FIRST REVIEW ROUND

Letter from editor

Dear author,

I hope this email finds you well. I have reached a decision regarding your submission to Seismica, “The Impact of the Three-Dimensional Structure of a Subduction Zone on Time-dependent Crustal Deformation Measured by HR-GNSS". Thank you once again for submitting your work to Seismica.

I am pleased to say that I have now received two peer-review reports for your manuscript. Both reviewers raised important concerns that should be addressed before the paper can be considered for publication. In particular, Reviewer A is concerned about the originality of the work presented with respect to what is already known from modelling of seismic data (i.e. accounting for complex 3D structure is important to properly model waveform). I consider that this work can still be a relevant contribution to Seismica, since it provides quantitative estimates on the influence of neglecting 3D effects (at different depths) and specifically investigates the application to HR-GNSS. However, I agree with Reviewer A that a clarification on the originality of the approach with respect to state-of the art in seismology is necessary. Terminology should also be clarified: the authors distinguish between "dynamic crustal deformation" and "time-dependent co-seismic ground motion" (l.93-95) which seems to me to be the same thing (ground motion derived from the same elastic wave theory), only recorded by distinct sensors (seismic or GNSS).

In addition, I agree with Reviewer B’s concern (comment #1) about the possible bias in the source models used for the forward calculations (because they were derived with different velocity models than the real data). I think that the comparison with real data remains relevant, despite the possible bias described, and that a synthetic test (comparing synthetic time series for 1D and 3D models) should provide an important complementary validation of the results for real data.

Please find below the comments submitted by both reviewers.

[…]

Kind regards,

Mathilde Radiguet

Next page contains the reviews and point to point response

<u>Response to Reviewers</u>

Dear Reviewers and Editor,

Thank you for agreeing to review our manuscript titled "The Impact of the Three-Dimensional Structure of Subduction Zone on Time-dependent Crustal Deformation Measured by HR-GNSS". We greatly appreciate the time you and the two reviewers put into writing thoughtful and constructive comments. We believe that they have improved the manuscript, and hope you agree. We received some revisions on this manuscript and spent considerable time addressing each comment in detail. We respond to each of them below to describe how we modified the manuscript to address them.

We agree with Reviewer A that a clarification on the originality of the approach with respect to state-of the art in seismology is necessary. We added text in the introduction to acknowledge the importance of including 3D structure to properly model waveforms, particularly of time-dependent crustal deformation time series as used in geodetic disciplines for seismological purposes.

We agree that "dynamic crustal deformation" and "time-dependent co-seismic ground motion" are the same and only recorded by distinct sensors. We were alluding to the fact that previous advances show that both static and dynamic crustal deformation suffer from path effect just as the high-frequency ground motion do. We have clarified the sentence in the manuscript.

We understand Reviewer B's concerns. We have shown that the choice of 1D velocity model, even though different from the source models' 1D model, do not affect the conclusions and the PGD residuals in the 1D simulations are still very different from the 3D simulations. We performed 1D simulations of Ibaraki 2011 earthquake using the SRCMOD mean rupture model but using 1D velocity models used by Koketsu et al. (2004) and Zheng et al. (2020). We compared the PGD residuals of the resulting waveforms using these 1D velocity models with respect to the observed waveforms. The comparison plot shows that the PGD residuals using these additional 1D velocity models are different in some sense but are not significantly different compared to the trend of residuals observed for the 3D simulations in Figure 9. Therefore, the significant deviation from the PGD residual in the 1D simulations is most likely due to the path rather than the choice of the 1D velocity model. We added the figure in the supplementary material.

Below, we respond to each comment individually to describe how we modified the manuscript to address them. Due to character, reference, and figure constraints, some have been addressed by adding information to the supplement as opposed to main text, but we believe we have sufficiently addressed the largest comments within the main text itself.
Best,
Oluwaseun Fadugba & co-authors

Reviewer A:

1. L.41: There are two entries of Melgar et al. (2020) in the reference lists. Which one is cited here?
   Thank you! We have differentiated the two references by adding second author.

