Dear reviewer,

Thanks for the thorough and constructive comments to our submission. Integration of these suggestions will definitely help to further improve our manuscript. In the following we will address each of the comments separately (author replies in blue).

Dear Editor and authors, the work by Brill et al. presents an insight into the Typhoon Haiyan's sedimentary record in coastal environments of the Philippines and its palaeotempestological implications. I commend the authors on a well-written and interesting manuscript. The authors addressed a topic with particular societal relevance due to the consequences of these catastrophic events for coastal areas. In this case, it is particularly relevant to say that the authors conducted extensive fieldwork and were also able to complement that with results derived from the application of some sedimentological proxies (grain-size, XRD and magnetic susceptibility). The data set gathered seems to be solid and very interesting from a scientific point of view. Overall, the manuscript has a clear structure and aims. However, in my opinion, several aspects should be addressed by the authors before the manuscript is accepted for publication, please see details below.

Although most of the issues I raise (please see below) are minor, I would like to stress that the authors need to be more consistent in terms of the vertical datum that they used. They should rewrite some of the numbered lists and make the text easier to follow.

All height information will be presented in meters above mean sea level in the revised manuscript version. The respective sections will be rewritten for clarification (see also replies to specific comments below for more details).

They need to address more clearly the differences between tsunami and storm deposits and they should discuss transport modes and its implications for the depositional signature of the Typhoon Haiyan's sedimentary record in coastal environments of the Philippines (Jaffe et al., 2012 - Sed Geol).

We will add some more explanations concerning potential features that may allow to differentiate between tsunami and storm deposits in the discussion section of the revised version. We will provide sufficient details on potential tsunami and cyclone indicators (and the problematic that all of them are dependent on local setting) to allow the reader to follow our argumentation. However, in our opinion this discussion has already occurred in a large number of publications, and we therefore will also refer to the respective literature for more information (we will provide more references dealing with this topic) instead of going too much into detail.

We also will enlarge the discussion of transport modes for the different storm signatures reported. In agreement with observations on the deposits of other tropical cyclones (e.g. Williams, 2009 at the US coast), we mainly attribute the formation of the slightly normally graded sand layers to settling from suspension during an early stage of the storm surge (at least at sites HER and TOL this initial flooding of the back-barrier areas was supported by infra-gravity waves). On the other hand, the laminated washover fans are interpreted to be the result of bedload transport over the coastal barrier, related to distinct storm waves during a later stage of the storm surge.

On top of this, they should stress that although local settings and sediment source are fundamental aspects that control storm deposit bed formation, there are a group of common features between the studied sites and that they share characteristics with deposits elsewhere (maybe adding a table summarizing these sedimentological features would help to the reader).

We will highlight this aspect in both the discussion section and the conclusions. This will also include a table listing the characteristics of the different storm features (including comparison with features of other storms and tsunamis) as suggested by the reviewer.

Abstract

The abstract is clear and well written. However, the authors need to clarify if they are studying 3 or 4 sites (they mention 4 sites here but mention 3 sites on page 3 line 10).

We will align the information on the number of locations in the manuscript to 4 sites: Hernani (Samar), Tacloban (Leyte), Carbin/Molocaboc (Northern Negros), and Bantayan.

I also suggest that the authors need to provide more comparisons with palaeotsunami data to sustain their sentence in line 20. In my opinion, that sentence should end "(...) typhoon signatures that can be used for palaeotempestological studies." the rest of the sentence should be deleted unless discussion is enriched with further topics on the comparison between tsunami and storm depositional signatures.

We will slightly enlarge the discussion of tsunami signatures in the revised version of the manuscript (see reply to comment above) and, thus, will leave the sentence as is.

Introduction

Page 2, line 18, once we started talking about using geological record for several millennia we should also mention (and take in consideration) sea-level changes especially when we are using just a few specific study sites.

We absolutely agree with the reviewer that sea-level changes (and changes in palaeogeography) since the time prehistoric tsunamis or storms made landfall have to be considered when interpreting their geological records. We will add a short section to highlight the importance of this aspect.

Page 2, line 25 - "natura are" - spelling mistake

Will be corrected.

Page 2, line 27 - suggest you delete text up to line 30... "Here, we report..."

