Interactive comment on “Revised earthquake sources along Manila Trench for tsunami hazard assessment in the South China Sea” by Qiang Qiu et al.

We thank Reviewer 1 for the constructive suggestions which have greatly improved the manuscript. In this revised version, we have addressed the questions and highlighted areas where those changes are made. Our point-by-point responses and changes to each comment are given below.

Overall comments

This paper offers a new set of tsunami scenarios for the Manila trench, devised using updated geometric, coupling and geological results and interpretations. The topic is certainly significant and of interest to readers of NHESS.

I suggest the paper will be suitable for publication following some reasonably straightforward revisions, in particular to better emphasise the inevitable uncertainties in tsunami scenario design. The authors already discuss these issues well in some parts of the paper - but in other parts they gloss over the difficulties. Currently, I think the paper implies that the newly proposed scenarios are “better” than previously published scenarios. I doubt this is justifiable, considering the huge uncertainties in key subduction zone parameters (particularly Mw-max) on the Manila trench. These uncertainties could overwhelm any improvements due to better characterisation of coupling, geometry, etc. Thus, notwithstanding the advances in this paper, it is very difficult to say with confidence that the scenarios in this paper are “necessarily better” than previous scenarios, in terms of how well they represent the tsunami hazard.

Let me stress that this reflects the fundamental difficulty of tsunami scenario design in general, in the face of large uncertainties around frequencies of large-magnitude earthquakes. I don’t think it is something that the authors can solve. However, I would like to see the discussion “softened” in various parts of the paper to better reflect these uncertainties. I also suggest they make some passing mention of probabilistic tsunami hazard assessment approaches, which offer a means to integrate uncertainties into the analysis (although this is also not straightforward).

Overall the paper is well written. I have suggested a number of grammatical corrections, but they will be straightforward to address.

Author’s response: We are grateful for your positive comments. Your suggestions about putting more emphasis on the large uncertainties in the tsunami scenario are very sensible and are well taken. As you mentioned that designing tsunami scenarios is fundamentally difficult in subduction zones where large uncertainties exist for the frequencies of large-magnitude earthquakes, this is especially true for the Manila

Subduction Zone where no large earthquake occurred historically. Moreover, the spatial and temporal coverage of observational data (GPS, Seismicity, etc) is relatively limited. We have added the uncertainty discussion in various parts of the revised version.

Author's change to manuscript: Please see the detailed changes in our responses to section-specific comments.

Section-specific, high-level comments

Abstract

This reads well. The study sounds interesting and relevant.

Introduction

This generally reads well and provides appropriate background for the study.

Near the end, where you discuss the use of geodetic coupling to constrain the rupture, I would suggest giving some mention of potential problems with using coupling maps to represent future slip. For instance, there is evidence in Alaska that a "currently uncoupled" part of the megathrust may regularly produce tsunamis (Witter et al., 2016). The manuscript discusses some of the challenges with coupling maps later on – so alternatively, you could make this point then.

Author's response: as suggested by Reviewer 1, we have pointed the limits of using geodetic coupling to constrain the rupture in Section 3 before proposing the slip deficit models.

Author's change to manuscript: we added "... and in some cases uncoupled parts of the megathrust may regularly produce tsunamis (Witter et al., 2016)." at lines 316-317.

Somewhere in the paper (perhaps in the introduction), it would be good to mention probabilistic approaches to tsunami hazard assessment (e.g. Grezio et al., 2017; Li et al., 2016), as a contrast to the scenario-based approach in this paper. The main advantage of probabilistic approaches is that they offer a framework within which the uncertainties can be accounted for (e.g. Mw-max). The downside is that they are much more complex to understand and implement than scenario approaches.

Author's response: We have modified the text accordingly in the introduction part and mentioned the difference between the two main tsunami hazard assessment approaches.

Author's change to manuscript: The text is added in lines 137-141: "Scenario-based rupture models are different with the probabilistic-based tsunami hazard assessments within which hundreds and thousands are implemented for rupture uncertainty estimates. Therefore, the probabilistic approaches (Li et al. 2016; Grezio et al. 2017) are often more complex to understand and implement than the scenario-based approaches."

Section 2.

In my opinion this gives an OK justification for the segmentation. However, it is (unavoidably) far from certain that this is the best way to represent things. That's ok, but make sure the manuscript gives appropriate qualification.

Author's response: We have made great effort to collect as much geophysical information as possible to justify the probable segmentation. However, we do acknowledge the segment boundaries given in this study are by no means the only possibility. Such uncertainties have been emphasized in this section.

Section 3

I'm not sure, but it looks like the scenarios are motivated by the assumption that:

- The earthquake of interest has a 1000 year return period; and perhaps also that
- All the strain is released in that earthquake

It's not perfectly clear to me if these are the assumptions, so consider re-writing to make it very explicit.

