tektonika

Review Report

van Zelst et al. - Investigating global correlations between earthquake-generated tsunamis and subduction zone characteristics, TEKTONIKA, 2025.

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1st Round of Revisions

Decision letter

07/10/2024

Dear Iris van Zelst, Silvia Brizzi, Elenora van Rijsingen, Francesca Funiciello , Ylona van Dinther:

We have reached a decision regarding your submission to тектоніка, "Investigating global correlations between tsunami, earthquake, and subduction zone characteristics".

We have now obtained two reviews that provide constructive comments on the manuscript and I would just like to take this opportunity to thanks our reviewers. Jack Williams, as AE, has led this review process and also scrutinised the reviews and provided their own comments, which I include below. I have carefully read all necessary documents myself and agree with Jack's assessment. Firstly, the paper is well-illustrated and well-written. Overall, we think this is a very interesting paper that compiles crucial data for understanding tsunami hazards, and agree that this will be a welcome addition to Tektonika. However, the uncertainties involved make establishing correlations difficult and both reviewers, and Jack, have suggestions about extra considerations that should be made before publication. The reviews highlight issues / possibilities that should ideally be resolved through extra analysis, but at least should be discussed in the text.

If you have any queries regarding our decision, please do not hesitate to contact us. We look forward to seeing your revised manuscript.

Kind regards,

Craig Magee

From Jack Williams (AE):

Dear authors,

Thank you for your submission to Tektonika, for which we have now received two reviews. Based on these reviews, and my own reading of the manuscript, I can see that this potentially represents a novel and interesting Tektonika article, but that revisions are required prior to acceptance. In particular, I note that both reviewers ask for more analysis to test the significance of the correlations given the limited observational record, and the potential role of local topography controlling tsunami height.

As a suggestion, with regard to the former (and my own curiosity), could it be worth rewriting Nt as: Nt = number of tsunami/(segment length * number of earthquakes >M_min)

Thank you for this thoughtful and interesting suggestion. After careful consideration, we decided to maintain our original definition of Nt in the manuscript. There were multiple reasons for this decision. First of all, the tsunami parameters are from 1962 to 2018 while the data on the (number of) earthquakes is from 1900 to 2007 and contains two different minimum earthquake magnitudes: thrust earthquakes from 1976 to 2007 with Mw 55 and from 1900 to 1975 with Mw 70. Indeed, this is the reason we run our bivariate analysis between Nt and the seismicity parameters only using a subset of the tsunami data (cut off at 2007). Another reason is that we only have these seismicity-derived parameters available from the original database of Heuret et al. (2011), not an actual earthquake list of events. Hence, our database only contains tsunami events caused by earthquakes and no explicit earthquake events themselves. Work is currently ongoing to update the database from Heuret et al. (2011) based on a longer earthquake catalogue. When this updated database becomes available in a few years, it would indeed be great to update our study with this data. However, for now that remains within the realm of future work.

Where M_min is some magnitude you can define based on the instrumental record completeness/magnitude of earthquakes typically associated with tsunami. In this way, you are not penalising the tsunami potential of subduction zone segments that have hosted relatively few earthquakes in the instrumental record?

This is a very interesting point and indeed, we apply the Fisher analysis with the exact intent of finding that out. In our case, we use the parameter Darc trench (the distance between the volcanic arc and the trench) as a rough proxy for slab dip that is independent from any time-limited seismicity data. We find that this parameter is not important in dividing classes of subduction zones that have hosted many and few earthquake-generated tsunamis from 1962 to 2018. Our first-order bivariate analysis also fails to reveal any evident correlations between Nt and Darc trench or, indeed, any of the geometric parameters that are derived from megathrust seismicity. As such, our study shows that the geometry of the subduction slab has not been a crucial factor in tsunamigenesis in the last 56 years. We have added some extra sentences in the bivariate results section to highlight these points more (lines 388-389).

Lines 274-279: Intuitively, the correlation between Nt and the subduction zone velocities makes sense, as faster subduction zones have more earthquakes. But this relationship is not 1:1 as the earthquake rate is also influnced by the coupling on the subduction zone and thrust dip (for a given velocity, a gently dipping thrust will produce more earthquakes as it has a large area at a depth above the base of the seismogenic crust)? So do these co-varying parameters influence Nt? Maybe this is something that the Fisher analysis can indicate? Additionally, in Table 1, is it possible to use more recent data than DeMets (1990) for the vsn and vcn parameters? Bayona-Viveros et al. (2019) could give you some good ideas for addressing these suggestions.

