment…"

Answer to Comment 2.15

Please see Answer to Comment 2.1.

Comment 2.16

- Line 81: The sentence is a little confusing; I would separate the thought regarding the debris and the built environment.

Answer to Comment 2.16

The authors follow the suggestions made by the reviewer (line 86 – 89).

...Yao et al. (2014) investigated motion of debris accelerated onto a sloped beach model by a broken solitary wave in laboratory. They concluded that presence of debris can reduce maximum inundation (although maximum inundation was subject to high uncertainty). A similar effect on maximum inundation was observed for the presence of built environment (Goseberg and Schlurmann 2015)…

Comment 2.17

- Line 87: The sentence is a little convoluted; I am not sure what you mean by strategy. I think you mean using a dam-break wave but I am not sure.

Answer to Comment 2.17

We thank the reviewer for this constructive comment. The sentence was revised for clarity (line 94 – 95).

…Laboratory conditions should approximate the complexity associated with the spatiotemporal scales of the fluid-structure-interaction. Thus, tsunami propagation over land is often simulated by bores or surges using dam-break waves.…

Comment 2.18

- Line 186: "Test runs" sometimes refers to experimental trials, I would just remove that phrase and just use "calibrations runs".

Answer to Comment 2.18

Thank you for this comment. The authors agree and reworked the whole manuscript accordingly (line 196).

…~~Following calibration and test runs, the~~ The experiment protocol included a total of 24 experiment runs within three different experiment setups (Table 3)….

Comment 2.19

- Line 225: "… were installed in close proximity to the …"

Answer to Comment 2.19

Thank you for this comment. We adopted the proposed wording (line 229 – 232).

… The cameras (CAM 1, CAM 2) are installed in close proximity to the membrane barrier. The field of view (FOV) of CAM 1 allows for an overview and upstream perspective on the barrier, whereas the FOV of CAM 2 provides a side view perspective (see Fig. 2a). …

Comment 2.20

- Line 225: How were the cameras synchronized? Alternatively, were they synchronized?

Answer to Comment 2.20

Thank you for rising our attention to this missing information. We added a brief mentioning of the synchronization methods in line 232 – 233.

…The field of view (FOV) of CAM 1 allows for an overview and upstream perspective on the barrier, whereas the FOV of CAM 2 provides a side view perspective (see Fig. 2a). Synchronization of camera footage was achieved visually by surge arrival at the tape markers upstream the SLMB (see Fig. 3a) as well as by a clapping noise on the soundtrack. An evaluation of the image data recorded allowed for qualitative and quantitative assessment of the surge-barrier-interaction, including debris damming and debris obstruction as well as capture ratios.…

Comment 2.21

- Figure 5: The markers are a little difficult to see. It would also help if the markers were different (like in Figure 4) to help distinguish the lines further. I would also adjust the figures so you are showing the same data between the two since it is similar analysis (i.e. subtract the beach slope).

Answer to Comment 2.21

We thank the reviewer for this comment. The authors can understand the reviewer's concern. Indeed, markers were included in an earlier version of this figure. This however led to a diminished clarity of the figure as some data points were so close to each other that markers were covered by each other. This was also the reasoning for splitting the figure for different impoundment depths. The authors rated the improved comprehensibleness of the figure higher than the uniformity with previous figures.

Comment 2.22

- Line 318: I am not sure what "not outscore" means in this context.

Answer to Comment 2.22

We thank you for this comment and rephrased the sentence (line 306 – 308).

…In the high surge impact case increased debris amount leads to slightly higher flow depths at the beginning of the roller formation. Both Figure 5a and 6 indicate that debris-induced backwater rise does not lead to increased maximum flow depths.…

Comment 2.23

- Line 326: "…and shows the barrier dynamics as well as the debris dynamics…"

Answer to Comment 2.23

We thank the reviewer for this remark. The sentence was revised accordingly.

Comment 2.24

- Line 346: Sentence is a little awkward, consider revising.

Answer to Comment 2.24

Thank you for this remark. The sentence was revised (line 333 – 335).

…Barrier-debris-interaction is a highly dynamic and transient phenomenon. Usually, only $1.15^{+0.38}_{-0.22}$ *s lasts from surge arrival to completed barrier lift. A dam-like debris pattern stable for more than a short fraction of a second could not be observed….*

Comment 2.25

- Line 361: I am not sure what the "attributions determining factor" is.

Answer to Comment 2.25

We thank the reviewer for this remark and rephrased the paragraph (line 344 – 349).

…For this purpose, three position modes are introduced to categorize the debris model positions in relation to the barrier (Fig. 8) before the 1^{st} *surge reflection arrives at the barrier (see Fig. 3b). Debris models that are washed over the FM of the membrane barrier are categorized as overtopped. Whereas the mode blocked is attributed with debris found upstream of the FM, the mode trapped refers to the cavity that forms when the membrane barrier is lifted (see Fig. 1 and Fig. 8). The decision in which mode a debris is, is made based on the position of its geometric center. Figure 8 depicts the boundaries of the mode regions.*

Comment 2.26

- Line 391: "impractical"

Answer to Comment 2.26

Please see Answer to Comment 2.1.