Assuming I have correctly described your approach – obviously these assumptions are unlikely to be exactly true, because (for example) some strain will be released by smaller earthquakes; furthermore, maybe larger magnitude events occur with even longer return periods. Of course no- one can say for sure at present.

Regardless, I think it is reasonable to define a scenario as "an event which would release 1000 years of strain accummulation". But if this is what you are doing, it should be presented in this way.
Currently the paper attempts to justify the scenario choice from the coupling and paleo data – whereas to me, the choices seem "reasonable, but definitely ad-hoc". See detailed comments.

Author's response: We apologize for the confusion. It is true the scenarios are designed based on the assumptions listed by Reviewer 1. We have modified the main content and made these assumptions clear at lines 354-355.

Author's change to manuscript: Sentence is added in section 3 ".. assuming each event releasing 1000 years of strain accumulation while ignoring possible portion of strain release by smaller events."

Section 4

Generally good, see detailed comments.

Section 5

Here I think the uncertainties in scenario design and our understanding of subduction zonesare not sufficiently integrated into the discussion. See detailed comments.

Conclusions

Generally good, see detailed comments.

Detailed comments

Around Line 36 – I suggest slight edits as follows (bold).

- Since 1900, many megathrust ruptures have triggered numerous devastating near- and far- field tsunamis including the 1952 Mw 8.8-9.0 Kamchatka **event** (e.g., Johnson & Satake 1999; Kanamori 1976), the 1960 Mw 9.5 **event** in the Chile subduction zone (e.g., Cifuentes 1989; Moreno et al. 2009), the 1964 M w 9.2 Alaska **earthquake** (e.g., Plafker 1965), the 2004 M w 9.2 Sumatra-Andaman Earthquake along northern Sunda Trench (e.g., Vigny et al. 2005; Banerjee et al. 2007; Chlieh et al. 2007) , and the more recent 2010 M w 8.8 Maule **event** in Chile (e.g., Vigny et al. 2011; Pollitz et al. 2011) and 2011 Mw 9.0 Tohoku- Oki earthquake along the northwest border of the pacific ocean (e.g., Koketsu et al. 2011; Wei et al. 2012).

Around Line 56-57 – Suggest to replace "are considerably inconsistent" with "differ greatly among studies".

Line 73 – remove "is likely to occur"

Line 76 – add "the" before

"1960s".

Line 99 – Suggest to remove "could only have been" – I think the point is very reasonable, but one could hypothetically imagine various less plausible causes (e.g. asteroid). I'm certainly not promoting that idea – simply noting that logically, "could only have been" might be too strong a statement, and is not required.
Line 123 – Suggest to replace "only ca.1/20" with "only about 1/20". Alternatively, consider "approximately 1/20" or "less than 1/XX".

Line 128 – "we utilize the" – suggest to replace "the" with "a".

Author's response: All the grammatical corrections between line 36 -128 are made according to the suggestions given by Reviewer 1.

Line 132 - "Our rupture models afford standard examples for an improved understanding of the tsunami hazard in the SCS. " – I don't understand this, consider re-wording. Not sure if you mean to imply that your results are necessarily better than previous work? I would be cautious about accepting any notion that "because we use better data, our scenarios are better", given the huge uncertainties about Mw-max, seismogenic depth, etc. I don't think those issues are "perfectly resolved" by your paper (and I would not expect or require that, because they are "deep" problems in general).

Author's response: We have modified the text accordingly.

Author's change to manuscript: At lines 141-142, we added "Here the proposed rupture models afford a physical-based understanding of the tsunami hazard in the SCS."

Line 138-158. I like this discussion, which gives a realistic depiction of the limitations in our understanding of megathrust earthquakes.

Author's response: Thanks for your positive comments.

Line 162: – "Systematic analysis of collections of great earthquakes globally indeed suggests that some of the physical parameters do play key roles in controlling the rupture characteristics (Bilek and Lay, 2018; Bletery et al., 2016; Schellart and Rawlinson, 2013)." – I would suggest adding something like ", although limitations in the historical earthquake record inevitably make it difficult to have high confidence such relationships". For instance, we saw this with the (now discredited) idea that only 'young, fast' subduction zones can host large magnitude earthquakes (Stein and Okal, 2007). It seemed reasonable when proposed, but now we have enough data to say it doesn't work. Given recent "suprises", I think we still suffer from 'lack of data', and cannot be sure about physical controls on ruptures. See also related comments on Section 4 below.

Author's response: We have modified the text according to the suggestions.

Author's change to manuscript: At lines 182-183: ".. although limitations in the historical earthquake records inevitably make it difficult to have high confidence on such relationships."