We will delete the part of the mentioned section referring to a previous study on coastal boulders from the same area (May et al., 2015). However, we think the sentence concerning the potential relevance of the presented data for a general discrimination of tsunami and storm deposits is necessary and should not be deleted, since we believe that sediments of exceptional typhoons such as Haiyan definitely contribute to this discussion.

Page 3 - I believe you should clarify or stress again the aims of your work, in particular at the end of the Introduction.

We will stress the aims of our study at the end of the introduction to make this aspect clearer for the reader. The revised section should read: "The major aims of this study are to (i) document Haiyan's impact on the sedimentology and geomorphology of heavily affected coastal areas by recording onshore and intertidal sedimentation, coastal erosion and geomorphological changes. Based on these data, sedimentary and geomorphological typhoon signatures typical for the study area shall be established. In addition, (ii) the spatial variability of these typhoon signatures due to site-specific characteristics such as the local topography, bathymetry, geology, and hydrodynamics as well as the exposure to the typhoon track shall be investigated. Finally, (iii) the potential of these modern typhoon deposits will be evaluated in respect of possible implications for the identification of prehistoric cyclones in the geological record."

Study area

Page 3, line 10 - "three study areas"???

We will change to "four" to be in accordance with the information given earlier (see reply to first comment).

Page 3, line 28 - when you refer to Samar please make reference to Figure or provide some clues about the specific location.

We will add a reference to Figure 1a, where the track of Typhoon Haiyan as well as the location of Samar are indicated.

Page 4, line 5 - "three distinctive wave pulses" - Three sets of waves? How was this established? Was it measured? What was the Hs difference between the different pulses? Where any of these pulses related with infra-gravity waves?

These three pulses of flooding with periods much longer than those of wind waves are based on eye-witness observations, so their heights (or the differences of heights between the pulses) are not well constrained. As mentioned on page 4, line 4, numerical models suggest that they may be the result of seiches (i.e. standing waves) in the semi-enclosed San Pedro Bay (Mori et al., 2014).

Page 4, line 10 - i) and ii) and iii) - numbered lists were used intensively in this manuscript. I do not think they were used properly. Each numbered topic is very extensive and the reader is not guided properly. I suggest you rewrite all parts in the manuscript were you used numbered lists. Either you simplify the topics or you should write them as different sentences and start the sentences with "on the other hand" or "moreover" or etc.

We will change the structure of this section according to the reviewer's suggestions.

Page 4, line 11 - "model-predicted". Throughout the manuscript you mention several times this but provide no details about modeled data. I strongly suggest you do that! Which model was used? What was the source data? What equations were used to calculate Hs, etc? etc, etc?

There are a number of different models that have been used. The data presented by Bricker et a. (2014), Mori et al. (2014), Cuadra et al. (2014), May et al. (2015b), Roeber and Bricker (2015), Kennedy et al. (2016), and Soria et al. (2016) are all based on models with different specifications. Although we agree that knowledge of models and parameters is important to evaluate the model output, providing all specifications in the manuscript would be a lengthy description that in our opinion would rather distract the reader. Since we refer to the original literature wherever we mention modelled data, interested readers can easily consult these articles for further information.
While the detailed specifications of the models are in our opinion not required to understand the presented data, there are two main types of models that have been used to predict flooding levels, and which to discriminate is indeed important for the interpretation of our data: (1) numerical storm-surge models combined with phase-averaged wave models are routinely used to model surge heights for larger areas (Bricker et al., 2014; Cuadra et al., 2014; Mori et al., 2014; May et al., 2015; Soria et al., 2016); (2) numerical surge models with phase-resolved (boussinesq-type) wave models are required to reproduce the interactions of waves with the local topography, which may generate infra-gravity waves (Roeber and Bricker, 2015; Kennedy et al., 2016). So while we think it is sufficient to refer to the original literature for detailed model specifications, we will explicitly include the discrimination of phase-averaged and phase-resolved wave models in the revised version of the manuscript.

Page 4, line 13 - throughout the paper you refer to, at least 3, height (vertical datum) units (atl, msl, above mean low water and depth below surface). This makes it really hard for the reader. I strongly suggest you convert all to m above mean sea level!

We agree with the reviewer that the use of different height levels might be confusing for the reader. To allow for comparability between all sites, we will provide all height references for data presented in this research (topography, sediments, flood marks, etc.) in meters above or below mean sea level (above/below msl). However, since the same values relative to mean sea level may – depending on the elevation of the ground – have completely different implications for sedimentation, we will also provide the flow depth in meters above ground level in case of the measured flood marks. For describing the stratigraphies of sediment profiles, we will stick to meters below surface, since here a relation to sea level would be rather confusing. We will state this information explicitly in the methods section of the revised version.

