Authors’ reply to comments from the reviewers and the list of changes made in the revised manuscript NHESS-2015-190

We appreciate the reviewers’ comments and suggestions that served to improve the article. We do agree with most of them. Based on their comments, we made significant changes in the revised manuscript which are listed below.

Comments of the Reviewer #1

General Comments:

Only small particles (less than 10^{-4} *m) are considered and I assume that the same particle size is specified under the normal and typhoon conditions. Why is it so? Won’t the typhoon forcing lead to the presence of coarser sediments, not only to the increased sediment concentration? In terms of a coastal hazard, the intermittent layers of coarse and finer sediments represent a much greater danger for the submarine landslide.*

Many thanks for this point. We totally agree with you that the typhoon conditions will result in increase of concentration for both fine and coarse sediments. However, in this work we considered only fine sediments due to the following reasons. Firstly, under moderate forcing conditions coarse sediments are mainly deposited in the proximity of the river mouth, therefore potential submarine landslides caused by elevated deposit of coarse sediments will be induced at the area adjacent to the river mouth. In particular, granulometric analysis of water samples collected during our field work showed almost complete absence of sediment particles with diameter greater than 10^{-4} at the station situated about 500 km far from the Peinan river mouth. Secondly, the Peinan River is likely to form a hyperpycnal plume under typhoon conditions which transport the majority of inflowing coarse sediments [Milliman and Kao, 2005; Liu et al., 2006; 2012], while a significant share of incoming fine sediments is transported offshore by a hypopycnal plume [Chien at al., 2011] and as a result wide seafloor areas become potential areas of landslide generation [Talling et al., 2013]. A big number of works studied the first mechanism for different rivers of Taiwan [e.g., Dadson et al., 2005; Milliman and Kao, 2005; Milliman et al., 2007], however much less attention was paid to the second mechanism considered in our study.

In Section 2, the authors describe in details the Kuroshio Current and argue (rightly so) that it strongly affects coastal circulation in the study area and the Peinan river plume in particular. However, when it comes to model formulation, I don't see the Kuroshio Current to be represented in any form at the model open boundaries: only barotropic tides are mentioned on page 5166. What is the point of discussing the Kuroshio Current then?

Thank you for this important comment. In the presented model, we do represent the Kuroshio Current as a boundary condition along the southern boundary of the model domain, however, in a simplified form defined in the following way. Basing on observational data and numerical experiments (Yuan et al., 1998; Johns et al., 2001), we prescribed the surface velocity as linearly changing along the southern boundary of the domain from 0 m/s at the westernmost wet grid point to 0.4 m/s at the eastern grid point of the boundary. In vertical dimension we also used linearly decaying velocity from surface to bottom. Since the core of the Kuroshio Current is situated outside the model domain we can use this simplification. The corresponding clarification was added to the text at Section 4.1.

I am not convinced that the typhoon Marakot forcing conditions are correctly represented in this study: the inset in Figure 5e shows that the discharge rises and falls at the same temporal rate. This is not correct; even for mountain rivers the peaking discharge subsides to its normal level at a slower rate than it rises. The temporal evolution of the freshwater discharge should be asymmetric: faster increase and slower decrease. The authors should present observational data if they insist that they correctly represent the freshwater discharge evolution during the typhoon passage.

We agree with the reviewer that the typical river discharge under freshet conditions is strongly asymmetric and the decreasing rate is slower than the increasing. The discharge curve of the Peinan River during typhoon Morakot used in our article was adopted from Fig. 4 at Mirabito et al. (2012). This curve is not based on gauge measurements and was calculated by the discharge model described in Mirabito et al. (2012), but the model itself was validated against gauge measurements performed by Taiwan Water Resources Agency at two big rivers of Taiwan, namely, Zhuoshui and Gaoping, during typhoon Morakot. Gauge measurements and model simulations showed fast and almost uniform increase of discharges of the main Taiwan rivers (including the Peinan River) during 7-9 August 2009 succeeded by exponential decrease during 10-16 August 2009. In particular, river discharges sharply dropped during 10 August and then slowly decreased during more 5 days down to the seasonally averaged value (see also Figure 2 at [Jan et al., 2013]). As a result the discharge rate of the Peinan River is indeed asymmetric, and decrease rate of discharge is significantly slower than its increase rate during the whole decrease period except 10 August 2009. This feature is possibly caused by inhomogeneous precipitation distribution during passage of the typhoon Morakot over the Taiwan Island and by relatively small length and basin area of the Peinan River. The discussion about the discharge rate of the Peinan River during the typhoon Morakot was added to the Section 5.2 of the manuscript. Also we enlarged the size and improved the quality of the related inset in Figure 5, so the asymmetric shape of the discharge curve of the Peinan River now is more evident.