Fgure 1 – It would help if you explicitly denoted the rupture segments. I appreciate the figure is already crowded, and so some judgement is required here -- but as a reader I did have to spend a bit more time to figure out where the 3 segments are.

Author's change to manuscript: we have denoted the segments using colour-shaded curves.

Line 183: Suggest to replace "While" with "In

contrast,"

Line 190: Suggest to replace "rupture cases" with "ruptures".

Line 260: "they would have be buoyant" – missing a "to" before "be".

Line 288: Missing "a" before "velocity value"

Line 295: Suggest to replace "the coupling map though not the perfect" with "the coupling map, although not perfect,"

Author's change to manuscript: All the grammatical corrections between line 183- 295 are made according to the suggestions given by Reviewer 1.

Line 302: "To gain a comprehensive understanding of the seismic return time period, large amount of historical seismic data and geological evidence are required." – I disagree with this – rather, currently it seems implausible to comprehensively understand the seismic return period. In my opinion, a more realistic statement would be something like "Given the short duration of historical records relative to the return-periods of high-magnitude events of interest, and limitations in our capacity to infer earthquake return-periods from first-principles physics, it is unrealistic to expect to develop a comprehensive understanding of seismic return periods."

Author's response: This point is well taken.

Author's change to manuscript: We have modified the text at lines 326-330: "For seismic return time period, given the short duration of historical records relative to the return-periods of large-magnitude events of interest, and limitations in our capacity to infer earthquake return-periods from first-principles physics, it is unrealistic to expect to develop a comprehensive understanding of seismic return periods."

Line 316: Here the text states that "We, thus, use the only available information that the seismic return period is likely to be ca.1000 year and a giant event had ruptured the Manila trench in the last seismic cycle". I cannot see that the available information can tell you either of these things. The "1000 year" number appears to be an ad-hoc decision in the earlier paper (it may indeed be a "reasonable ad-hoc decision" – that's OK – but the key point is that it is not a precise consequence of observations). To be clear what I mean by 'ad-hoc', consider the question: Why 1000 year? Why not 2000? Why not 500? Why not 784.37?. Further, I doubt that the paleo work can "strongly" justify the above points, given the limited number of sites and difficulties in interpretation.

In my opinion, it would be much better to say something like "We choose to model scenarios which release 1000 years of accumulated strain, because these represent large, rare and yet plausible events which are of interest for hazard assessment purposes, and the paleo data indicates that large events may well occur". This would seem to be more consistent with what we actually do know, and reflective of the uncertainties.

Author's response: This point is well taken.

Author's change to manuscript: We have updated this part accordingly at lines 347 to 351: "Here we choose to model scenarios, which release 1000 years of accumulated strain, because these represent large, rare and yet plausible events, which are of interest for hazard assessment purposes, and paleo-geological data indicate that large events may occur about 1000 years ago."

Line 321: Missing “of” before “1000

years”.

Line 433: “deficient” – should this be “deficit”?

Line 349: “and beyond behave semi-brittle” consider re-wording, e.g. you could replace

“behave” with “induce”.

Line 350: Some suggested edits: “~~By doing so it could~~ **This can** capture ~~the~~ **to** first-order the ~~of~~ potential slip extent (Figure 3c and f), ~~similar as the estimated~~ **with a** depth-range of slip **consistent with observations** from global megathrust great earthquakes (e.g...”

Figure 3: Suggest to increase the size of the panel labels (a), (b), … etc. They are hard to find at present.

Line 381: Add a full-stop “.” after

“ruptures”.

Line 383: Add “the” after “solves”.

Line 386: Suggest to delete “We considered one grid layer for each case to model wave propagations in the SCS because...”, and replace it with “A uniform grid was used because ...”

Line 389: Add “the” after “along”

Line 392: Suggest to delete “used to simulate the tsunami wave propagations” and replace with “equal to the initial ocean surface deformation”.

Line 432: Suggest to replace “Potential tsunami arrival time” with “The tsunami

travel time”

Line 445: There is a typo here around “as”

Line 446: Remove “further afield”.
Line 448: Replace “route” with “routes”.
Line 455: Suggest to replace “Whereas” with “Conversely”.
Line 466: Suggest to replace “flat gentle dipping” with “flat, gently dipping”

Line 467: Suggest to replace “dipping” with “dip”.

Author's change to manuscript: All the grammatical corrections between line 321- 467 are made according to the suggestions given by Reviewer 1.