2. L.45-47: I agree that broadband seismometers will saturate during strong shaking, but I strong motion seismographs will not saturate particularly in the low-frequency range even for near-source strong ground motions from large earthquakes because their measurable range is 4G or 8G for most strong motion seismographs currently used in Japan.
   Thanks for pointing this out. We have clarified the sentence by differentiating between the saturation of displacement in the broadband seismograms for large earthquakes from the challenge with baseline offset not resolve by double integrating strong motion data (Goldberg et al., 2021).

3. L.74: Wald et al. (2001) should be corrected to Wald and Graves (2001).
   Thanks for pointing this out. We have corrected the references.

4. L.88: Langer et al. (2022) might be Langer et al. (2023).
   Thanks for noticing the typo. We have changed the year in the reference list to 2022.

5. L.110: The abbreviation "SD" is not defined before this sentence.
   Thanks. We have clarified its definition in the manuscript.

6. L.110-111: I could not understand correctly the sentence "at stations generally and hypocentral distance above 350-400 km".
   Thanks for pointing that out. We have modified the sentence in the manuscript.

7. L.114: What is SNR^3 3?
   Thanks for noticing that. It should be SNR $\geq$ 3. We have corrected it in the manuscript.

8. Fig. 1: It is impossible to see the slip amount on the map in Fig.1. Please check the setting of "PS_LINE_JOIN" in GMT6.
   Thank you so much for your comment. We removed the slip of the earthquakes but show only the subfaults (in dark gray) associated with the earthquakes. The slip information does not affect the purpose of the map.

9. L.118: What is "rfile"? The definition should be given.
   We have edited the sentence in the manuscript.

10. L.137: There are two entries of Melgar et al. (2020) in the reference lists. Which one is cited here?

Thank you! We have differentiated the two references in the manuscript using Melgar et al. (2020a) and Melgar et al. (2020b).

11. Table1: NEIC (2014) is not listed in the references.
    We have corrected the reference.

12. L.168-170: How did the authors project the slip in the original rupture model onto the fault geometry constructed from the slab 2.0 interface? Please describe correctly what the authors actually did.
    Thanks for your comment. We have clarified the procedure. Specifically, we project both the subfault locations of the rupture model and the centroid of the mesh of the fault geometry on a 2D plane with a strike of 210 and a dip value of 20 based on the fault geometry's general strike and dip values. We then performed linear interpolation to evaluate the strike and dip-slip slips from the rupture model at the mesh locations.

13. Fig. 2: It is difficult to see the amount of slips in this color scale.
    Thank you for the comment. We have changed the color scale to be more visible. We also changed similar figures in Figures S3 and S4.

14. L. 188: Hayes (2017) is not listed in the reference.
    Thanks! We have updated the reference list.

15. L. 192: VJMA (2001) is not listed in the reference. According to the Seismological Bulletin of Japan published by JMA, the authors should cite Ueno et al. (2002) when they refer to JMA2001 velocity model.
    Thanks for the reference. We have replaced VJMA (2001) to Ueno et al. (2002) and updated the reference list.

16. L. 192: Hayes (2014) is not listed in the references.
    We have corrected the reference.
17. Fig. 3: Hayes (2017) is not listed in the references.
    Thanks! We have updated the reference list.

18. L. 233: What does it mean “original ifile format of the 3D velocity structure”? Is it provided from the Headquarters for Earthquake Research Promotion of Japanese Government?
    Thanks! We have clarified the sentence in the manuscript.

19. Fig. 4: What is a vertical velocity boundary at about 950 in x-axis? Is it supported by seismological evidence?
    Thanks for pointing that out. The vertical velocity boundary corresponds to the edge of the subducting slab in the 3D velocity structure.

20. L. 248-250: Please check this figure caption. I think that the profile AB does not show the geometry of the two subducting slabs but the profile CD does.
Thanks for catching this. We have modified the figure caption that profile CD shows the geometry of the two subducting slabs while both profile lines show the heterogenous velocity structure in the upper 30 km depth of the 3D velocity structure.

21. L.243 & 262-264: The description of the vertical grid spacing in this paragraph is not consistent with the description of the vertical grid spacing in L. 242-243.
Thanks! The grid sizes of rfile are independent of the grid sizes in the computational domain (Petersson and Sjögreen, 2017). The minimum grid size in the computational grid depends on the desired maximum frequency. We have clarified the sentence in the manuscript and removed "vertical" from the description of the grid spacing of the computation grid since horizontal and vertical grid spacings are the same.