Viveros, J. A. B., von Specht, S., Strader, A., Hainzl, S., Cotton, F., & Schorlemmer, D. (2019). A regionalized seismicity model for subduction zones based on geodetic strain rates, geomechanical parameters, and earthquake-catalog data. Bulletin of the Seismological Society of America, 109(5), 2036-2049.

We thank the Associate Editor for the valuable suggestion. In our study, we build upon existing databases and specifically introduce parameters related to earthquake-generated tsunamis to try and gain insights into overall subduction characteristics that may promote tsunami generation. As such, we have translated the tsunamis caused by earthquakes into the segmentation of Heuret et al. (2011), but we refrain from adding other, new parameters to the existing database. We considered updating vsn and vcn following Viveros et al. (2019), as they indeed use the same segmentation. However, implementing their methodology to recompute these parameters for all subduction segments 2would require a significant shift in our study's scope. In addition, retaining velocities from DeMets et al. (1990) ensures consistency with previous studies like (Heuret et al., 2011; Brizzi et al., 2018; Van Rijsingen et al., 2018).

I look forward to receiving your revised manuscript

Kind regards

Jack

Comments by Reviewer C (Jonathan Griffin)

Reviewer C: To perform this study, the authors compile a large dataset of subduction zone and tsunami parameters that will likely be of use to future studies, and is a substantial contribution. The paper thens looks at correlations between a large number of parameters related to subduction zone crustal properties and structure, sediment cover, seismicity and convergence rates with parameters used to define tsunamigenic potential. The paper aims to determine which factors might promote a particular subduction zone to be more likely to generate tsunamigenic earthquakes. Understanding the hazard posed by these earthquakes is an important topic of investigation, and any results from such a study may help inform tsunami hazard assessment. However, given this significance, it is important than correlations are robustly established.

I have added several comments on the annotated pdf, however below are some of my major comments.

Thanks for the detailed comments in the pdf. We incorporated them all, clarifying the paper in the process. A key challenge in the paper, and one that is worthy of further investigation, is dealing with the relatively short record of earthquakes and tsunamis in the database, when compared with typical return periods for large tsunamigenic earthquakes. A related issue is consideration of the magnitude-frequency distribution of tsunamigenic earthquakes on different subduction zones, and hence what sampling biases may result within the dataset. One thing that I would like to see in the paper is further work to show that the results are significant. One gets the feeling that in the analysis of the correlation coefficients that some of the observed correlations might arise by chance. Significance is considered by the authors for the correlations individually, however as correlations are compared between dozens of parameters one might expect that a few correlations might show up simply by chance, given the relatively small dataset. The Fisher analysis goes some way to address this, however I would like to see some analysis that shows why we might expect the next 56 years to look like the past 56 years. One can only work with the data available, but I'd recommend the authors do some work to better demonstrate that the correlations do not arise by chance. We thank the reviewer for this comment. We fully acknowledge that the relatively short record of tsunami events presents a challenge when compared to the long recurrence interval of major earthquake-generated tsunamis. As with any statistical analysis based on observational data, there are inherent limitations in sample size and potential biases in data availability. Unfortunately there is little that can be done beyond transparently communicating this constraint to the reader and throughout the manuscript we have explicitly stated these limitations on our findings. Our approach focuses on identifying general trends rather than precise predictions. While some correlations may arise by chance, our results were interpreted in the context of previous work done on the topic. Regarding the concern about whether the next 56 years would be similar to the previous 56 years, again we acknowledge that while earthquake and tsunami recurrence interval can span centuries, our analysis can only provide insights into observable patterns with the available time frame. We explicitly recognize this in the revised manuscript and emphasize that our results should be viewed as a contribution to understanding long-term trends rather than precise predictions of future behaviour.

Related to the relatively small dataset, how might the results differ if a few additional big events (e.g., 1960 Chile) are included? Also, surely the earthquake record can be easily extended from 2007 to 2018 to match the tsunami record, given these are publicly available datasets. This should be done prior to publication.