Page 4, line 24 - please provide reference after "Philippine plate".

We will add Rangin et al. (1989) as a reference.

Page 4, line 29 - suggest you replace "originating" with "originated" and add "denser" to make the sentence ..."darker and denser minerals..."

The sentence will be changed accordingly.

Page 4, line 31 - Please see comment to page 4, line 10.

The structure of this sections will be changed according to the reviewer's suggestions.

Methods

Page 5, line 12 - "along-shore perpendicular transects". So, cross-shore? What was the space between consecutive profiles? Did you created a DEM?

Due to the limited time available at each study location during the survey, we measured only a single transect at most of the sites. Only on Carbin Reef (6 transects) and at BAN B (3 transects) several transects were measured (all of them are documented in the respective figures). Consequently, no DEMs were created as well.

Page 5, line 16 - heights - Please see comment to page 4, line 13.

As already mentioned in our reply to page 4, line 13, all heights will be provided in meters above mean sea level. Flow depth (so meters above ground surface) will be provided additionally in case of flood marks.

Page 5, line 21 - please replace "was" with "were".

Will be corrected.

Page 5, line 23 - this is a relevant aspect of the manuscript. Here, you suggest that in some locations you only used one core? Do you think this is enough for well supported interpretations? Especially, when later you refer to all local specific conditions and lateral variations of the deposit!!

We indeed analyzed only a single sediment core for site BAN A. For all other locations (HER, TOL, BAN B), several samples collected at different distances to the shoreline were analyzed. We agree that there are lateral variations in terms of granulometry and faunal composition at the individual sites, which are of course not covered by the single core at BAN A. However, at site BAN A the lateral extension of the deposit is only ~10-20 meters. We checked the lateral structure by means of trenches and the section sampled by BAN 4 is assumed to be

representative for the entire washover fan. Especially for the comparison of BAN 4 with other sites the lack of lateral data should be negligible, since the differences between sediments from different sites are much more pronounced. Although some limitations must be expected for granulometry and faunal composition that vary laterally, we therefore assume that the results of this single core (i) represent the typical sediment composition at BAN A quite well, and (ii) can already document the main differences compared to the other locations.

Page 6, line 13 - suggest you compare your approach with Quintela et al. (2016 – Quaternary International) methodology to identify allochthonous Foraminifera species within high energy deposits.

The methodological aspects of foraminifer determination, counting and taphonomy classification used in our study should be similar to those applied by Quintela et al. (2016). Particularly the argument that higher percentages of broken foraminifer tests are the result of high grain density in the traction-transport dominated parts of high energy flows may be of importance for discussing our foraminifer assemblages, and will be considered in the discussion section of the revised version.

Results

Page 6, lines 19, 26, 27, 28 - heights - Please see comment to page 4, line 13.

All heights will be provided in meters above mean sea level and (additionally) as flow depth above surface.

Page 6, line 23 - please refer to Figure 2 (?).

A reference to figure 2 will be added.

Page 7, line 3 - I believe you should provide/describe more grain-size data information. I suggest you add information on the D10, D90, sorting and unimodal or bimodal character of your samples.

We will complement the grain-size information for all sites, and provide data on mean, sorting, and modality for each site.

Page 7, line 9 to 13 - I feel that in the discussion you should refer to the relationship between reworking and sediment concentration. Did you detected more reworked material in the basal sector of the storm layer or on the top? How was this correlated with grain-size?

In case of HER 10, no clear vertical trend in foraminifer taphonomy or species composition could be detected. There is rather a slight correlation between coarser grain size and stronger reworking (Fig. 4). This is similar for core BAN 4, where strong reworking correlates with larger grain size as well (Fig. 12). However, since sediments first tend to become coarser towards the top of BAN 4 and fine afterwards, reworking is highest in the central section of BAN 4.

Page 7, line 23 - again, the heights...what vertical datum did you used this time?

All heights are provided in meters above mean sea level and, in case of flood marks, (additionally) as flow depth above surface.

Page 8, line 2 - please refer to Figure.

We will insert a reference to figure 6.

