We appreciate the careful and insightful reviews of all five referees. Below we address all points raised (answers are colored), and indicate the changes to the manuscript that were made to address these points (*in italic*).

Referee 1

I think that this is a wonderful paper because it addresses a rarely observed, but widely acknowledged, phenomena that occurs within the iceberg-calving zone of Greenland (and other marine terminating) glaciers. The potential for extraordinarily violent ocean wave phenomena, documented from a unique perspective within this study, suggests that episodic, rare events contribute not only to hazards in the vicinity of calving fronts, but may actually help to determine the state of ice in the area (e.g., ice fracture in response to impulsive pressure variations caused by calving and other impulsive mass movements).

I have really no significant comments except for a couple of suggested re-wordings:

1. At the outset, the term "slide" should be defined as the "calving" or "face collapse" that are more common ways of describing the source. I was midway into the paper and realized that I was seeing the word "slide" and had to figure out that it was a reference to a "mass slide" or "ice slide" or "land slide"...

We now added "ice slide" to most occurrences to avoid this potential misunderstanding.

2. The manuscript presents ranges of numbers using three periods. e.g.., 1....5 represents "in a range of 1 to 5". I dont know if this is within the style of TC... Id prefer a longer-handed way of writing out the range.

We gave this some thought, and figured this would be the best solution. Using dashes looks like negative values. Given the irritation of several referees, we changed this to dashes, e.g. from $1 \ldots 2$ m to $1 - 2$ m.

3. p. 6475 line 7... the boat was not "in" but was "at a distance"

changed as suggested

4. p/ 6477 line 20 not "unstable" but "unstable"

We now use "unstable" everywhere, even though "instable" seems to be a valid form as well (which it should, being derived from Latin; `http://www.merriam-webster.com/dictionary/unstable`).

Otherwise, I am delighted to read such an interesting paper and to see a top-notch observation come from the combination of disparate and unexpected field sources.

We really appreciate these kind words.

Referee 2

This is a valuable contribution to the field of landslide-tsunamis given that a real event was documents (video) and measured (terrestrial radar interferometer, tide gauge) in great detail including slide and tsunami properties at various locations from the impacting mass. Field data about landslide-tsunamis are rare indeed, and the data presented by the Authors are perhaps the best documented data set of its kind so far. The Authors further describe the field data with empirical methods based on laboratory data, and make prognoses for potential future events. The article is well written and most figures are nicely presented. Before the article may be considered for publication, however, the comments below should be addressed by the Authors.

Specific comments:

The Authors do not fully appreciate the effect of the water body geometry. Based on Fig. 1, the presented case is clearly a three-dimensional (3D) event (the waves propagate on semi-circle from the source), in contrast to most studies investigating landslide-tsunamis in two-dimensional (2D) geometries representing narrower water body geometries such as narrow lakes or reservoirs. The wave magnitude between 2D and 3D easily changes by 1, 2 or even more order of magnitudes, particularly far away from the ice impact location (see recent contributions to this field such as Heller and Spinneken 2015). In Section 5.1 the Authors got it right as they apply mainly empirical equations derived for 3D cases. However, Eq. (3) is based on 2D and should not be applied to this 3D event due to several reasons such as the incorrect geometry of the water body, the fact that this formula was derived for rock slides (density of 2745 kg/m3) rather than ice and also the violation of parameter limitations; however, later studies conducted in the same institution (Zweifel et al. 2006, Heller and Hager 2010) include a much wider parameter range including densities lighter than water.

The article by Heller an Spinneken (2015) was not published when this manuscript was written, but it gives good guidance how to deal with 3D effects.
However, using formulas (4) and (5) from Heller and Spinneken (2015) yields completely unrealistic wave heights for impact water depths of $h_0 = 20 - 40\,\mathrm{m}$. If, on the other hand, we use formula (4) to infer impact zone water depth, we obtain $h_0 = 70 - 80\,\mathrm{m}$. Analyzing the data set with these water depths gives results in disagreement with all other measurements, as now discussed in the manuscript

An alternative formulation for the maximum wave amplitude in 3D (Eq. 5 in Heller and Spinneken, 2015) yields values that are an order of magnitude too high for the inferred impact zone water depth. Using on the other hand this formula to infer water depth yields $h_0 = 70 - 80$ m*, which, however, lead to higher values of wave speeds, smaller values of far-field wave heights, and different wave type parameters (cnoidal instead of bore-like) than observed.*

Concerning the question of 2D vs 3D equations it should be noted that the geometry of the impact area is constrained on the left (east) side by an ice wall, such that the geometry is something in between 2D and 3D. But obviously we agree that the wave propagates across a relatively open bay, and that 3D equations should be used.

