1. Introduction

1: Many reference deficiences - Grayzer, 1991, should be Graizer, 1991, moreover the corresponding reference in the list is not correct Hua and Zhang (2002), in the list with the year 2022, moreover the citation cannot be traced with either of those years Lee (2007) is missing in the reference list and it is not traceable anywhere on the internet Chen et al. (2014) is not traceable on internet (in the following I no longer list the deficiencies individually, but all references must be carefully checked and corrected (many other deficiencies detected in the text !))

Thank you for the comments. We have corrected the errors and omissions, carefully checked all references, and corrected any other potential deficiencies found.

2: Line 35, the sentence "Seismic rotational motions are recorded in plenty of earthquakes..." - the sentence sounds somewhat exaggerated, it gives the impression that registration of seismic rotational motions is quite common, which is not true. Rotational seismometry is constantly evolving and there is still no universally applicable and reliable rotational sensor. Each rotation record must therefore be carefully analysed.

Thank you for the comments. We have revised it to a more cautious wording to better reflect the current situation more accurately.

3: Lines 38-39, the term 'rotational torsions' is not defined.

Thank you for the comment. The term "rotational torsions" refers to the rotation around the vertical axis (rotational z-component) caused by seismic waves, which we have explained in the manuscript.

4: Lines 47-48, "...observed rotations during strong ground motions exceed calculated translational components by one to two orders of magnitude" Seismic rotations and translations represent totally different types of motion, measured in different units (e.g., m/s vs. rad/s). Thus, simple comparing their amplitudes does not make sense. The above statement cannot even be supported by the literature.

Thank you for the comments. We are not referring to a comparison of the magnitude of the translational and rotational components, but to a comparison between directly observed rotational motion and rotational motion indirectly derived from arrayed translational components, especially in the case of strong and near earthquake, where the directly observed rotational motion is 1 - 2 orders of magnitude stronger than the indirectly observed rotational motion. We have improved our wording to avoid any potential misinterpretation.

5: Line 66 "... vertical rotation ..." What is it? Rotation about the vertical axis or rotation in a vertical plane? Please be specific.

Thank you for the comments. We made it clear in the text that 'vertical rotation' refers to rotation about the vertical axis.

2. Theories

6: Fig. 1 does not correspond to the explanatory text. Why the vectors dx and dx' are not shown in the figure (when mentioned in the text) and ds and ds' are used instead? The notation is boldface indicating vectors, but they are scalars (defined as distances).

Thank you for the comments. We have updated Figure 1 to be consistent with the description in the text and corrected inappropriate notation.

7: Line 91 "The following equations and tensors are written using the Kronecker symbol ..." I do not see the Kronecker symbol in any of the following equations.Eq. 1 ... i,j=x,y,z... Strange notation, please write i=x,y,z and j=x,y,z (likewise in Eq. 13) Line 98, notation inconsistency: uppercase X, Y, Z, but in Eq. 1 and below lowercase x,y,z (and likewise in the following text).

Thank you for the comments. We have revised the use of symbols in the equations and have used uniform characters to represent the coordinate axes.

8: Line 131, using term 'small deformation' here (and likewise in the following text) is misleading in the given context as it implies neglecting the higher-order terms, including the second-order ones.

Thank you for the comments. We understand the possible misunderstanding of the term "small deformation". Indeed, in conventional understanding, it is often associated with the hypothesis of small linear deformation that ignores higher-order terms. To avoid misunderstanding, we will seek to make this point more explicit in our manuscript and use more precise wording to ensure that readers can understand the content of our study and the theoretical framework used.

9: Eq. 10, mixing indices i,j,k and x,y,z, please clarify

Thank you for the comment. We have corrected the use of symbols to use consistent characters to represent the axes.

10: Line 162, ... Rxz corresponds to..., but in Eq 11, there is no Rxz. Also boldface typing is inappropriate when denoting tensor components. Why the uppercase symbol is used when we see lowercase in Eq. 4.? Please unify the notation.

Thank you for the comments. We have changed the text, removed the inappropriate bold, and standardized the capitalization of physical quantities.

11: Line 181, " ... to ensure the free-surface condition at the upper boundary ..." Under the free-surface condition, provided z is vertical, the rotational components Rx and Ry simplify to individual space derivatives instead of their linear combinations (differences). This would be a good test of the numerical approach adopted.