22. L. 287: The word "definitions" is repeated.
Modified.

23. L. 309-310: The reviewer could not understand correctly "We removed the outliers outside the whiskers to improve readability" because the top and bottom whicker corresponds to the maximum and minimum values, respectively. Thus, what is outliers outside the whickers? Does it mean that the maximum value is not the maximum? How did the authors judge these outliers?
Thanks for noticing this. We have modified the statement for clarity and added the definition of outliers.

24. L. 316: The PGD residual is defined for a station for a rupture model (Eq. 4a). How did the authors combined the PGD residuals from all stations for all rupture models to produce Fig. 6?
Thanks! We combined the residuals from all stations for all rupture models into a single dataset and binned with respect to the hypocentral distance.

25. L. 316: The equation $\delta_{ij,PGD}$ 2a should be equation 4a.
Thanks for pointing this out. We have modified it in the manuscript.

26. L. 318-320: There are no blue circle patterns, light blue circles, and orange diamonds in Fig. 6. Please check the figures.
Thanks for your observation. We have the sentence with boxplot and the corresponding patterns.

27. L. 320-322: Though the authors claim that PGD residuals do not change significantly with distance when they used different rupture models for the same earthquake, PGD residuals for MudPy 1D SRCMOD look almost twice as those for MudPy 1D Zheng in Fig. 6 (A).

Thanks for the comments. We have clarified the sentence in the manuscript. We observed that the PGD residuals for MudPy 1D Zheng model is lower than that of the MudPy 1D SRCMOD model, but we show later in the manuscript that the residuals for 3D velocity models are still lower than the corresponding 1D models.

28. Line numbers are missing from p.20.

29. 1st paragraph in p. 21: The authors wrote that shaking may be due to waveguide phenomenon is in the shallow slab within the low-velocity wedge. However, this sentence does not make sense. The low-velocity wedge is located above the plate interface near the trench, but the slab here is the subducting Pacific Plate, and it should be below the plate interface. Please describe the phenomenon more clearly.
    Thanks for the comments. It is true that the waveguide is within the low-velocity wedge. We have clarified the sentences about shallow slab.

30. Kaneko et al. (2019) is not listed in the References.
    Thanks for noticing. We have updated the reference list.

31. Bottom in p. 23: Why the Ibaraki 2011 earthquake was exceptional?
    Here, we are not intending to point out that it is "exceptional", but rather that it does not follow a similar trend; there is no particular standard for "exceptional" that we see here. However, we added a note to describe that potentially it is slightly different from the other models due to a source inversion effect from the SRCMOD model.

32. 1st paragraph in p. 25: Tokachi 2003 Hayes model is also exceptional in Fig. 10D as well as Ibaraki 2011.
    We have added Tokachi here.

33. Fig. 11: Legend for D Critical (solid blue line) is not consistent with the graphs (blue dash-dotted line).

    Thank you so much. We have corrected the legend.

34. p. 31: PDG should be corrected to PGD.
    Thanks! We have corrected the typo.

35. Data and Resources: The embedded link to https://github.com/oluwaseunfadugba/1D_vs_3D_HR-GNSS_CrustalDeformation did not work.
    Thanks! We confirmed that the link is working. We guess it's because of the underscores.

36. References: Hills et al. (2012) and Kanamori (1972) are not cited in this manuscript.
    Thanks for noticing. We have removed the references.

37. Text S2: The authors wrote that rfile is available on Zenodo. As far as the reviewer understood, this rfile contains the 3D Japan Integrated Velocity Structure Model (JIVSM) (Koketsu et al., 2008, 2009). JIVSM is a product of the Headquarters for Earthquake Research Promotion of Japanese Government, and it is not copyright-free. Did the authors get a permission to redistribute their velocity model from the Japanese government?
    Thanks for your comments. We downloaded the 3D Japan Integrated Velocity Structure Model (JIVSM) from the Strong Motion Group page of The University of Tokyo, where we found no copyright information, nor preferred publications to reference if used, and assumed this was acceptable to use as an open-source product. In good faith, we have cited all publications present on this site associated with the model, assuming these are the appropriate references to recognize the work. We are not familiar with any additional copyright or required permissions but would very much appreciate more information or specific contact information to be sure we are within acceptable bounds, if the reviewer or journal thinks we are not.