Thank you for your suggestion regarding extending the earthquake record to match the tsunami record. It is important to note that in this study we actually only explicitly use tsunami events (not earthquakes), although we realise this has been phrased in a confusing manner in the paper. We have tried to rectify this in this new version of the paper. The other parameters, such as those describing tectonic or seismic characteristics of subduction zones are taken from previously existing databases (such as Heuret et al., 2011) that consider potentially different timespans of earthquake catalogues to derive the parameters (but crucially the earthquake events themselves are not part of the database). For example, the maximum earthquake magnitude that a subduction zone experienced is determined using earthquake data from 1900– 2007. To make sure our analysis is not affected by differences in timespans of different data sets, we focus our main (multivariate) analysis solely on the tsunami events data set and the timespan-independent tectonic parameters of our SNITCH database. Regarding updating the database parameters derived from data with a timespan up to 2018, this study builds upon the database of (Heuret et al., 2011), which has not yet been updated to incorporate data beyond 2007. While publicly available earthquake catalogues exist, identifying megathrust earthquakes as done in Heuret et al. (2011) requires careful selection and classification to separate megathrust events from shallow intraslab or sedimentary wedge earthquakes. This is a time-intensive process. As such, extending the database to incorporate additional events beyond 2007 would essentially require us to build a completely new database in the same vein as Heuret et al. (2011). This is a worthwhile endeavour and indeed ongoing work, but in starting to look at this possibility, we realised that the significant reprocessing that is needed to ensure consistency with our methodology is far beyond the scope of this work. Extending the dataset would necessitate recomputing all seismicity-derived parameters, making this a non-trivial task. We also explored the possibility of conducting independent tests using other databases (e.g., Schellart and Rawlinson, 2013) to check the consistency of our results, but this has similar difficulties due to the subduction segmentation we use. As such, we are unfortunately limited to the database we have at hand for this work; an updated subduction database using the segmentation approach of Heuret et al. (2011) is in progress and will hopefully become available in the next few years.

A key challenge for the study is choosing a suitable metric for tsunami potential. Using the number of tsunami events is tricky, as it does not seem that there is good discrimination between subduction zones that have produced one big earthquake and those that have produced multiple smaller events. The correlations between tsunami height and subduction parameters are also problematic. As the authors note themselves, observed tsunami run-up heights are highly dependent on localised bathymetry and topography, as well as the causative earthquake. Are there hidden correlations in the data, e.g. are erosional margins more likely to have steep nearshore bathymetry and topography that amplify run-up heights? Given these uncertainties, I am cautious regarding these correlations. Some additional commentary about this issue would be welcomed.

As mentioned above, we realised that these comments could be traced back to our inclusion of incomplete and sometimes inconsistent data on the tsunamis, like maximum water height, and tsunami magnitude. We thought it would make sense to show these data, but we haven't been able to clearly communicate the caveats with which they should be viewed, making the paper more confusing than it needs to be. Therefore, in order to make the paper more clear, we decided to remove these parameters from our study. We still mention them in the paper and comment on why they are omitted from the study and how they were internally inconsistent. Hopefully, this answers part of the comment above, since this now ensures there are no 'hidden' correlations between things like bathymetry and the observed maximum water height. In response to the first part of the comment on choosing a suitable metric for tsunami potential, we have now added some extra commentary on this (also discussing the suggestion from the pdf to perhaps scale by number of earthquakes). See for example lines 228- 237; 273- 285; 516- 531.

Comments by Reviewer B (Qiang Qiu)

Zelst et al. have a done a thorough study on investigating the possible correlations between tsunami, earthquake, and subduction zone characteristics through analyzing comprehensive data sets, which include tsunami wave height, runup, geometry, and tectonic features of the global subduction zones. It would be interesting to look at such correlations through a statistic way. If indeed there are some intrinsic correlations, then it would be important for future seismic and tsunami hazard assessments in regarding the hazard preparedness at a global scale. However, to my impression, the seismic and tsunami hazard are directly defined by the earthquake. Therefore, a tectonic fault zone where an earthquake nucleates controls the earthquake rupture. While there are many factors that can affect the earthquake nucleation, rupture propagation, interaction etc. The regional and on fault stress stage that migrate from a long-term behavior plays crucial roles in timing, magnitude and rupture style of an earthquake, and therefore the ensuring tsunami hazard. These factors are different at different subduction zones, therefore, difficult to compare. In the meanwhile, whether a sediment-starved, erosional margin with a complex shallow subduction interface prone to be more tsunamigenic or less tsunamigenic as a creeping fault is still on active debate, not concluded yet.

We appreciate the reviewer’s thoughtful perspective on the complexities of relationships between earthquake nucleation, rupture propagation, and their links to tsunami generation. Indeed, many of these factors vary across subduction zones and are still the subject of active debate. Our study does not aim to resolve these long-standing questions but rather to explore statistical correlations between subduction zone characteristics and tsunami occurrence on a global scale to contribute to this debate. While we recognise the inherent limitations of such an approach, identifying first-order trends can provide insights into broad patterns of tsunamigenesis. These insights may serve as a stepping stone for future studies that incorporate more detailed physical and regional modelling. We have clarified this in the manuscript to better reflect the scope and objectives of our study (lines 85- 86).