Thank you for the comments. The acoustic boundary replacement method is widely accepted for efficiently dealing with the reflection of seismic waves at free surfaces and accurately modeling the effect of the ground surface. We didn't use the free-surface rotational motion definition equation because we obtained the numerical solutions for both translational and rotational motion simultaneously at all model grid points using the acoustic boundary method. Based on the setting formula of the free surface conditions, the influence is applied to the simulation, and numerical solutions at the free surface are obtained. Of course, this is indeed a good test method, and we will test both free boundary simulation methods further to verify the validity and reliability of the results.

12: Line 182 (and Fig. 2) ... speaking about upper boundary implies that x is the vertical coordinate (not z, as usual)

Thank you for the comment. The upper boundary is a common expression for this model, which we will describe in the text as setting the free surface conditions at the corresponding position of the z-axis of the model according to setting formulas.

13: Eq. 13 and the text below, there is no Mij in the equation. Please introduce Mij more strictly (from the mathematical point of view).

Thank you for the comments. We have optimized the introduction of Mij.

14: Line 191, a reader not familiar with seismic source represeantations may be surprised by 'force direction' and 'force arm' without previously mentioning force pairs and dipoles.

Thank you for the comments. We agree and have removed this potentially unnecessary introduction.

3. Wavefield simulation

15: Figs. 3, 4 and 5: I'm not convinced that the non-linear effects in the figures are realistic, especially since this is a homogeneous isotropic model. Rather, I suspect that we are seeing the effect of numerical errors. An analysis of the accuracy of the computational scheme used is unfortunately lacking. However, the presence of numerical errors is evident, e.g., in the S waves for the ISO source. Since in the linear case such a source does not generate any S waves, it is impossible for the relative difference to be finite. The analysis of numerical errors is extremely important when dealing with amplitudes as small as those typical of seismic rotations. According to the theory, the rotation associated to P waves must be zero in a homogeneous, isotropic, unbounded medium assuming a linear strain tensor. Again, the fact that we see a finite relative difference between the linear and nonlinear cases is due to numerical errors. An analysis of the errors for the linear case could be made by comparison with the analytical solution known for the homogeneous isotropic unbounded medium. Next, I think there is some problem with the time evolution of the wavefield. How is it possible to see the S wave in the snapshots (when it is generated, e.g., by DC) if it propagates at 3 km/s and therefore cannot reach the distance of 30 km in 8 seconds? Also, the wave between the P and S waveforms in Fig. 5 left is very suspicious. One last note: the pictures show the amplitude values without specifying the 'source strength' (or initial amplitude).

Thank you for the comments. We would like to clarify and respond to the concerns regarding Figures 3, 4, and 5

(1) Comparative validation and error analysis: The reviewer's comment that the nonlinear effects may reflect numerical errors is very pertinent. We plan to improve this by (a) benchmarking the simulation method (in linear case) to verify the accuracy of our simulations. (b) Evaluating and analyzing numerical error, quantifying it and using a more accurate and refined mesh to reduce the numerical error during simulation.

(2) S-wave propagation time: We have checked the simulation settings and confirmed that the S-wave velocity is 3 km/s, the time of the wavefield snapshot is the 8th second, and the S-wave in the figure has not yet reached the observation point 30 km away. The misrepresentation here may be due to poorly selected or labeled time slices in the graphical display. We will readjust the time slices to ensure a clear presentation.

(3) S-waves in ISO source simulations: We agree that ISO sources do not generate S-waves, and according to linear theory, there is only P-wave propagation in homogeneous isotropic media due to the volume change associated with a pure pressure field. In numerical simulations, due to the inherent presence of numerical errors, the numerical results may show non-zero values even at locations where no S-wave propagation is theoretically expected. This situation can lead to bias in the comparison results when the relative change rate is subsequently calculated. This comparison method suffers from a lack of accuracy. Therefore, we will actively seek a more accurate and reliable comparison method to ensure the accuracy and reliability of the results.

(4) P-waves in rotational component: Indeed, P-waves are usually described as pure compressional waves. Numerical approximations and discretization grids during simulation may introduce small unphysical effects. These effects may become significant at high accuracy requirements, or perhaps the forces are loaded in such a way that there are residual wavefields that lead to the observed P waves of the rotational component signals. We will make further investigation on this.