38. Text S3: In order to reduce memory usage, the authors increased the minimum shear wave velocity to 1200 m/s based on the average Vs in the upper 400 m. Does it mean that they replaced the velocity layer whose Vs is smaller than 1200 m/s with a layer of 1200 m/s? The reviewer felt that the procedure modify the minimum Vs in the velocity model is unclear.
    Thanks for your comment. SW4 allows user to set the P- and S-wave minimum velocity values in the simulations using the global material command, thus replacing the velocity layer whose Vp and Vs are smaller than threshold values with the threshold values. Yes, SW4 replaces the velocity layer whose Vs is smaller than 1200 m/s with a layer of 1200 m/s. We have clarified the procedure and included the minimum P-wave velocity we used (i.e., 2500 m/s) in the manuscript.

Reviewer B:

1. In general, the study is very well thought out and executed. It is very technically sound. However, there is an issue related to studying the residuals of the 3D/1D structures.
    - I assume that most of the mean rupture models in Table 1 are derived/inverted from 1D crustal models by other researchers (e.g. Kubo et al, 2013). Have the authors compared their 1D model (figure 3) with those researchers' 1D model?

      Thank you for the comments. We understand that the concerns, and have performed an additional analysis presented in the manuscript and supplementary figures to allay the concerns, finding that varying the 1D structure has significantly less of an impact on the modeled waveforms than including 3D structure does. To demonstrate this, we performed 1D simulations Ibaraki 2011 earthquake using the SRCMOD mean rupture model but using 1D velocity models used by the other source models, Koketsu et al. (2004) and Zheng et al. (2020). We compared the PGD residuals of the resulting waveforms using these 1D velocity models with respect to the observed waveforms. The comparison plot shows that the PGD residuals using these additional 1D velocity models are different in some sense but are not significantly different compared to the trend of residuals observed for the 3D simulations in Figure 9. Therefore, the significant deviation from the PGD residual in the 1D simulations is most likely due to the path rather than the choice of the 1D velocity model. These results are shown in Figure S6.

    - I understand that the many scenarios (Figure 2) show the range of residuals well (Figure 6) and the authors use the observed waveform as a reference for highlighting the impacts of the 1D and 3D structures. However, what if the source models' 1D model is inconsistent with the 1D model (Figure 3) adopted by the authors? If they are not consistent, the residuals between the modeled and observed waveform could be simultaneously caused by the different 1D models as well as the uncertainty of the source mean models. In other words, the 1D model of the slip inversion and waveform propagation should be consistent if the observed GPS waveforms are studied. Otherwise, there is a possibility that the mean source model is already biased to provide a different result.

      We have shown that the choice of 1D velocity model, even though different from the source models' 1D model, do not affect the conclusions and the PGD residuals in the 1D simulations are still very different from the 3D simulations. In addition, in Figure 6, we show that the choice of source model does similarly have some effect on the residuals, the magnitude of which is still much less than the effect of including 3D structure. We also add text in the manuscript (~p. 26) to point this out.

- I think the ideal case for designating the mean source models is by a) Inverting the observed GPS waveform using the adopted 1D and 3D model (Figure 3) for estimating the 1D and 3D slip solutions. b) Use the inverted slip solutions as the mean slip model to generate the fakequake scenarios. From these solutions, the residuals of the 1D and 3D solutions could be compared to highlight the superiority of the 3D solution. In fact, to underline the impact of 1D/3D structures, comparisons between 1D and 3D forward solutions might already suffice (from a few particular scenarios), instead of also involving the actual observed waveform.

  Thanks for suggesting the methodology, we appreciate it and agree that this is an important next step. However, performing the 1D and 3D slip inversions of the observed GNSS waveform for the earthquakes is above the scope of this study, and that these results are still worth presenting on their own. We are in fact currently working on another study to that focuses on comparing 1D and 3D slip inversions for motivating 3D Greens functions. Thanks!

2. Better statistics are needed to consolidate the meaning of being a "better fit" (line 22 in the abstract). How much better is statistically significant? For the instance of Figure 9a, the 3D model obviously fits better than the 1D model. This might be justified by statistical methods such as f-ratio. However, in Figure 9d, the same conclusion is less discernable. Maybe it is workable to compare the summation of the mean residual at each distance for justifying the "better fit"?