45,65, why don’t mention the high angle imbricate or splay faults in the wedge? E.g., Judith et al., 2015; Hananto et al., 2020; Yang et al., 2020; Hubbard, J., Barbot, S., Hill, E.M. & Tapponnier, P., 2015. Coseismic slip on shallow de’collement megathrusts: implications for seismic and tsunami hazard, Earth-Sci. Rev., 141, 45–55. Hananto, N.D. et al.. 2020. Tsunami earthquakes: vertical pop-up expulsion at the forefront of subduction megathrust, Earth Planet. Sci. Lett., 538, 116197. Yang, X., Peel, F.J., McNeill, L.C. & Sanderson, D.J., 2020. Comparison of fold-thrust belts driven by plate convergence and gravitational failure, Earth Sci. Rev., 203, 103136.

Thanks for pointing out these additional references. We have added them to the paragraphs where we discuss splay faults and the accretionary wedge. Additionally, we have rephrased the sentences slightly to highlight that we are talking about splay faults here. We omitted the reference to Yang et al. (2020) as their review on deepwater fold-thrust-belts was not relevant to our descriptions or mentions of tsunamigenic potential of splay faults in the accretionary wedge.

63, why the sediments in the accretionary wedge are unconsolidated? There are many high-angle thrusts fault in the wedge, which can be measured at many subduction zones.

We have rephrased this, such that we now explicitly talk about splay faults in the accretionary wedge.

70, that just one example, yet the rest of the tsunami earthquake catalog did not find this correlation.

This is indeed an example that tsunami earthquakes have been linked to seamounts. We show in this work that there might indeed be some evidence to link earthquake-generated tsunamis with seamounts through the seafloor roughness parameter. We have clarified the text in the introduction, so that it is clear that this is just one example. We refer back to this and discuss the potential relationship between seamounts and tsunami earthquakes more in Section 5.2.

58,61-62, same reference cited at two places but talking about contrast content. Thanks for pointing this out.

We referenced Polet and Kanamori (2000) in sentences linking tsunami earthquakes to both sediment-starved and sediment-rich margins, which is indeed confusing and contrasting. We have now clarified it, so that it is clear that Polet and Kanamori (2000) link (the normal, overarching set of) tsunami earthquakes to sediment-starved margins and the subset of tsunami earthquakes called ‘slow tsunami earthquakes’ to margins that have a thin layer of sediments.

Figure 1. why there is no single point at the Makran subduction zone? The 1954 Mw 8.2 Makran event caused >10 m high tsunamis.

As noted in the figure caption, the events plotted here are from our database, which starts in 1962. This is why the 1945 Mw 8.2 Makran event is not included. As explained in the manuscript (lines 159– 162), this starting point was chosen because the installation of the World-Wide Standardized Seismograph Network in 1962 ensured global earthquake monitoring. To improve clarity, we have now explicitly listed this event among those not considered (line 164- 165; 249- 253; 626- 630) to make readers aware of its exclusion and discuss this data limitation.

96-98, these features are currently not subducted yet. Not sure the same dimensional features were subducted at deeper depths, therefore it is difficult to assume this as a proxy for the roughness on the plate interface. Even if look at the seafloor roughness, they are not continuously distributed.

We agree with the reviewer that seafloor roughness features currently approaching the trench may not always be directly subducted or continuously distributed. However, as discussed in Lallemand et al. (2018), seafloor roughness measured immediately seaward of the trench may reasonably approximate the roughness immediately landward. This justifies using seafloor roughness as a proxy for the roughness on the plate interface. We discuss this in the manuscript at lines 128- 138.

111-113, the geometry of the downgoing slab represent the plate interfaced therefore the geometry of the megathrust, no? confusing.

The geometric parameters used in our analysis specifically describe the seismogenic zone along the megathrust (Heuret et al., 2011) and do not encompass the full complexity of the subduction system, including deeper slab geometry or other tectonic features. To improve clarity, we have revised the sentence accordingly (lines 145- 151).

122,124, but tsunami record seems start much earlier than the modern seismograph network.

Both tsunami and earthquake records have started before 1962, the year when the World-Wide Standardised Seismograph Network was installed. However, those records depend heavily on location and — on a global level — are biased towards large events. In this work, we wanted to take a global approach towards earthquake-generated tsunami events without the assumption of merely looking at large earthquakes that caused tsunamis. As such, we are limited in our tsunami data retrieval to the years when there is global coverage of earthquake monitoring, as we are only considering tsunamis caused by earthquakes (i.e., meaning that the earthquakes need to be recorded globally for them to be linked to a recorded tsunami). The NOAA NGDC/WDS Global Historical Tsunami Database (Global Historical Tsunami Database, 2019) explicitly cautions that data on tsunamis before 1962 might be incomplete. Hence, in order to get the most robust statistics, we need to confine our analysis to data from 1962 onwards.