(5) Source intensity description: This is our oversight, and we will clearly introduce the source intensity parameters in the revised version.

16: Line 282, "...wavefield energy..." What exactly do you mean by this term? Give the corresponding formula.

Thank you for the comment. We have added the wavefield energy calculation formula. Regarding the 'wavefield energy', it is an approximate total energy by calculating the square of the wavefield value at each grid point and summing, which is calculated by the following formula:

$$E = \sum_{i,j,k} \left| u_{i,j,k} \right|^2 \Delta V_{i,j,k}$$

where $u_{i,j,k}$ is the wavefield value (which can be a physical quantity such as displacement or velocity) at the grid point (i,j,k) and $\Delta V_{i,j,k}$ is the volume of the unit grid. This calculation is based on the fundamental principle that the energy is proportional to the square of the amplitude, and we use it as an approximation for the wavefield energy in discretized grid systems.

17: Fig. 6b, For larger magnitudes (i.e., larger faults) the point source approximation may not be acceptable at the distance of 30 km.

Thank you for the comments. We understand that in practice the sources of large earthquakes usually have complex geometries and slip distributions, and these properties cannot be adequately characterized in point source approximation. However, our work focuses on the impact of nonlinear effects on seismic waves rather than on accurately describing the sources themselves. Therefore, we chose the point-source approximation as a starting point that is reasonable and effective given the goals of this study. We are also aware of the limitations of the point-source approximation, which we will explain in the discussion section. In future research, we plan to further explore more complex seismic source models (such as finite fault models) to more accurately evaluate the performance of nonlinear effects under different seismic source conditions.

18: Line 313, ...(Wessel et al., 2019) refers only to the mapping tool. Please cite where the locations come from.

Thank you for the comment. We have made it clear in the text.

19: Line 313, ..."The receiver for E2...." Please give the name of the station (QS01?) and its instrumentation (the same for NA01 providing records of E1).

Thank you for the comment. We use Blueseis in the QS01 station for E2, which we specified in the revised manuscript.

20: Line 315-316 (and Fig. 7b), "Additionally, a seismic array comprising seven 3C translational seismometers was deployed approximately 53 km from the epicenter of E1". It is not clear whether you use only the NA01 record or records from the whole array in your study. In that case provide at least array apperture and basic instrument specifications. If the array is not used, Fig. 7b is irrelevant.

Thank you for the comments. We analyzed the seismic data from the NA01 station, not the entire seismic array. Figure 7b shows the array deployment, which provides the experimental context, not the direct object of analysis. We have clarified this information in the relevant part of the text and made appropriate changes to Figure 7b to ensure that readers are not misled.

21: Eqs. 20, 21, Why new coordinates r,t,p are used? (not defined, not consistent with the previous equations and not used in further calculations)

Thank you for the comment. We have represented these tensor components as expressions of the coordinates x, y, z.

22: Tab. 1 caption, "...observing stations". Plural form indicates that the same 1D structure is used for both stations (more than 300 km apart situated at different 1-degree cells of the CRUST1.0 model). Is the structure so slowly changing to allow for that. Please comment on that.

Thank you for the comments. Regarding the use of the same one-dimensional (1D) structure model for two stations more than 200 km apart in this study, we have the following main considerations:

(1) Aim of the study and simplification: Our main objective is to study the effect of different source mechanisms and seismic wave propagation distances on the nonlinear errors of seismic records by simulating the two earthquakes. To reduce the influence of other factors, we decided to use the same stratigraphic model in the simulation. Although there may be differences in the details of the actual media, we are concerned with the nonlinear effects during seismic wave propagation rather than the local details of the stratigraphic structures. These differences are not a major comparative factor for the nonlinear features of interest in our work, and this simplification has been adopted given the relative stability of the structures at larger scales. By adopting a unified medium model, we can more easily identify the effect of source mechanism or propagation distance on nonlinear errors in the seismic record.

(2) Stability of crustal structure: Crustal structure changes relatively slowly on macroscopic scales (e.g., a few hundred kilometers), especially in regions without significant influence of geological activity. Therefore, it is reasonable to assume that the two observatories are in a similar crustal structure context, despite their distance from each other.