   Thank you so much for your comments. We determine if the 1D and 3D residuals are statistically different from each other (i.e., come from different distributions), we perform Komogorov-Smirnov (K-S) tests (Kolmogorov, 1933; Smirnov, 1948) on the 1D and 3D residuals for each earthquake. We show the results of the K-S test in Figure 10. Two distributions are significantly different when the statistical value (KS-stat) is above a critical value (Dcrit) which is a function of the number of samples of each distribution and when the p-value is below the significance level of 0.05. We reference this figure in describing better fit, however do not mention the figure specifically in the abstract itself.

3. How the fake quake scenarios are generated? What are the parameters controlling the scenarios deviating from the mean slip model? Figure 2 shows some scenarios and their end members. Are these within the uncertainties of the mean slip models? Usually, the scenarios might be characterized by defining a slip uncertainty of each sub-fault node. If this was done, please describe it since this would control the width of the boxes in, for example, figure 9.

   Thank you so much for your comments. FakeQuakes generates slip distributions from the perturbations around a known slip model given a target magnitude or mean slip distribution and a prescribed fault geometry. It uses a VonKarman correlation function

the covariance matrix employing correlation lengths between the subfaults associated with the rupture. FakeQuakes then uses the Karhunen-Loéve (K-L) expansion (LeVeque et al., 2016) to determine several nonnegative slip distributions by linear combinations of the eigenmodes of a lognormal covariance matrix that are sampled from a probability density function. Describing the process in its entirety is out of the scope of this paper as it has been described at length in the cited references. However, we agree it is important context, so we have included a brief description of how FakeQuakes works and the tunable parameters in the manuscript.

Additionally, yes, we used a uniform standard deviation of the slip (σ) value of 0.9 for all subfaults and set the limit on the peak value of slip to 40 m. The end members in Figure 2 are within the uncertainties of the mean slip models.

SECOND REVIEW ROUND

Letter from editor

Dear author,

Many thanks for submitting the revised version of your manuscript “The Impact of the Three-Dimensional Structure of a Subduction Zone on Time-dependent Crustal Deformation Measured by HR-GNSS”. I have now received two reviews regarding your re-submission. Both reviewers are happy with the changes made in the revised version. Reviewer A is still pointing out some minor points that should be corrected before we can formally accept your manuscript. In particular, the reference for the JIVSM model should be corrected to refer to the original website and credit should be given to the organization providing the data.

We will be pleased to formally accept your manuscript for publication in Seismica once these minor changes are made, and once we have received some manuscript source files so that we can immediately proceed to the Copyediting stage.

[…]

Kind regards,

Mathilde Radiguet

Note from editor :
This second round of review was not subject to a formal point by point response from the authors.

Reviewer A

The reviewer read the revised manuscript and the response letter by the authors and evaluated that the authors revised and responded sufficiently to the comments and questions by the editor and two reviewers except some minor points described below. The reviewer appreciated the authors for their efforts on improving the manuscript.

Comments on the revised manuscript. The line number refers to the markup version of the manuscript.

1) Table 1: Symbol characters for latitude and longitude are broken. Please check them.

2) L. 183: Melgar and Hayes (2019), which was added in the revision, is not listed in the reference list.

3) L. 199: Mena et al. (2010), which was added in the revision, is not listed in the reference list.

4) L. 254 (related to the comment 16 in the initial review): Hayes (2014) is still missing in the reference list. Is it Hayes (2017)?

5) L. 555: Komogorov-Smirnov tests might be Kolmogorov-Smirnov tests.

6) L. 859 (related to the comment 36 in the initial review): As pointed out in the initial review, Kanamori (1972) is not cited in this manuscript.

7) L. 902: Volume and page numbers for LeVeque et al. (2016) is 173 and 3671-3692, respectively.

8) L. 939: There are two entries for Petersson and Sjögreen (2017).