125, Using the ..., we extract 395 tsunami events. Changed as suggested.

Note that we have changed our phrasing from 'tsunamis' to 'tsunami events' throughout the entire manuscript, as per the suggestion of the reviewer. Table 2, number of tsunamis–> number of tsunami events Changed as suggested.

In table 2, what is the difference between maximum water height and maximum tsunami magnitude? How do you define the tsunami intensity?

Although we previously included lots of details on the exact definitions of maximum water height, tsunami magnitude, and tsunami intensity, we have decided to remove these parameters from our analysis to make the paper more clear. These measures were not consistent in the Global Historical Tsunami Database (2019) anyway (stemming from different or inconsistent measurements across and between individual tsunami events), so we decided to streamline our manuscript and remove any mentions of these parameters. The only place in which we still mention the maximum water height is in Figure 1 for illustrative purposes when we visualise the different tsunami events in our database. We have clarified the caption of this figure to detail exactly which measurements have been used to measure the maximum water height in the Global Historical Tsunami 6Database (2019).

126-129, these events and also the 1945 Mw8.2 Makran event have tsunami wave information records from NOAA. In addition, you analysis time starts at 1962, so even for these events, the missed earthquake, if there are some, won't affect the seismic records. So, these missed events should be included in the analysis.

As noted in our previous response, the completeness of the seismic and tsunami record before 1962 can vary by region and might be biased towards big events. Considering several well-known large quakes for some segments and not others would skew our analysis as the earthquake-generated tsunami data would be associated with different time frames for different segments. This is why we limit our analysis to the period when earthquake monitoring became globally reliable (from 1962 onwards), as recommended by the NOAA NGDC/WDS Global Historical Tsunami Database (Global Historical Tsunami Database, 2019). However, we explicitly list earlier significant events in the manuscript to acknowledge their existence and ensure transparency of the limitations of our approach.

131,.... Consists of 284 tsunami events.

Changed as suggested.

138,equation 1- tsunami magnitude is scaled by the maximum runup which is highly localized and depended on the topography, and also whether the maximum one can be reached and found in the field. This is probably not a good way to look at the tsunami magnitude, instead, the tsunami wave height at the coastline, the inundation distance and flow depth are more proper to describe the tsunami magnitude. Equation 2 has a similar issue with 1. This is essentially true because the runup could be not reported for a particular event.

Agreed. This is a very good point by the reviewer and highlights the incompleteness of the Global Historical Tsunami Database (2019) database when it comes to measures like maximum water height and its derivatives like tsunami magnitude and intensity. Since these measures of tsunami size are imperfect and not crucial to our analysis, we have removed them from our study to make the whole manuscript clearer and more focussed.

144, a total of 329 tsunamis.... It is better to correct this from tsunamis to tsunami events through the whole paper.

We have changed ‘tsunamis’ to ‘tsunami events’ throughout the entire manuscript.

147-153, the reported tsunami events are likely associated with earthquake events, thus, if move them around, will that introduce bias in your analysis? Because a long subduction zone is highly segmented with some places have frequent earthquakes, while others not. E.g., Sumatra, Java, Nankai, Chile etc.

We agree with the reviewer that manually assigning tsunami events to overlapping segments could introduce some (difficult to quantify) bias. However, as our study relies on previously compiled databases, we follow this predefined segmentation framework adopted by Heuret et al. (2011). This framework ensures consistency across studies (e.g., Heuret et al., 2012; Brizzi et al., 2018). While we acknowledge this is a limitation, we have clearly indicated how tsunami events are sorted between overlapping segments in the manuscript (lines 185- 222), and we believe this is the best approach given the available data.

166-169, if they have different definitions, and then how to compare them in a statistic analysis.

As mentioned in a previous response, we acknowledge the limitations of the Global Historical Tsunami Database (2019) regarding measures like maximum water height and its derivatives, such as tsunami magnitude and intensity. Since these parameters are imperfect and not crucial to our analysis, we have removed them from our study to ensure clarity and consistency.