We are also aware of possible differences in crustal structure at the microscopic scale and the effect that these differences may have on the propagation characteristics of seismic waves. In future studies, we plan to further refine the crustal model to account for differences in crustal structure between different observatories and to more fully assess their impact on the nonlinear effects in seismic records.

23: Tab. 2, (53 km, :, 0 km) ? Shouldn't it be (53 km, 4 km, 0 km)?

Thank you for the comment. We have changed this to make it clear that an observation system is along the entire line in this dimension.

24: Tab. 2, Grid interval 1 km is more than about 1/4 of the S-wavelength and about 1/6 of the P-wavelength. Isn't it too sparse grid for finite spatial differences needed to evaluate rotations? (The situation is even much worse for model2 in Tab. 3)

Thank you for the comments. We will evaluate the numerical error of the simulation before deciding whether to further reduce the grid spacing.

25: Line 347-349, The general statement is true, simply speaking the higher the frequency is, the larger rotation amplitude we usually observe. However,I do not understand the argumentation. The prevailing frequency in real records is higher (for body waves) than that of the synthetics, thus, no surprise that we see larger rotation amplitude.

Thank you for the comments. Observed data tend to be richer in high-frequency signals than synthetic data with a single dominant frequency, and this can indeed be an important factor in the difference in magnitude between the observed translational and rotational magnitudes and the synthetic translational and rotational amplitudes. Although the propagation medium is elastic in the simulations, the real medium tends to be viscoelastic, leading to absorption of seismic energy and attenuation of high-frequency signals. Furthermore, the mechanism behind the phenomenon of more pronounced rotational signals at high frequencies may involve many complex factors, such as the source characteristics, the propagation path, and the local geological conditions at the receiver site. The interplay of these factors provides new perspectives and rich scientific research space for the in-depth study of high-frequency rotational signals, which is worthy of further exploration in future work.

26: Fig. 8a - seems to be incorrect for three reasons: 1) strange dominant translational S-wave amplitude on z-component (I would expect the largest S-wave amplitude on y-component, as it is almost in transverse direction), 2) Rx much larger that Ry. According to Tab. 2 and the text bellow, x coordinate is almost in the radial direction. Rotation about x thus should be negligible (at 53 km), Ry should be much larger! But Fig. 8a shows the opposite (no reason for that in the simple 1D structure), 3) synthetic rotational components should have higher frequency content than the translational components (rotation rate proportional to acceleration)

Thank you for the comments. We will adjust the parameters, and after adjustment, we will rerun the simulation and analyze the results to obtain more reliable and convincing results. We will seek reasonable explanations and justifications if the adjusted simulation results still show "unintended" or "strange" phenomena.

27: Fig 8b, (technical comment), It is not logical to show P- and S- onsets in iasp91 when the authors consider the CRUST1.0 model in their simulations.

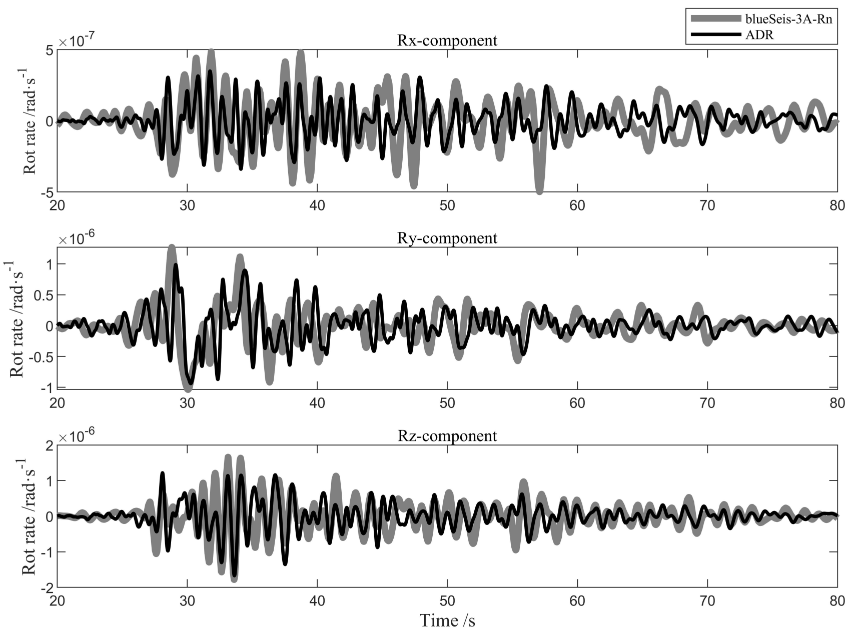
Thank you for the comment. In the revised manuscript, we have removed the P- and S- onsets in iasp91.