Model validation is entirely inadequate. So little information and details are given in section 5 and the corresponding Figure 4 that this part can be dropped altogether without any reduction of the paper quality. But it would be much better if the authors provided sufficient information. The surface salinity is much lower in the model than in observations on April 16 (look at the 30 isohaline). What about the vertical structure? It seems to me that the model fails to reproduce the observed level of vertical mixing in the plume. The observed and modelled plumes are quite different on 17 April too, except for the fact that they are both affected by the upwelling favorable wind (being shifted northward from the mouth). Most importantly, there is no validation for the sediment transport and deposition, the primary focus of this study.

Thank you for this important comment. We fully agree with the reviewer that our study will significantly benefit from the improvement of the presented validation and tried to make maximal use of the limited available data as a mean to validate the model. Some discrepancy between numerical results and in situ measurements observed at Figure 4 was caused by the following reasons. Firstly, we want to point out that daily surface salinity distributions used for validation were calculated basing on continuous field measurements lasted for 6-7 hours, while the model outputs presented on Fig. 4 are daily averaged salinity distributions. On the other hand, the Peinan river plume is characterized by high temporal variability caused by external forcing which reduces accuracy of the performed validation. Secondly, we used monthly averaged T-S data provided by Levitus Atlas (Antonov et al., 2010) for numerical modeling which also increases discrepancy between model output and observational data. According to your recommendations we added validation of vertical haline structure as well as horizontal and vertical sediment distribution within the Peinan plume against available field measurements. The new validation procedures also showed good level of agreement between the numerical modeling and in situ data which increased reliability of the article results.

Specific Comments:

The quality of figures needs to be improved. All geographic objects mentioned in the text should be shown on the map or in other figures; this is a common standard. For instance, Green Island, Taitung Canyon, meteorological station in the Fugang Fishery Harbor, the location of freshwater discharge measurements and the tide gauge station CG all need to be marked either in figure 1 or 3. The quality of Figure 2 is poor, it's hard to see anything there, in particular those two canyons mentioned on line 10, page 5162.

We agree with these points, Figures 1, 2 and 3 were improved according to your recommendations. Figures 1, 2, and 3 shows the region of interest at different spatial scales: the whole Taiwan Island (Fig. 1, ~400 km), region of numerical modelling (Fig. 2, ~100 km), and region of field work (Fig. 3, ~20 km). The Peinan River basin and the Kuroshio Current are shown on Figure 1. The Green Island, the Taitung Canyon, two smaller local canyons and a ridge between them are shown on Figure 2 basing on NOAA bottom topography with spatial resolution of one minute which was used in the model experiments. The description of the bathymetry of the study region given in Section 2.4 was modified to emphasize these points. Location of the CG tide-gauge station is also presented on Figure 2. Locations of the meteorological, gauge, and hydrological stations are shown on Figure 3.

The alongshore propagation … were about 16 and 3 km…" (line 12, p. 5163). Perhaps, the alongshore extension? What do the authors mean by "plume dissipation" (line 21, p. 5163)? Perhaps plume dispersal? What is "one nautical minute" (line 17, p.5164)? Perhaps, nautical mile (or simply "minute")?

Thank you for these specific comments, we adopted them.

The Mellor-Yamada turbulence closure scheme (line 25, p. 5164). Which level?

We used level 2.5 Mellor–Yamada turbulence closure scheme, this is clarified in the Section 4.1.

Mean velocities in eq. (1) and vertically averaged velocities in eq. (5): are they the same? If so, use the same terminology and notation (overbar), if not, then what does the "mean velocity" in eq. 1 mean?

In Eq. 1 U and V stand for the time averaged components of velocity in context of the Reynolds hypothesis. In Eq. 5 $\overline{U}_n$ denotes the vertically averaged normal component of the mean velocity U (or V) at the open boundary at the time moment t. This denotation is usual for a 2-D barotropic tidal model providing boundary conditions for a 3-D hydrodynamic model. The corresponding clarifications were added to the text in Section 4.1.

The Peinan plume cannot show any synoptic variability (it's too small for that) (line 11, p.5171). Perhaps it's mesoscale or even submesoscale variability?

In this sentence we spoke about temporal synoptic variability of the river plume, i.e., its quick response to the variable atmospheric forcing. We agree with the reviewer that spatial variability of the plume is indeed submesoscale (or mesoscale under elevated discharge conditions). The related clarification was added to the revised version of the manuscript.