Page 8, line 29 - I guess you should cite it as personal communication.

We will cite the observation as "personal communication".

Page 8, line 30 - heights - Please see comment to page 4, line 13.

The heights are provided in meters above/below mean sea level.

Page 9, line 16 - Rsubt was collected at approximately what depth?

The sample was collected at 0.5 m below mean sea level. We will add this information in the revised manuscript.

Page 10 - line 14 to 17 - the fact that the basal sector is slightly finer than the middle section is not just a consequence of the more erosive character of the initial stage of the event? The following phases benefited from a lowered coastal sector thus were capable of transporting coarser sediments farther inland.

The proposed mechanism is a very plausible explanation for the observed stratigraphical pattern at this location, because both units are assumed to be deposited by similar processes, i.e. wave swash overtopping the coastal barrier. We will briefly address this aspect in the discussion section of the revised version.

Page 11, line 16 to 19 – this just reflects the dominance of the original (2nd cycle) sediment source.

We agree that the mineralogy and geochemistry mainly indicate the differences between limestone and volcanic environments. We already address this topic in the discussion section (page 15, lines 15 ff in the original version of the manuscript).

Page 11, line 20 - I believe it is the first time you refer to principal components analysis, I suggest you refer to it in full.

The abbreviation PCA is already mentioned in the methods section. However, we agree that referring to it in full at this position might facilitate reading.

Line 12 - line 13 - this strongly suggests this area as the main sediment source.

That is how we interpret this data in the discussion section as well. To make this implications already clear in section 4.5, we will add a brief explanation.

Discussion

Page 12, line 27 - again the numbered list.

We will remove the numbering to facilitate reading.

Page 12, line 30 - "normally graded or massive layers of sand". This implies totally different sediment transport modes (suspended grading and traction). I believe you should add a sentence here to comment on this and discuss reasons for the differences observed.

This should read "normally graded to massive layers of sand". While slight normal grading could be detected for thicker layers (close to the coast) and especially those analyzed for vertical grain-size variations in the laboratory, the small thickness of the sand layers further inland did not allow for unambiguous identification of grading. We assume that even the thinner parts of the sand sheets might be normally graded. But since we cannot prove this (macroscopically their structure could be both massive and slightly graded), we prefer to describe them as "normally graded to massive".

Page 13, line 3 - I suggest you add references from one of the several works conducted by Donnelly et al. or Liu et al. in the eastern coast of the US.

We will add Donnelly et al. (2006) as a reference from the US coast.

Page 13, line 8 - I believe you should also mention infra-gravity waves.

Actually, the mentioned "long-wave phenomena" already include infra-gravity waves that can result e.g. from surf beat. To make this clearer, we will explicitly use the term "infra-gravity waves" at this position.

Page 13, line 13 - very very interesting but why? Can you add a comment on this?

The deposits described by Williams (2009) have actually a very similar structure as those described on the Philippines: a finer, graded sand layer formed during the initial inundation of the back-barrier plains, topped by washover deposits during a later stage of the storm surge. We therefore assume similar transport modes for our deposits, i.e. suspension settling for the graded sand sheet and bedload/traction for the formation of the washover lobes. The role of long-wave phenomena for the deposits presented in our study (infra-gravity waves at HER and seiches at TOL) is probably a contribution to higher and more extensive flood levels, but not significantly different sedimentation processes.

Page 13, line 16 - now it is important to know at what depth was your sample (Rsubt) retrieved!!

As mentioned before, the sample was collected at 0.5 m below mean sea level. It contrasts significantly in terms of faunal composition from the storm deposits, while the littoral reference samples from BAN reveal a similar granulometry and faunal composition with the typhoon deposits.

Page 13, line 25 to 29 - I suggest you rewrite this sentence.

We will change the structure of this section to make it clearer for the reader.

Page 14 - line 1 - you must refer, for example, to the work of Komar and Wang (1984) or Komar (in Mange, 2007).

We will add Komar and Wang (1984) as a reference for density sorting.

Page 14, line 6 to 9 - Agree with interpretation.

Page 14, line 18 to 20 - I accept your interpretation but I think formation of ridges implies a "continuum in time" more suitable with normal storm regime and a succession of events.

We agree that ridges might form during several successive events rather than single storms. In fact, we state the possibility of ridge formation by several typhoons (with significant growth of a pre-existing ridge during Haiyan) further down in this section.