Line 459 – 467: I think this section gives the impression that we understand how a number of physical factors control the distribution of megathrust earthquakes. To me, the point should be highly qualified, because really we do not have sufficient data to be "highly confident" that such relationships work. An alternative view is that currently, we don't have enough data to say for with confidence how big/how often very large subduction earthquakes occur on any source-zone, or to relate this to physical factors such as mentioned in this section. McCaffrey (2008) is a classic reference in this regard. Also, see Stein and Okal (2007) for an example study which argues against the Ruff and Kanamori (1980) result cited here. You might also note examples of earthquakes which have crossed supposed "rupture barriers", such as the 2007 Solomon earthquake (see discussion in Lorito et al 2015 review paper as a starting point). In this vein, it is also worth noting alternative approaches to modelling Mw-frequency relations which do not make heavy use of such "physical information" – for example Rong et al. (2014), Kagan and Jackson (2013), which only employ basic moment conservation principles and catalogue data.

In sum, I think you should add some discussion of the "unknowns" to this part of the text, perhaps using the references above, so that the reader is not left with the impression that our understanding is better than it actually is.

Author's response: We understand the reviewer's concern about the certainty shown in the previous version. In this revised version, we added some discussion of the "unknowns" according to the reviewer's suggestion.

Author's change to manuscript: We have added this part of discussion in the main text at lines 523 to 533: "While as the boost of geodetic measurements, the relationship between great ruptures and the convergence rate was challenged (McCaffrey 1994; Stein and Okal 2007; and Nishikawa and Idel, 14). The maximum moment magnitude of a potential earthquake is often determined from seismic catalogue data, alternatively determined from basic moment conservation principles and catalog data (Rong et al., 2014; Kagan and Jackson, 2013). Overall, with current short observation time span as compared with multi-century seismic return period, it is improper to make the determination on the relationship between these physical parameters and how big or how often a giant earthquake can occur in any subduction zone (McCaffery, 2008). Clearly, long-term and complete observations within seismic cycles are required for a better understanding of subduction zone rupture behaviors."

Line 475-486: Consider mentioning the 2007 Solomon earthquake here, as a counter-example to your points. This will emphasise the need for caution when making assumptions about rupture barriers.

Author's change to manuscript: We have included the 2007 Solomon example that shows in some cases the rupture went across triple junction at lines 554-555: "…like the 2007 Mw 8.1 ruptured a triple junction (Furlong et al., 2009; Taylor et al., 2008)."

Line 497-514: Here the text appears to be claiming that the scenarios in this paper are "better" than others. I think these conclusions must be softened, because the only way to "scientifically test" this claim would be to observe many tsunami events on the Manila trench, and determine whether your tsunami scenarios better captured the true behaviour. Obviously this is impossible in practice (maybe our ancestors could do it in a few thousand years!). Thus, we are necessarily left with substantial doubt as to whether one set of tsunami scenarios is "better" than another.

In this paper, the scenarios are developed using better data than some previous studies. Does this lead to high confidence that the scenarios are more realistic? I would say "no". Even if we only consider the large uncertainties in Mw-max that have been proposed for the Manila trench (e.g. Berryman et al., 2015), it follows that the scenarios in this paper could be seriously wrong and not necessarily better than others, nothwithstanding the use of better data in the current study.

Let me stress that I do not expect the authors to solve this problem – rather, it reflects the current "deep" uncertainties regarding size constraints for subduction earthquakes. The author's approach to scenario construction is reasonable, but there are too many "deep" uncertainties to conclude it is "better". The text in this section should be softened accordingly. Of course you should still emphasise the good things about the scenarios in this paper (i.e. better data, etc).

Line 504-505: "Heterogeneous slip models, as we observe from finite rupture models of earthquakes, are more realistic and could better explain the observations". Here it's unclear what "observations" you mean. Consider replacing this sentence with "Finite rupture models of historical earthquakes indicate that slip is heterogeneous, and this is represented by our scenarios".

Author's response: We have updated and softened this paragraph accordingly.

Author's change to manuscript: Please see the changes in Section 5 Discussion: "… planar fault with uniform slip assumed rupture cases. We've seen that finite rupture models of historical earthquakes indicate that slip is heterogeneous, and this is represented by our scenarios. Further detailed tsunami hazard assessment in SCS demonstrates that uniform slip models underpredict tsunami hazards as compared to a heterogeneous slip model (Li et al., 2016). Therefore, our refined earthquake rupture scenarios in zones 1 and 2 provide new insights for tsunami hazard assessment in SCS."

Line 520 – 522: Along the lines of my comment above, I would suggest replacing "provide new constraints for" with "enable" – because although I think your scenarios are reasonable, I don't think they really provide strong new constraints as to what is possible on the Manila trench. This is a deep problem that reallistically cannot be solved by the current paper.

Author's change to manuscript: The grammatical corrections between line 504- 522 are made according to the suggestions given by Reviewer 1.

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