*"Under moderate discharge conditions wind forcing and Coriolis force determine the alongshore spread of the Peinan plume". I am not sure what it means or whether it is correct. Alongshore velocity in the surface trapped plume is scaled as sqrt(g'*d) where g' is the reduced gravity and d is the plume thickness (this scale holds even for the geostrophic cross-shore balance). So it is the density anomaly! And of course mixing will reduce the density anomaly and ultimately the downstream propagation. As far as mixing goes, both wind stress and tidal mixing should be taken into consideration.*

We fully agree that the alongshore transport of a surface-advected river plume is typically in geostrophic balance, however, influenced by winds, tides, and local bathymetry. This is the case for the first model experiment with freshet discharge conditions and moderate wind forcing. The typhoon model experiment showed, on the opposite, that strong wind is the dominant driver of transport in the plume and it diminishes the alongshore transport. Thank you for this comment, we clarified this point in the text and rephrased the quoted sentence.

The authors should define quantities and their units shown in Fig. 7 (left panel). How is "relative possibility of formation of submarine landslides" defined?

Thank you for this comment. Figure 7 indeed lacks readability, however, it illustrates an important outcome of our article. On this figure we wanted to show three different properties of the seafloor within the study region which are related to formation of submarine landslides and gravity flows. Firstly, we showed the bottom topography gradient (right panel, in blue), i.e., slope angle distribution measured in degrees. Secondly, we showed distribution of downslope component of gravity force applied to the bottom sediment (left panel, in green). This parameter is calculated in the following way: $F = m \cdot \sin(\alpha)$, where m is the mass of sediment deposit to the considered unit seabed area and α is the local slope angle. We assume that this value is proportional to the rate of overpressure which cause a submarine landslide if exceeds a certain value which depend on local bottom friction. Therefore this parameter normalized by its maximal value was referred in the text as “relative possibility of formation of submarine landslides”. As far as this parameter was normalized it is measured in relative units. Finally, we showed paths of the turbidity flows potentially caused by submarine landslides within the study region (left panel, in red). Every point of the seafloor where a submarine landslide can be potentially generated “produces” a single path which propagates from this point in direction of maximal topography gradient. Saturation of color of a path corresponds to the saturation of color of its initial point, i.e., dark/light red paths illustrate more/less possible gravity flows. If two different paths (with different saturations of their colors) go through the same point we draw the darkest path. These paths illustrate distribution of risk of erosion by gravity flows which is also is measured in relative units. The description of the Figure 7 given in Section 6.2 was significantly refined in the revised version of the manuscript, the figure itself was also reworked to improve its clarity.

Overall, the manuscript will benefit from proof reading by a native English speaker.
Thank you for this point. The revised manuscript has undergone proofreading by an expert English speaker.

On behalf of all authors, Alexander Osadchiev

Comments of the Reviewer #2

General Comments:

The authors refer to hyperpycnal flow (HPF) as a common occurrence in this area, and indeed their discharge concentration (120 g/l) is well above the threshold for direct HPF (33-40 g/l), however their STRiPE model is based on the dynamics of a buoyant plume (if I understand it correctly). This would likely have implications for the observed deposit.

Many thanks for this important comment. Under typhoon conditions discharges of Taiwan rivers with high SSC indeed form HPF, which was particularly observed during the typhoon Morakot. However together with HPF these rivers also form buoyant surface-advected plumes [Chien at al., 2011; Jan et al., 2013], the role of the buoyant Peinan plume in sediment transport was addressed in our study. This clarification was added to the text at Section 6.1.

The authors use a freshet SSC of 4 g/l, which seems high. Perhaps including a figure of gauged SSC vs. Q would be useful, which brings me to my next point.

SSC concentrations prescribed for numerical experiments are based on gauge measurements performed at the Peinan River. Firstly, according to Fig. 1 at Milliman and Kao (2005) mean annual SSC in the Peinan River is equal to 5 g/l. Secondly, Taiwan rivers (including Peinan River) are characterized by significant variability of dependence between SSC and Q (Fig. 2 at Milliman and Kao (2005)). In particular, according to gauge data shown in Fig. 4 at Hwang (1982) SSC varies from 0.6 to 4.5 g/l at seven different water samples taken during 90-110 m^3/s discharge rates. We do agree with the reviewer that SSC of 4 g/l prescribed for discharge of 100 m^3/s is a relatively high value. However, in our study we wanted to consider upper limit of possible SSC values during freshet discharge conditions to compare the resulting sediment load with the huge sediment discharge which took place during the typhoon Morakot. Coefficients for the dependencies of SSC on Q used this study were calculated basing on SSC and Q data presented at Fig. 4 at Hwang (1982) for $Q < 500\ m^3/s$ and at Fig. 10 at Milliman and Kao (2005) for $Q > 500\ m^3/s$. The dependences used in our study represent relatively well SSC values at the discharge ranges used in the performed numerical experiments. To our opinion more precise study of the dependence between SSC and Q for the Peinan River is beyond the current article especially taking into consideration limited availability and scarcity of necessary gauge data.