Title, P6472/L2/3, P6473/L14, P6475/L10/11/12, P6479/L18, P6482/L3, P6483/L4: The terms impulse wave and tsunami are very much related and indicate basically the same phenomena. The terms impulse wave and tsunami differ only in the sense that impulse wave is the general term, while a tsunami is an impulse wave in an open water body such as an ocean. For restricted water bodies such as a lake or reservoir, the term impulse wave should be used. However, the application of these terms is changing over time and more and more scientists use the term tsunami also to describe waves in lakes. Anyway, in the present study the wave may just be called tsunami. E.g. the title may be written as Multi-method observation and analysis of a tsunami caused by glacier calving to avoid repetition. This needs to be revised in the entire manuscript.

The common usage of the term "tsunami" for the source mechanism was not clear to us. We have now changed the "impulse wave" to "tsunami" in most instances.

P6477/L18: The water depth on its own is not the most important parameter, it is rather its relation to the ice impact velocity, the ice thickness etc. which matters. Generic scale modelling is essentially based on dimensionless parameters (Froude scaling, dimensional analysis). The Authors may cover this if they write . . .is one of the most important parameters which. . . rather than . . .is the most important parameter which. . ..

True, but most subsequent formulas are scaled by impact area water depth. Changed as suggested.

Further, it is unclear what the Authors try to say with the term scaling factor, as this term carries a clear meaning in physical modelling, which seems out of context here. The Authors may replace scaling factor with reference parameter (the water depth is one of the reference parameters in the dimensional analysis to derive the dimensionless parameters later used in Section 5.1).

This is better, changed as suggested.

P6478/L18: Waves traveling along the shore are expected to be considerable slower than direct waves and they are thus an important, but not the main contributor for the messy signal at the tidal gauge. The main reason may be (i) frequency dispersion (wave components may separate and overtake one another, however, whether this happens depends on the wave type, see e.g. Heller and Spinneken 2015) and (ii) reflections from the shoreline.

We now mention both possibilities and write
The recorded wave signal is a superposition of waves traveling along different paths

or with different speeds. A series of waves has been observed to follow the shore (east of glacier) with run-ups far exceeding 10 m*. Since these waves travel at considerably lower speed than the direct wave, the observed signal is likely due to reflections at the shore line or frequency dispersion (e.g. Heller and Spinneken, 2015).*

Gabl et al. (2015) conducted a similar study as presented by the Authors.

As the Gabl study was not published when this paper was written, we were not aware of it. Also, these authors are presenting modeling results, whereas we analyze field data, so it's not clear why this is "similar". We now cite Gabl, but do not reference any of their results.

Section 5.2 (1st paragraph): It is not fully clear if this sensitivity analysis is conducted by keeping all other parameters constant or not. On L5 it is written . . .all other quantities equal. . ., but does this apply to the later sentences (water depth, slide thickness) as well? This needs to be communicated clearer.

p 6482, L5: We think that this sentence is unambiguous, we just vary front height. We agree that the following cases should be written more carefully. We now added a qualifying statement
For example, with a depth of the impact zone of $h_0 = 100$ m *instead of the current* $20 - 40$ m *(leaving all other quantities equal), the maximum wave amplitude will decrease by* 20 %*, while …*

Technical corrections:

P6472/L24: The YouTube link for the video is incorrect, it refers to a video from 2010.

Yes, it is from 2010, and it illustrates the serious hazards that iceberg-related tsunamis have for the Greenland population.
We mention it here to give some wider context of the practical importance of the phenomenon.

P6474: Some specifications of the measurement accuracy of the terrestrial radar interferometer and the tide gauge should be added.