Equation 3, in each subduction and/or each segment, the Nt,tot is very different. By selecting the maximum one over all the data base, and use to scale the each of the segment may not meaningful. Since different subductions behavior differently at time state of their seismic cycle, current time window is not sufficient to capture even one segment. For example, the value at Ryukyu is surprisingly high but no large earthquake occurred in the analyzed time window; however, in Sumatra, there are great to giant earthquakes and their associated tsunamis since 2004, but the value is much lower (184,185). Nt does not work well for long rupture length events such as 2004 Sumatra, 1960 Chile, 1965 Alaska.

We address the limitation of the time window (which may indeed be less than the recurrence interval and/or a complete seismic cycle) in the discussion (lines 251- 253; 568- 580; 608- 614). Note that the 2004 Sumatra-Andaman tsunami event is not sorted into the Sumatra subduction segment, but is instead sorted in the Andaman segment. We divide by Nttot to normalise the values and look at the relative amount of earthquake-generated tsunami events, rather than the absolute amount, so we can better compare them. The fact that we look at Nt indeed results in the fact that the size of the individual tsunamis becomes irrelevant; only their number counts. Although this is indeed a disadvantage, we also view it as an opportunity to see if there are any parameters conducive to the process of tsunamigenesis regardless of the size of the tsunami that is created or the size of the earthquake that is responsible for triggering the tsunami. To make this clear to the reader, we have updated our methods section as per your suggestions: we extended our discussion on ideal alternative measures of tsunami energy that unfortunately do not exist for the purposes of our database yet (lines 269- 277), and we have also added a few sentences on why we think it could also be beneficial to look at just the number of tsunamis, regardless of size (lines 281- 285).

194-199, the 2018 Palu event is not a megathrust event. Not countable in the analysis.

This is an excellent point — this event is indeed not part of our analysis. We have therefore opted to remove these sentences. Thank you very much for pointing this out!

203-210, the tsunami energy is mainly determined by the potential energy of the water column raised by the earthquake. So, if the initial seafloor deformation raised by earthquake was estimated, then one can use that to quantify the tsunami energy. Haye et al., 2017 provide the finite fault model for earthquake Mw>7.5 since 1990. Also Ye et al., 2016 provide another finite fault model with earthquake Mw>=7 between 1990 to 2015 • Hayes, G. P. (2017). The finite, kinematic rupture properties of great-sized earthquakes since 1990. Earth Planet. Sci. Lett., 468, 94–100, doi: 10.1016/j.epsl.2017.04.003. • Ye, L., T. Lay, H. Kanamori, and L. Rivera (2016), Rupture characteristics of major and great (Mw7.0) megathrust earthquakes from 1990 to 2015: 2. Depth dependence, J. Geophys. Res. Solid Earth, 121, 845–863, doi:10.1002/2015JB012427.

We thank the reviewer for these references. The finite fault models provided by Hayes (2017) and Ye et al. (2016) primarily cover large (Mw 7 or 7.5) megathrust earthquakes since 1990, which limits their applicability in terms of the time frame of our database, which starts from 1962. Furthermore, their focus on larger earthquakes means that smaller tsunamigenic events may not be well represented. Exploring how these datasets could complement our approach is certainly something that could be considered in future work, but is unfortunately not applicable to our current study. Since this is a valuable suggestion, we added these references and rephrased the sentences to provide more context on ideal tsunami parameters that quantify tsunami size (lines 273- 277).

234,235, the wave height does really depend on total number of tsunami events or Nt in a particular subduction zone segment, while it depends the magnitude and depth of the rupture.

We agree with the reviewer that tsunami wave height depends primarily on earthquake magnitude and depth rather than the total number of tsunamis in a given subduction segment. While our original explanation aimed to highlight a statistical relationship, we recognize that it may not fully capture the physical controls on tsunami wave height. In any case, as previously mentioned, we have opted to exclude these measures and their statistical analysis and have clarified this in the revised manuscript.

238,239, it may not true. It can have many moderate to strong earthquakes Mw 6+ to Mw 7+, but without big earthquake e.g., Manila and Mariana subduction zones.

Yes, this is an interesting point, but as mentioned above, we have removed these parameters from our manuscript for clarification. As such, we do not discuss this in the current version of the manuscript any more.

244-248, this may not true. For example, a 600 km depth earthquake in the Java subduction zone won’t trigger any tsunami, this is because the vertical deformation is very small over a wide area. For typical seismogenic earthquakes e.g., 2005 Mw 8.6

Nias, 2007 Mw 8.4 Bengkulu earthquakes, the tsunami is much smaller than the shallower 2010 Mw 7.8 Mentawai earthquake. Shallower earthquake can trigger exaggerated tsunami waves like the tsunami earthquakes.