Is a validation performed under quiescent conditions reasonable to use for either the freshet or the monsoon conditions? Flashy systems such as small-mountainous rivers often scale in unpredictable fashion when stochastic events occur.

Thank you for this comment. Firstly, validation of the model was performed for the moderate discharge conditions (about 20 m^3/s) which is only four times lower than the freshet discharge (80 m^3/s) prescribed in the first numerical experiment. To this end we assume that after performed validation the model is capable to correctly reproduce behavior of the Peinan plume during freshet periods. However, the Section 5.1 focused on model validation was significantly extended, we added validation of vertical thermohaline structure of the plume as well as horizontal and vertical sediment transport against in situ measurements. Secondly, we agree with the reviewer that monsoon conditions characterized by extremely strong wind forcing and discharge rate can result in unpredictable features of the river plume dynamics. Nevertheless, model validation for the monsoon conditions is limited due to lack of available in situ data collected within the area influenced by the Peinan plume during typhoon conditions as well as absence of optical satellite imagery of the buoyant plume caused by cloud obscuration of the study region during typhoons. Also we are not aware of any previous study focused on behavior of the buoyant Peinan plume under monsoon conditions. The numerical simulation during the typhoon Morakot showed reasonable dynamical and mixing behavior of the Peinan plume (comparing to plumes formed by other large Taiwan rivers during similar typhoon conditions) therefore we presumed that the model is reliable for this case. This clarification was added to the text at Section 5.

I recognize that this would be a different study altogether, but the lack of field validation of the ensuing deposits seems like an oversight. At the very least, comparing these results to other studies conducted on/near Taiwanese Rivers (of which there are many) would be beneficial.

We totally agree with the reviewer that our work will significantly benefit from validation of model results against in situ measurements of sediment deposits. Unfortunately the field data in our possession are indeed limited and we are not able to extend our observational campaign to collect sediment samples, especially from the deep sea floor at the study region. Instead we, firstly, performed validation of horizontal and vertical sediment transport against in situ measurements. Secodly, according to your suggestion, we tried to make use of the data presented in other works concerning fate of river borne sediments at the Taiwan coastal areas after typhoon events and the associated formation of submarine landslides. The analysis of these data and its comparison with our results was added to the Sections 5 and 6 of the revised manuscript.

Is 1300 kg/m3 a reasonable sediment density? My understanding is that 2650 kg/m3 is a more commonly used density. Forgive me if I have overlooked something in your calculation.
Many thanks for this comment. This sentence lacks the word “anomaly” as a result of a misprint. The correct meaning is that sediment density anomaly (i.e., difference between sediment density and river water density as specified in Eq. 9) was prescribed as 1300 kg/m^3, thus sediment density itself was prescribed as 2300 kg/m^3. This value was calculated basing on dry bulk density of sediments delivered by Taiwan rivers to the surrounding seas which was prescribed equal to 1600 kg/m^3 (Liu et al., 2008) and sediment porosity which was set equal to 0.3 (Jiang et al., 2006). This clarification was added to the text at Section 5.

Specific Comments:

Figures 2 and 3 could be readily combined.
Figures 2 and 3 have too different spatial scales to be combined in one figure. Figure 2 shows region of numerical modelling which spatial sizes are about 100 km × 100 km, while Figure 3 illustrates region of field work adjacent to the Peinan River mouth which spatial sizes are about 20 km × 15 km. If Figure 3 will be inserted into Figure 2 it will be hard to adequately represent location of the 14 hydrological station situated in proximity of the river mouth, their symbols will be either too small or merged with each other. However Figures 2 and 3 were reworked to improve their clarity.

I agree with reviewer #1 that the paper would benefit from an additional proofreading by a native english speaker.
Thank you for this point. The revised manuscript has undergone proofreading by an expert English speaker.

On behalf of all authors, Alexander Osadchiev

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