We added such information. For the GPRI:
The so derived elevations differ from the GIMP digital elevation model (GIMPDEM; Howat et al., 2014) on stable terrain by less than 5 m on slightly inclined areas, but can exceed 20 m on steep terrain.
and for the tide gauge:
… with an accuracy of ±2.5 mbar*, or* ±2.5 cm *water level.*

P6475/L7: Consider replacing slide with ice mass.

changed as suggested

P6476/L4: There is good evidence of all selected parameters in Eq. (1) apart from the friction coefficient f, and it would be good to mention why 0.1-0.2 for f was selected.

We added the sentence and reference:
This choice of the friction parameter is motivated by studies of dynamic friction of ice on ice at high temperature and speed (Schulson and Fortt, 2012).

Eq. (2): The parameter ac should be defined.

We now specify
This is equal to the theoretical speed of a solitary wave with wave crest amplitude (height above undisturbed sea level) a_c *in water of* $h = 110$ m *depth ...*

P6479/L26: Again, it is unclear what the term scaling parameter specifies. Please revise.

as suggested above, we replaced this with *reference parameter*

Eq. (9): The term describing the wave propagation angle is absent. Which angle was selected? This angle may considerable change the wave height as landslide-tsunamis show a different height in different propagation directions.

We now added the angle term

$$\cos^2\left(\frac{2\gamma}{3}\right)$$

which influence is still quite moderate. At a reasonable angle of $\gamma = 30°$ the wave height is reduced by 11 %.
The sentence now reads
...for the direction angle $\gamma = 0$ *of the tide gauge with respect to the wave source. Using an angle of* $30°$ *reduces wave amplitudes by* 11 % *to* $H = 4.2 - 5.1$ m. *These wave heights are again in good agreement with the* 5.5 m *(maximum to minimum) observed at the tide gauge.*

P6481/L11: Some research in recent years looked into the wave types in different water body geometries and while the 2D study Heller and Hager (2011) may still give a good estimate in the slide impact zone, it would be better to apply a 3D study, such as Heller and Spinneken (2015), to quantify the wave type in the 3D configuration of the present case. It is better understood in the meantime that wave types in 3D tend to be less nonlinear than it 2D.

That study was not yet published when this paper was written.

The source area is in between a 2D and a 3D geometry since the left side (east) is constrained by a slightly protruding ice wall. So, we tend to think that a 2D treatment of the source region is a pretty good approximation of reality. Evidently, the wave propagation in the embayment is 3D and should be treated as such.

The wave type of the observed event is very nonlinear and clearly bore-like. As the wave propagates into deeper water and spreads circularly, the wave energy is more evenly distributed, and the waves hitting the opposite shore have changed to cnoidal or Stokes-like waves.

Referee 3

GENERAL COMMENTS:

In my view the main contribution of this work is its potential to extend the validity of empirically derived formulas for small scales of tsunami wave properties and show how these formulas hold or do not, when not in idealized situation or outside of the experimental parameter regime. In case some of these formulas hold, it would be possible and useful to discuss potential scenarios for these waves, if however these potential scenarios were well justified to begin with. The authors refer to likely future scenarios but do not provide any basis for their inferences, for example supporting evidence from a numerical model of future possible scenarios section 5.2 is highly speculative.

The empirical formulas agree well with the observational data. What we do in section 5.2 is a very simple parameter study for extreme case scenarios to obtain an estimate on range of possible future tsunamis, which is of high practical interest (boat landing). It would only be speculative if we argued that any of these scenarios will happen in the future, which we don't.
Very likely the glacier will thin in the terminus area, and then retreat into deeper water, but that is speculative, and hence we don't discuss this. We currently have a funded project to work on the calving behavior and the long-term evolution of this glacier. Only such work will provide a well-founded basis for the estimation of future tsunami danger.
Since we don't discuss any of the points suggested in this comment, we don't see any need to change the manuscript.

The authors do not provide good scientific evidence that large tsunami waves are a new recent phenomenon. Their argumentation relies mainly on discussion with local people and speculation (e.g. page 6479) . While the documented event is a valuable new observation which can be used to validate empirically derived formulas from laboratory scale experiments, this unique but single data point cannot be used to make inferences about frequency of such events or their novelty on the scale of a century (e.g. section 5, page 6479).