Reviewer D

This paper examines the effects of a self-lifting membrane barrier (SLMB) on tsunami inundation and tsunami-induced debris transport in coastal areas, by conducting physical experiments. Although the number of cased investigated was limited, the paper includes some important findings. The reviewer suggests that some additional explanations and discussions should be included, before moving to the publish. Please find the following comments.

Comment 3.1

The effects of SLMB on the flow depth was discussed in the paper, but how about those on tsunami arrival time inland? It is expected that SLMB somehow can delay tsunami arrival time inland because it hinders inundation from the coast, and thus it is better to discuss this point as another function of SLMB?

Answer to Comment 3.1

We thank the reviewer for this constructive comment. We picked this point up in line 402 – 406 adding a few sentences.

… It has been shown both in numerical and physical studies that tsunami breakwaters and other mitigation systems can lead to energy dissipation (Li et al. 2019; Rahman et al. 2021). The significant reduction in downstream propagation velocity (see Fig. 5c and d) indicates that the SLMB can delay tsunami arrival further inland. This potential advantage of the barrier could be detected by using additional wave gauge and velocimetry probes downstream the barrier in future studies. ….

Comment 3.2

In the discussion part, side end design of SLMB was mentioned. How did you care these sides (in particular the gap between the channel side and the SLMB model side) in the experiments presented in this paper? To discuss the effects of SLMB accurately, it is important to understand well the behavior of water and SLMB model at the sides. Please some more detailed explanation on this point.

Answer to Comment 3.2

Thank you for this comment. We would like to point to line 185 – 189 in section 2.1 where side end design for our barrier model is explained. However, the membrane is not entirely sealed against the flume walls which results in a model effect discussed in section 3.5.

Line 185 – 189:

… On both edges of the membrane, a 0.01 m wide and 0.32 m long stripe of tape is added to reduce side leakage between membrane and flume wall. Due to the water pressure acting on the membrane and unfolding it entirely, the tape is pushed against the acrylic side walls of the flume sealing the membrane against the flume wall. However, the sealing is not fully watertight which may have resulted in a minor model effect discussed in section 3.5. …

Line 376 – 379 (section 3.5):

…Debris impacting the membrane near the wall slide around the membrane in some cases (Fig. 10b). These debris models were counted as trapped. The sealing between flume wall and membrane is disturbed while a debris is passing, leading to leakage which increases downstream flow depth. Furthermore, friction between membrane as well as the FM and the flume wall influences the barriers lifting time…

Comment 3.3

It was mentioned that cases were carried out 3 times. However, with a number of debris models, 3 times is enough to capture a reliable result? Debris transport looks very random, thus perhaps it is better to carry out more tests for each case in order to see the characteristics of the behavior of debris. Please also discuss the number of tests needed for each case of this type of experiments.

Answer to Comment 3.3

We thank the reviewer for this comment. Regarding the request for more experimental runs, we kindly ask the reviewer to refer to Comment 1.5. The latter question about the number of tests necessary is addressed in section 4 by a short discussion about repetitions conducted in related studies.

Line 420 – 426:

…Secondly, considering multiple types of debris is beyond the scope of this preliminary study, since a much larger experimental program would be necessary to quantify statistical variations.

As this is a preliminary study, the number of tests for each case was limited to three. This is however within the range of many studies about tsunami debris transport. Repetitions in studies presented in section 1.2 vary from one to six, some of them conducted three tests per case as well (Goseberg et al. 2016a; Goseberg et al. 2016b; Nistor et al. 2017b; Khan et al. 2000; Stolle et al. 2017a; Stolle et al. 2017b; Nouri et al. 2010). Nevertheless, other studies carry out more tests (ten in Yao et al. 2014, ten to thirty in Derschum et al. 2017).

…

From: Bas Hofland

To: Hauke Günther Schlesier, Hajo von Häfen, Nils Goseberg

Subject: [JCHS] Editor Decision

Dear Hauke Günther Schlesier, Hajo von Häfen, Nils Goseberg:

We have reached a decision regarding your submission to Journal of Coastal and Hydraulic Structures, "Physical modelling of tsunami barrier and debris interaction".

Our decision is to accept the paper for publication.

Note that Reviewer 2 kindly replied to your rebuttal, as given below. The reply includes a couple of textual remarks. Would you be so kind as to deal with these items, add the author names to the manuscript, complete sections that were not filled in yet like the Credit section, and update the formatting to the best of your ability, and then resubmit the paper. As the formatting has changed slightly from the template, it is preferable if you use the latest formatting template from the JCHS.

After your resubmission our copyediting-editor will check formatting. Small changes can be made by him and sent back for approval, or suggestions can be given to improve formatting further.