Page 14, line 21 - please see comment to page 4, line 10.

We will remove the numbering.

Page 14, line 27 and 28 - I think this partially contradicts statements above. I suggest you rewrite it

Since our evidence is not unambiguous without robust age data, we have to provide both possible explanations for the ridge formation. Of course these explanations partially contradict each other, because only one of them can reflect reality. We will rewrite the section to clarify this aspect.

Page 15, line 3 - please quantify the "remarkable amplification".

In the central part of the bay, water levels of more than 8 m above sea level were recorded, which is much higher than in Haiyan-affected areas not subject to infra-gravity waves (HER) or raised water levels related to shore configuration (TOL). We will add the value in the revised manuscript.

Page 15, line 4 - which models?

Here we refer to storm surge models combined with phase-averaged wave models (for details we refer to the original reference by Bricker et al., 2014) that do not account for the effects of infra-gravity waves (phase-resolved wave models). We will add this information in the revised version.

Page 15, line 8 to 14 - Reasoning perfectly reasonable

Page 15, line 15 - in fact, you can add that sediment source is always a decisive factor.

We explicitly will add sediment source as a further decisive factor.

Page 15, line 24 to 27 - please rewrite this sentence.

The sentence will be rewritten.

Page 15, line 28 – is backwash really relevant for depositional imprints in storm events? Against gravity?

Usually backwash is probably of minor or no importance during storms. However, sample HER 10 was collected close to a fluvial channel, where the backwash was not against gravity.

Page 16 - line 4 to 7 - here, you acknowledge that site-specific limits extrapolations of your conclusions. I agree and it really is hard to overcome this but, in my opinion, this field of science will progress will a multitude of sites, settings and events being studied. Maybe you can add a sentence regarding future work.

We agree that the value of case studies is their contribution to the database of locally and regionally differences of storm and tsunami deposits. We already tried to address this aspect later in this section. However, we will modify our statement to highlight this message.

Page 16, line 20 - you need to add a comment on the different settings studied by Hawkes et al. (2007) and Goto et al. (2011).

The settings mentioned here are wide coastal plains or beach-ridge plains with a low topography that does not hinder lateral inundation and sediment transport due to steep slopes. Indeed, this is not true for the sites investigated by Hawkes et al. (2007), so we replaced the reference with observations on 2004 Tsunami deposits from a beach-ridge plain in Thailand by Jankaew et al. (2008). While most of the sites presented here have a steeper topography and are therefore not directly comparable, similar conditions are given at TOL. Nevertheless, in spite of high surge levels >5m, sand transport is limited to not more than ~300 m (although the topography of the coastal plain would allow for much more extensive deposition).

Page 16, line 20 to 27 - your conclusions are somewhat constrained because you did not compared tsunami and storm deposits in the same locations (e.g. Kortekaas and Dawson, 2007).

We agree that the conclusions are limited due to this fact. However, by comparing with tsunami deposits from sites with similar settings (similar flood levels and similar topography), we think our findings nevertheless add to the observation that sediment extent tends to be a discriminative feature.

Page 16, line 28 to 31 - 2 units by one event is totally different from 2 units by more than one!! You need to discuss this!!

We agree that both interpretations would have completely different implications. But since we are not able to prove one of the two possibilities (the formation of the washover fans at TOL could neither be proved by eyewitnesses nor by satellite images), we have to present both options for this location.

Conclusions - Page 17, line 2 - "local factors"... After so much work, it is important to stress the relevance of local conditions. In fact, I suggest you provide a geomorphological sketch (conceptual) model that describes accurately the initial pre-event conditions and the deposit after the event.

The idea to present the main outcomes of the study in a conceptual figure is indeed reasonable. We decided to merge this figure with the table summarizing the characteristics of the storm features presented in this paper (see reply to an earlier comment). This figure will include schematic sketches for the formation of each storm feature (coral ridge, sand sheet, washover fan). It will, however, not provide a separate figure on the pre-event situation. This is poorly constrained and, thus, cannot be documented with sufficient detail.

References

I suggest you add the above mentioned references.

The mentioned references will be included.

Figures

The figures have very good quality, are well-designed and are informative.

Supplementary material

Useful.

I believe that in scientific terms the authors developed quality work that clearly deserves publication in NHESS, subject to very few minor changes. Regards Pedro J. M. Costa