As we argue in the paper, the phenomenon on this scale is new. As explained on page 6479, very old vegetation is currently being eroded (also Fig. 7) which is the clearest objective evidence that such high tsunami waves were not impacting the coast for centuries. In addition, an analysis of the century-long documented history of this glacier shows, that terminus cliff heights never attained anything close to the current 200 m (Lüthi et al., 2016)

And to reply to the more general point: how can discussion with local people be "no evidence" but speculation? People living off the land have a very good sense of the processes, observe long-term changes, and are often better observers than most scientists. (nothing changed)

Further, there is excessive emphasis throughout the manuscript on the discussion of the damage caused, which does not contribute to any better arguments or it does not help to answer any scientific question.

Excessive? We mention the destruction of the boat landing in the "Study Site" and at the beginning of the "Discussion" section, mainly to argue that the observed high tsunami waves are a novel phenomenon.
It is not clear what the referee is criticizing here. How does practical relevance devalue a scientific study? (nothing changed)

SPECIFIC COMMENTS:

6472-Line 10-14: These lines seems to indicate that it is concluded from observations of a single event, that there is a long term trend.

We do not claim that there is a long-term trend, and we cannot, as there is no long-term data set. But this type of event with calving tsunami waves that run up 10-15 m on the shore is a phenomenon that started in 2012. We know this from the local tour operators, who built infrastructure that was destroyed in 2013. This described event is not unique, and it was not the biggest one. Similar events happen roughly once a week during summer.

We now clarified this at the beginning of the "Results" section
Collapses of the 200 m *high glacier front which lead to big tsunami waves at the opposite shore have been observed since 2012, and happen roughly every week during the summer. One such event happened on 2 July 2014 when a tour boat was in proximity of the glacier terminus. The ice front collapse and the ensuing tsunami wave were filmed by several passengers on the tour boat (video on Youtube, https://www.youtube.com/watch?v=Cxd-jA0_QIM).*

It would be useful to mention whether the studied glacier has melange or not and how that varies seasonally, since that could have potentially some influence on the waves.

Yes, the glacier has frozen mélange in winter, which gets flushed out at the beginning of the melt season. So this does not affect calving and is therefore not mentioned (but see Lüthi et al. (2016)).

Youtube links didn't work at some point, so I would suggest citing a more permanent and reliable resource.

This might be copyright infringement. We tried to contact the Youtube film authors, without success (but, of course, we made local copies which we could publish, should the original version become unavailable).

6473-Line 7: The referenced article of Luethi et al. 2009 does not contain any information about the size of the calving waves at Jakobshavn, but in the manuscript it says they are of order of tens of meters, how was it determined?

The documented iceberg in figure 3 was rocking up and down with an initial amplitude of 50 m and for several cycles with amplitudes of 10 m. Obviously such a process creates waves of similar magnitude.

What is the difference between the impulse wave and the tsunami wave? When generated the wave is referred to as impulse wave, when it arrives at the shore it is referred to as tsunami wave, but in my understanding it is the same wave I find this terminology change through the paper confusing and it seems that one word should be sufficient.

This was not clear to us, since the literature on laboratory experiments refers to "impulse waves". According to the suggestion of referee 2 we now consistently use the the term "tsunami".

6476-Line 4: How is the friction coefficient determined? One could say that in stead of having two independent estimates of the wave velocity, in reality, the seconds estimate really serves to fit the parameter f of the first method.

We now reference studies of dynamic friction of ice on ice at high temperature and speed (Schulson and Fortt, 2012).

6476-Line 25: What is the error on the radar measurement? It was mentioned that the radar samples once a minute, therefore an estimate of 117 minutes traveling time of a wave can have quite large error when estimating other quantities later on. This should be clarified.

We now give an accuracy estimate of the radar-derived DEM.
The rest of the comment is not clear. We do not use the radar to determine wave speed directly. The only observation of the wave with the radar is the position of the wave at the boat, which we know very accurately from the precise timing of the radar measurement (using GPS time) and the known angular frequency of the radar motion. The radar pixel spacing in range is 0.75 m. Together with some signal refraction and small changes in signal velocity due to atmospheric pressure and temperature, the accuracy of the radar position is better than 10 m.