9) Text S2 (related to comment 37 in the initial review): The authors stated that they downloaded the digital data of JIVSM (Japan Integrated Velocity Structure Model) from the Strong Motion Group page of the University of Tokyo (https://www.eri.u-tokyo.ac.jp/people/hiroe/lik.html). However, it is impossible to download it from that web site because the website of the Strong Motion Group of ERI, University of Tokyo does not host or distribute any file about JIVSM. Thus, it is obvious that there is no copyright information on that web site because it is not a product of this website. As anybody can see the URL (link.html) of the website which the authors show in Data and Resource Section in the revised manuscript, this page is a collection of links for other websites related to strong motion studies. As seen in links, those digital files are hosted on the Headquarters for Earthquake Research Promotion of Japanese Government (their domain is jishin.go.jp).
(data files)

https://www.jishin.go.jp/main/chousa/12_choshuki/dat/nankai/lp2012nankai-e_str.zip

https://www.jishin.go.jp/main/chousa/12_choshuki/dat/nankai/lp2012nankai-w_str.zip

https://www.jishin.go.jp/main/chousa/12_choshuki/dat/nankai/lp2012nankai_str_val.pdf

(main page)

https://www.jishin.go.jp/evaluation/seismic_hazard_map/lpshm/

Therefore, the users of these files must follow the regulations by Headquarters for Earthquake Research Promotion. Their copyright information and contact information are provided at https://www.jishin.go.jp/agreement/ and https://www.jishin.go.jp/inquiry/, respectively. Unfortunately, these information is provided in Japanese language only.

Reviewer B

Comments:

This study aims to investigate the difference in the ground motion observed/simulated in 1D and 3D velocity models associated with four subduction-zone events in Japan. The backbone of the sensitivity analysis relies on the stochastic approach by generating numerous synthetic scenarios through "FakeQuake", while the test statistics are the residuals of PGD, T_{PGD}, SD, and Xcorr. The authors demonstrate that the residuals obtained by 3D simulations are generally smaller than those obtained by 1D simulations, especially at a farther distance away from the epicenter. This implies that the 3D simulations assemble a more realistic seismic-wave travel path that accounts for some additional wave features observed in the seismic time series.

This is a well-executed study with a thoughtful statistical analysis and huge computational efforts. The technical aspect of this work is incredible, given the multiple streams of methods and the large number of test cases and events being studied. Although 1D/3D sensitivity was first proposed elsewhere, this study showcases a very exclusive study of four actual events and their published slip models with statistically sound results (Figures 9 and 10). This consolidates the necessity of using 3D simulations over 1D simulations, for improving local tsunami warnings and beyond. Meanwhile, the writing format is clear and organizes the discussion in a very readable and logical manner.

I would like to also echo reviewer B regarding the velocity model being used in the slip model and forward 1D simulation. It appears that the slip models of the four events in Figure 6 are not based on the same type of 1D velocity, while the authors used the Hayes, 2017 model. But even though the slip models use different 1D velocity models (e.g. in the Ibaraki and Tokachi event), their trend of the residuals (Figure 6) is still quite similar systematically (Figure 6a and 6d), compared to the residuals in the 3D simulations (Figure 9a and 9d). This is further illustrated in the median residual difference in Figure 10. This suggests that the difference between 1D vs 3D simulation is discernably more important than the inconsistency between the source model's 1D velocity model and the adopted velocity in the 1D simulation. As such, I think the conclusion would not be affected in this aspect.

The inversion might be another important step to verify the 3D green's function would give rise to a better fit to the observed ground motion than the 1D green's function. However, the time series would involve other parameters (associated with inversion, smoothing, and/or Bayesian formulation) that might introduce extra complexity to verify the difference between 1D and 3D simulations. As such, I agree that doing the forward model is a simple first step to get this effort started. Furthermore, it is noteworthy that the 1D simulation could not recover a better time series than the 3D simulation, even though the mean slip models (e.g. from SRCMOD) are indeed estimated based on the 1D velocity model. This highlights that the 3D crustal path might exert a larger impact on the receiving ground motion than the "uncertainties" of the slip model resolved by 1D models. Following this logic, I would expect using the slip model inverted by 3D velocity models will yield an even better fit of ground motion time series than those presented in Figures 7 and 10, due to the fact that the linear inversion will further minimize residuals over distance and time. Although it would be nice to also see this kind of results, I believe it would be another step of the study, given the amount of computational efforts to be invested in dynamic slip inversion.

Overall, I suggest this manuscript be accepted and published promptly.