Thanks for pointing this out. This is indeed another interesting point. As mentioned above, we have removed these parameters from our manuscript for clarification. As such, we do not discuss this in the current version of the manuscript any more.

264-268, the depth-dependent temperature usually allows the brittle deformation of the megathrust reach to depth at 50 km, in this case, a steep megathrust actually has limited seismogenic zone e.g., Mariana subduction. Therefore, a limited seismogenic zone has less change to hold large earthquake which is observed at Mariana.

Yes, that is a good point. Re-evaluating this again, this is actually quite a nuanced, tricky subject. Although the seismogenic zone at the interface between the two plates might be smaller for a steeper slab (due to indeed the depth of the brittle-ductile transition), the interior of the slab could remain colder at larger depths, as it takes time to heat up, and earthquakes could also occur here. Many studies have been performed to figure out how the size of the seismogenic zone changes in relation to the brittle-ductile transition with different slab dip and other parameters (e.g., Van Zelst et al., 2023). In any case, as we do no longer consider these parameters in our study for clarification and better flow of the manuscript, these sentences have been removed.

Figure 5: it is hard to there is a correlation between Nt and Tsed; between Nt and AvsE.

We agree with the reviewer that these correlations are not immediately apparent, particularly for Nt vs. AvsE, as AvsE is a categorical variable. However, the trends we describe are supported by the Pearson and Spearman correlation metrics. Moreover, we then apply a multivariate approach, as these bivariate correlations are not always conclusive. We have slightly revised the manuscript (lines 369- 382) to clarify this point. 359-361, a rough plate interface prefer creeping, and thus less large earthquakes e.g., Mariana, Philippine. Yes, the reviewer is correct. However, we are not solely considering tsunamis generated by large earthquake events —precisely to see if there are any other parameters that play a role in tsunamigenesis. As shown, our analysis shows that plate roughness is associated with more earthquake-generated tsunami events. We discuss this in more detail in Section 5.3 and have added an extra paragraph (lines 695– 698) to that section to acknowledge the fact that smooth megathrusts promote large earthquakes, which in turn are often responsible for large tsunami events. 386-390, in the case of Japan trench, it is erosive and has 89mm/yr subduction rate, then would expect more tsunamigenic events; however, if compare to

Sumatra, which is accretionary margin and has 60mm/yr rate, it has more tsunami events than that of Japan since 2004. Our multivariate analysis aims to identify general trends in earthquake-generated tsunami occurrence over the time period from 1962 to 2018. As discussed in Section 5.1, while these trends provide insights, they are not without exception and may not be appliccable to smaller time windows (e.g., from 2004 to 2025). As such, our study does not seek to look at individual subduction zones or make concrete predictions about tsunami hazard in the future but rather highlights overarching patterns across global subduction margins. To clarify this, we have now explicitly written our aim in the introduction (lines 85- 86) and clarified our goals throughout the manuscript.

Did the megathrust seismicity used here all trigger tsunami event?

No, in our study we consider earthquake-generated tsunami events and– separately– we consider parameters describing the megathrust seismicity of each subduction segment (e.g., number of megathrust earthquakes, cumulative seismic moment, etc.). Crucially, the database used in the analysis does not include any individual earthquake events (megathrust or otherwise). This approach allows us to correlate the tsunami events with the overall (seismic, tectonic, geometric) behaviour of subduction zones. We have rephrased the first sentence of the discussion to clarify this (lines 511- 513).

433-435, why the seismicity and tsunami events are not consistent with each other? Confused.

We clarified this sentence to address the potential confusion. In this study we actually only explicitly work with tsunami events (and we have kept that timespan in the sentence). Other parameters, such as those describing tectonic or seismic characteristics of subduction zones are taken from previously existing databases (such as Heuret et al., 2011) that consider potentially different timespans that are relevant to the parameter that is being extracted. For example, the maximum earthquake magnitude that a subduction zone experienced is determined using data from 1900–2007. To make sure our analysis is not affected by differences in timespans of different data sets, we focus 9our multivariate analysis (which by definition is more robust and sensitive to non-linear correlations) solely on the tsunami events data set and the timespan-independent tectonic parameters of the our SNITCH database.

439-442, repeated description.

Thank you for pointing this out. We have rephrased the sentence (lines 575- 580) in the discussion to refer back to the methods section, avoiding repetition while still ensuring the reader is aware of this limitation.

446-447, statistically, the megathrust is the most important fault that triggers the major tsunamis reported elsewhere.

Yes, that is correct. We have rephrased the sentence.

451-454, confused ...