6478-Line 13: Were there marks of the water reaching 10 vertical meters above the sea level, or was that measured with the pressure meter? If not, what reference did the eye whiteness have to be able to claim wave heights over 10 meters?

The tide gauge data is shown in Fig. 6. The pressure height there reached 3 m. On the shore, the water reaches up much higher, marking the maximum with wet rock, eroded vegetation, ice fragments and dead fish. We saw all of these traces after each similar event, and measured the elevations above sea level in several cases. Depending on shore geometry the tsunami wave heights can vary substantially. For example, 1 km south of our tide gauge the tsunami wave are hitting a beach and have eroded vegetation some 30 m above sea level.

6479 Line 20: How do you support what likely future scenarios are?

We don't. Since we have a good match of empirical formulas with observations, we can do a rough estimate of the worst-case future danger potential assuming unlikely extreme cases. These are not scenarios, and we don't claim they are.

6481 equation 9: what is h_0 here? 20-30 or 110 m? How do such large variations in depth influence this formula is unclear, so the good agreement might be just a coincidence.

Throughout the paper $h_o = 20-40$ m, as defined on p 6478, l 4 (in the TCD paper). The rest of the comment is unclear. This is one of the empirical formulas taken from literature, but it is unclear what should be coincidental. (nothing changed)

Conclusions are incorrect, it is said that all observed quantities agreed well, but on page 6478 in section 4.6 none of the observed wave periods agree with experiments.

We now specifically repeat this
... except for the observed wave period which likely is a superposition of dispersed or reflected waves.

Scaling issues (mentioned in conclusions) should be addressed in more detail.

This is unclear. We cannot judge these issues due to lack of reproducable data. Probably the only way to really investigate such issues would be to replicate real events in the laboratory. (nothing changed)

It is not clear whether all the applied equations are valid for this 3D wave, for example Heller and Hager 2010 experiments are done in a channel.

This is a good point that was already partly addressed in the original submission, and has been extended to include the wave decay equations of Heller and Spinneken (2015).

A big part of this paper is due to a video made by the tourists, why are the tourists not thanked in the acknowledgments?

We now acknowledge the video authors for publishing their videos on Youtube.

TECHNICAL COMMENTS/SUGGESTIONS:

Keep consistency in giving range 2-3 vs 2...3 keeps varying through out the manuscript, e.g. in 6475-Line 29

We now use the minus sign throughout the manuscript.

6473-Line 26: what was the height of the ice front before the acceleration? Later you refer to 200m, not to 150-200m

The height is laterally varying between 150-200 m. The event we describe happened at the 200 m high face (nothing changed).

6474-Line 28: 15m is maximum observed ever or during this documented event? In case of the first, how is that measured or what is the reference?

These heights are clearly marked with traces on the shore: wet rock, eroded vegetation, ice fragments and dead fish.

6474-Line 28: Why is that relevant for this article that the boat landing was destroyed? It doesnt add any value to the findings about the physical mechanism.

As explained above, the destruction of the boat landing in 2013 is good evidence that such violent tsunamis never happened before, something the referee seems to challenge.

6474-Line 18-20: Is this description providing information relevant to data analysis, e.g. that the sensor was not moving and therefore the data is cleaner? If that is so, it maybe useful to emphasize that.

It describes how sensor was protected from the harsh environment, namely with a steel pipe hanging from a steel cable. This implies that the sensor might move during the event. Since water level before and after the event are the same, we assume that only little motion happened.
We tried in similar environments mounting pressure sensors within fixed steel pipes, and with other methods, but they were usually destroyed by the action of icebergs and sea ice (mélange).

6475-Line 1: 200 or 150-200m high?

The collapsed cliff was 200 m high (see Figures 3 and 4). Further north it was less high, therefore the range in the general description (nothing changed).

6475-Line 19: Was the 100 m change in vertical on average or was that the maximum?

Figures 3 and 4 show that it was 100 m over most of the area.

6475-Line 18: What is s here?

The value s is the surface-parallel slide thickness, as used on page 6477. We now explicitly name it here
... the ice thickness changed by about 100 m *in the vertical, which corresponds to a surface-parallel ice slab thickness of* $s = 50$ m.