Thanks again for choosing JCHS, and I look forward to having your paper online.

Best regards,

Bas Hofland
JCHS

Response Reviewer 2

Review of Physical modelling of tsunami barrier and debris interaction (revised submission)
I thank the authors for their efforts in responding to my suggestions, including the relevant changes to the manuscript. I am satisfied that the manuscript now meets the standards for publication in the Journal of Coastal and Hydraulic Structures and suggest that it is accepted for publication. There remain a few minor grammatical errors in the revised manuscript. However, I'm satisfied that the authors can quickly deal with these without the need for further review.
I have adopted the authors' helpful comment numbering in my suggestions and comments below.

Comment 1.1
My thanks to the authors for the response and for the paragraph in the revised manuscript. These have addressed my original comment. I note two grammatical errors in the revised manuscript:

• “This study uses broken bores originating from a dam-break mechanism, modelling the hydrodynamic effects tsunami wave propagating over dry land have.” Ending the sentence on a verb is slightly strange. I suggest rewording to “This study uses broken bores originating from a dam-break mechanism, modelling the hydrodynamic effects of a tsunami wave propagating over dry land.”
• “In this context, it is noted that the duration of the broken bores when interacting with the SLMB, as well as during the entrainment and transport of debris is short when compared to observed tsunami durations; …” I suggest adding an additional comma after “debris” for clarity.

Comment 1.2
I am satisfied that the authors have addressed this comment. I agree with their comments about the lack of knowledge on these topics, although I’d perhaps state this as a “significant” lack of knowledge rather than a “huge” lack of knowledge. This is merely my own preference regarding wording, so the authors are welcome to keep this paragraph unchanged if they like.

Comments 1.3-1.4
I am satisfied that the authors have addressed these comments.

Comment 1.5
I thank the authors for their response, and for discussing some of the context within which further experiments are impossible. I don’t believe that this should hold up publication of this paper, so am not inclined to press the point. However, I do suggest that a short qualifying statement could be added near the end of Section 3.4, noting the limited number of repetitions.

Comments 1.6-1.9
I am satisfied that the authors have addressed these comments.

Comment 1.10
I am satisfied that the authors have addressed this comment, although I would add a minor grammatical suggestion: Remove “Although” at the start of the sentence following the Kato and Fraser references. The word is not necessary here.

Comment 1.11-1.13
I am satisfied that the authors have addressed these comments.

Comment 1.14
I am satisfied that the authors have addressed this comment. Again, I have minor grammatical suggestions.
• “(although maximum inundation was subject to high uncertainty).” – change to “, although the measurement of maximum inundation was subject to high uncertainty.”
• “was observed for the presence of built environment” – change to “was observed due to the presence of built environment”.

Comment 1.15
I am satisfied that the authors have addressed this comment.

Comment 1.16

My apologies to the authors for any confusion caused. I was indeed referring to Figure 2. My concern was less with the caption, and more with the numerous dimension lines indicating the position of the different wave gauges. In my opinion, these make it rather difficult to interpret the locations of the different instruments within the figure. I don't have a strong opinion on this, but suggest that the authors could improve the clarity of the figure.

Comment 1.17
I agree with the authors comments about the variability of the Froude number and I certainly didn't mean to suggest that Froude similarity is not appropriate for this problem – it is the most sensible choice. However, I do not agree that it is impossible to compare values with the relevant literature, wherein the same spatial and temporal variability exists. Many studies quote the Froude number according to the mean depth and velocity measured within the "quasi-steady" phase of the flow, or according to the depth and velocity of the flow front. The former would be between approximately 5s and 12s (referring to Figure 3b), and gauge WG2 would be an appropriate location to determine these values (referring to Figure 2). Either quasi-steady or front would provide a representative Froude number that could be easily compared to the literature. The authors are welcome to ignore this suggestion as it pertains to the current manuscript, but I do suggest that they consider such a calculation in their future investigations.

Comment 1.18
I am satisfied that the authors have addressed this comment. However, I was not merely referring to the need to conduct larger-scale experiments in the future. I was more referring to the field-scale tsunami that the research is intending to replicate in the laboratory tests, irrespective of scale. Although it can be tempting to design for a very large tsunami, it would be interesting to look at the performance of the proposed mitigation measures under slightly less "extreme" tsunami conditions. I don't require any further changes to the manuscript in response to this comment, but suggest that the authors consider this in their future experimental work.

Comment 1.19
I am satisfied that the authors have addressed this comment.

Comment 1.20
I am satisfied that the authors have addressed this comment, although I suggest that they check for singular/plural errors in the first sentence ("are… its…").

Comments 1.21-1.22
I am satisfied that the authors have addressed these comments.

Comment 1.23
I am satisfied that the authors have addressed this comment and thank them for their thorough consideration and response.

Comments 1.24-1.26
I am satisfied that the authors have addressed these comments.