We have rephrased the sentence slightly; we hope it is more clear now.

458-463, if it is the case, how much should we trust the correlations? 465-469 if the results are going to change, then should we not trust the results right now?

We agree with the reviewer's concern regarding the reliability of correlations given the limited time frame of our tsunami dataset. Indeed, as already mentioned in the original version of the manuscript, the recurrence intervals of large megathrust earthquakes can span several centuries, meaning that some correlations between tectonic parameters and tsunamigenesis may not be fully captured in our analysis. However, our approach provides a first-order statistical assessment of trends in global tsunami occurrence based on the most complete dataset currently available. While we acknowledge that the results could evolve with longer time series or alternative tsunami measures, the identified correlations still offer insights into general patterns of tsunamigenesis. We have clarified this point in the manuscript (see changes in Section 5.1; e.g., lines 608- 614) to ensure transparency for the reader and openly acknowledge the limitations of this study.

470, the finite fault data base could provide the rupture length

As mentioned in our previous response, we appreciate the reviewer's suggestion and acknowledge that finite fault models could provide rupture length estimates. However, incorporating these datasets would significantly further reduce the time frame of our analysis and potentially shift the focus away from the global, first-order perspective we aim to achieve. We discuss this possibility as potential future work in lines 273- 277; 721- 722.

Figure 9, does not fit with the general concept that a flat and smooth megathrust is more prone to host great to megathrust earthquakes and therefore large tsunamis (Bletery et al., 2016; Wang and Bilek, 2014).

Correct. Here, we find that a slightly different subduction setting has produced more earthquake-generated tsunamis from 1962-2018. The reason for this discrepancy is likely that we are looking at all tsunamigenic earthquakes, regardless of their size, while studies like Wang and Bilek (2014); Bletery et al. (2016) focus on large megathrust events. We do not limit ourselves to the tsunami events related to the megathrust, or large tsunami events. Therefore, it is understandable that our results differ from these studies, as we are focusing on different aspects. To highlight the differences with other studies focusing solely on the megathrust and large earthquakes, we have added a paragraph at the start of Section 5.3 to reiterate the importance of the megathrust in generating tsunamigenic earthquakes (lines 695- 700).

481-483 this is because the historical and paleo seismic tsunami events are not incorporated in the analysis.

This is a good point. We have now added a sentence on this (lines 626- 630), so the reader is aware of this fact.

541-542, however, Wang and Bilek, 2011 suggest that more fractured environment could prefer a creeping fault thus not many strong earthquakes, then no tsunamis.

This is indeed the case when considering large earthquake events. Here, we also keep open the possibility that smaller magnitude earthquakes can cause tsunamis, potentially by rupturing outer rise and splay faults which can accommodate more vertical displacement and therefore have a higher tsunamigenic potential (solely commenting on the occurrence of tsunamigenic earthquakes, regardless of their size). We have rephrased the sentence and now discuss 10the relationship between smooth megathrusts and large quakes (and hence the occurrence of associated tsunami events) in a new paragraph in Section 5.3 (lines 695- 700).

544-546, repetition

We have removed this sentence and rephrased to maintain flow of the text.

548, the 1946 Mw 8.2 Unimak event is a tsunami earthquake not outer size or splay faults itself.

von Huene et al. (2016) identifies the 1946 Unimak earthquake as one that could possibly have ruptured a splay fault, resulting in its large tsunami. Hence, we list it here as an example of tsunami earthquakes that might potentially have ruptured planes other than the main megathrust (i.e., outer rise or splay faults). We have removed the word ‘smaller’ for clarification.

553-554, This may not true.

We have rephrased the statement, so it is now clear that this is just a hypothesis (lines 707- 709).

559-561, erosional margins have limited sediments, therefore have very limited region to have splay faults. The focal mechanism for the shallow tsunami earthquake general shows a very shallow dipping fault rather than the high-dipping splay fault.

Agreed. We have removed this sentence (and added some of the reviewer’s previously suggested references in this paragraph as well, since we are talking about splay faults and tsunamigenesis here).

Acceptance letter

Dear Iris van Zelst, Silvia Brizzi, Elenora van Rijsingen, Francesca Funiciello , Ylona van Dinther:

We have reached a decision regarding your submission to τεκτonικa, "Investigating global correlations between tsunami, earthquake, and subduction zone characteristics".

Our decision is to: Accept Submission

We thank the reviewers for their detailed and constructive reviews and feel you have addressed them all well. The manuscript will be an excellent contribution to Tektonika and we thank you for supporting our goals with your submission.

Kind regards,

Craig Magee, Jack Williams