6475-Line 20: Repetitive: details about the alternative names of boat landing are men- tioned in discussion.

Since this location is known under different names, we indicate both. (nothing changed).

6477-Equation 2: What is a_c here? Height of wave crest? What is its assumed value to solve for h?

Yes, we now specify this
This is equal to the theoretical speed of a solitary wave with wave crest amplitude (height above undisturbed sea level) a_c *in water of* $h = 110$ m *depth*

6477-Line 13: is or can be?

We now write *is*

6476-Line 25: precise is not the same as accurate

changed to *accurate*.

6476-Line 20: unstable in stead of instable

We now use "unstable" everywhere, even though "instable" seems to be a valid form as well (which it should, being derived from Latin;
`http://www.merriam-webster.com/dictionary/unstable`).

Referee 4

The authors present a fairly well-written paper which provides interesting results derived from a diverse data set collected with a Terrestrial Radar Interferometer (TRI), tide gauge, and video. The authors were very fortunate that there was a boat in the area and a calving event during their study period. The paper is worth publishing. However, there are a few suggestions that can be made to possibly improve the manuscript in addition to fixing some minor typographical and grammatical errors.

Some general comments: Considering that the TRI can also measure ice velocity, did the authors observe any velocity changes before and after calving?

There are some very local changes in velocity. Due to the noisiness of the data, most likely caused by the atmospheric inhomogeneity and turbulence (~5 km line of sight) this is very difficult to quantify on the very short time scales (minutes) of the observations. There were definitively no such big jumps as had been observed at Jakobshavn Isbræ.

It would have also been nice to see a more thorough description of the TRI DEM processing. What software did the authors use to process these TRI data? Were the DEM interferograms filtered? Do you have any estimates of the elevation uncertainties in the final DEMs?

The DEMs were produced with unfiltered interferograms. We now specify the software (from Gamma) and the elevation uncertainties in the Methods Section (3.1).

Did the authors see any parts of the wave (from 14:07 UTC) during the next minutes (14:08 UTC) TRI scan? If yes, would it be possible to try to get another estimate of wave speed by looking at how far the wave traveled between the two images?

There are clearly waves visible, but unfortunately the wave heights are not big enough to create an unambiguous signal. It is also not possible to determine to which wave crest (first, second...) the discernible wave fronts belong. Also, the radar signal was not recorded for nearer part of the bay. (nothing changed)

The future danger potential section seems somewhat tangential to the rest of the manuscript, and can probably be summarized in a sentence or two in another section.

We think that this is a quite important aspect of the whole study, and of high practical interest. It also exemplifies how the empirical formulas can be applied to a real-world problem. So we think that leaving away or condensing this section would reduce the findings of the paper. (nothing changed).

The Youtube videos should probably be hosted as backup elsewhere (and the authors will likely need to ask the people that uploaded the videos for permission).

This is a good point, but it is difficult to contact Youtube authors, and we did not receive any answer. Of course we made copies of the movies, but since they are copyrighted we cannot publish them.

Another minor point is the use of . . . to describe ranges of measurements throughout the manuscript. This seems to be very confusing. Could this be a typesetting error?

This was intentional, because we found minus signs quite confusing. Given the comments by all referees, we now use minus signs throughout the manuscript.

Some line comments:

6473 L25: The authors state that the glacier velocity was 14 m/d in 2014. Was this measured with the TRI?

Yes, this is now published in Lüthi et al. (2016).

6475 L21: How is the volume uncertainty calculated?

This is calculated from the estimated uncertainty of individual thickness measurements, which are assumed to be accurate to ± 10 m.

6476 Eqn. 1: Maybe mention what g is, just to be clear.

Added as recommended. We defined it after Equation (3), but moved it now to Equation (1).

6477 L5: Agrees with soundings, maybe elaborate on that just a little bit more (i.e., include a range).

The depth soundings show parts of the bed, but not all. Most of these are about $100-150\,\mathrm{m}$. We now write
This water depth estimate agrees well with depth soundings in the fjord which are mostly in the range $100-150\,\mathrm{m}$ *(Lüthi et al., 2016).*

6477 Eqn. 2: What is a_c?

This is now explained
This is equal to the theoretical speed of a solitary wave with wave crest amplitude (height above undisturbed sea level) a_c *in water of* $h = 110$ m *depth*

6478 L18-20: Some singular vs plural errors.

This section is reformulated now.

6479 L13-16: Maybe reword this a bit (or add the exact period when the glacier had a 50 m cliff). Right now, most of the paper states that the glacier cliff is 200 m, but the sentence reads that the glacier has attained cliffs up to 50 m high. Maybe say that the cliffs were usually 50 m, but became higher at some point in time.

We reworded this sentence, which now reads
Indeed, throughout the documented history of Eqip Sermia (since 1912) the maximum cliff height of the glacier was about 50 m *(Lüthi et al., 2016), from which smaller tsunamis were triggered.*

6480 Eqn. 5: What is m_s?

The slide mass. It is obvious from the context ($\rho_i V$), and not used anywhere else, so we leave away an explicit declaration (nothing changed).

Referee 5

The authors describe an odd calving event and nicely lay out some equations and scaling that future studies could use to better understand iceberg calving. It is a great contribution and it should be published after the authors consider the following minor grammatical corrections.

I have a few general issues. First, the authors do not explain how they obtain the DEM. The TRI gives you a line-of-sight reflections and the authors are definitive about the actual height of the cliffs, so how do they obtain the height of 200 m? Along these lines, Figures 3 and 4 seem to drive home this point. There is no thickness change of anything greater than 150 m (scale bar does not exceed 150 m) on Figure 4, while Figure 3 indicates a height of 200 m - the authors should reconcile these.

We now better explain the TRI data processing to obtain the DEMs. There seems to be a misunderstanding of Figures 3 and 4. Figure 3 shows the elevation profile along one line, whereas Figure 4 shows the differences of two DEMs acquired in a one-minute interval. The height of the cliff (200 m) can be read out of Figure 3.

6472/7: "in 800 m distance" seems awkward

we now write *at a distance of 800 m*

6472/9: "in 4 km distance" seems awkward

we now write *at a distance of 4 km*

6472/19: "which are threatening live" is poor grammar, maybe change to "which have the potential to threaten lives"

changed as suggested

6472/22: "in large distance" is poor grammar

changed to *at large distance*

6473/1-2: weird to define Glacier calving by using another reference to calving, edit this sentence

We leave away the second "calving" in this sentence.

6473/8: What unpublished data? Please specify what/where this is

We now write
unpublished pressure sensor data from different locations in the Kangia ice fjord.

6473/20: Where is the Luthi et al., 2015 paper?

It is accepted and about to be published in the Journal of Glaciology.

6473/27: "a new type" - how is this new? Also, "a new type of calving events" is poor grammar

We now leave away "a new type"

6475/15: How are the DEMs obtained? In my understanding of the TRI, it requires some sort of correction to obtain an actual DEM rather than just a line-of-sight measurement, so how is the TRI data processed to obtain a DEM?

The Gamma GPRI has two receiving antennas. The phase differences are used to determine the topographic phase, from which DEMs can be obtained (see Strozzi (2012) for details). The description in section 3.1 has been extended.

6476/14: Define celerity

This terminus is used in the relevant literature on impulse waves, so we use it here. This seems to be a synonym for the velocity of the leading wave. We now specify in section 4.3
The average celerity (wave crest velocity) of the tsunami wave

6476/24: How does the tide gauge keep time? Internal clock?

These are autonomous data loggers with internal real-time clock. We record the exact time of deployment and recovery to correct for offsets from UTC.

6478/19: "while other travel" should be plural "others"

changed as suggested

6479/4: Where is Eqe Lagoon?

Just adjacent to the Eqip Sermia glacier. We now specify
... (located south of the glacier terminus).

6479/9: "shores is eroded" should be "shores are eroded"

The sentence is "Vegetation ... is eroded" (nothing changed)

6479/15: change "with" to "which"

good catch, the sentence is now reformulated.

6481/4: "in a distance" poor English

we now use *at a distance*

6482/28: "in 3 km distance"

we now write *at a distance of 3 km*

Figure 7 caption: Remove "important"

changed as suggested

References

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