28: Fig 8b, Horizontal (translational) components are more or less comparable (not significantly stronger) to the vertical one. Moreover the references Abercombie (1997) and namely Guatteri et al., (2001) are totally inappropriate in the given context.

Thank you for the comments. We have modified our statements and re-evaluated the references to Abercombie (1997) and Guatteri et al. (2001) to ensure their applicability.

29: Fig 8b, (general comment), As mentioned above, none of the presently available rotational sensors can be considered as an 'ethalon' producing reliable rotational records under all conditions. Therefore it is worth verifying the records whenever it is possible. In the case of NA01, there are even two possibilities: ADR (array derived rotations) method utilizing Nanao array records, and matching the rotational rate components to the related translational acceleration components (for the frequency range considered it should be feasible). In case of a waveform mismatch, either the records are wrong or the structure is significantly laterally inhomogeneous. In any case it would call into question any comparison of seismograms in Fig. 8a and 8b.

Thank you for the comments. We fully understand your concern about the reliability of the data recorded by the rotation sensors and agree with the importance of validating the records. For the E1 observation record, we have compared the ADR-derived rotational motions with the rotational motions directly observed at the NA01 station, and the results are in good agreement (please see the following figure). Although both methods have their strengths and limitations, deriving rotational motions from translational seismometers involves approximations and may introduce errors and the direct measurement is a more direct and potentially less error-prone way to capture rotational motions, especially in cases where the structural integrity and homogeneity of the observation site is well established.



30: Tab. 3, Grid interval 5 km is comparable to the S wavelength and about one half of the P wavelength, i.e., too big for spatial final differencing.

Thank you for the comments. We will evaluate the numerical error of the simulation before deciding whether to further reduce the grid spacing.

31: Tab. 3, (:, 0 km, 0 km) ? Shouldn't it be (100 km, 0 km, 0 km)? (Line 370)

Thank you for the comment. We have changed this to make it clear that an observation system is along the entire line in this dimension.

32: Line 385-388, "That is consistent with previous studies that have argued that the observed rotational components have a relatively stronger amplitude than the rotational component converted from translational components (Teisseyre et al., 2003)." Teisseyre et al. (2003) make no such general claim. They comment only on one case, where, moreover, the rotations were measured by a method which is nowadays outdated. What do you mean by "converted from translational components"?

Thank you for the comments. We have corrected the citation. Lee (2007) pointed out that the directly recorded rotational motions in Japan and Taiwan under strong and near-field earthquakes are one to two orders of magnitude larger than the rotational motions derived indirectly from translational accelerometer arrays. This difference may be partly due to errors introduced during the differential calculation process, but it is more likely mainly due to complex factors such as nonlinear elastic or site conditions at the receiving point affecting the seismic rotation. The inaccurate expression in the original manuscript, "converted from translational components," refers to the rotational motion derived from records of translational accelerometer arrays, which we have corrected.

33: Fig. 9a - seems to be incorrect for several reasons: 1) high frequencies on Vz after 180 s (surface waves), the same for Ry, 2) synthetic rotational components should be of a higher frequency content compared to the translational components (rotation rate proportional to acceleration), 3) prevailing frequency seems to be smaller than the declared 0.5 Hz (~ 0.3 Hz), 4) It's a pitty that the theoretical S-onset is not shown, I expect, it is at about 90 s. It is again really strange that the S-wave amplitude is that much stronger on the vertical component than on the horizontal ones.

Thank you for the comments. We will adjust the parameters, and after adjustment, we will rerun the simulation and analyze the results to obtain more reliable and convincing results. We will seek reasonable explanations and justifications if the adjusted simulation results still show "unintended" or "strange" phenomena.

34: Fig. 9b, (technical comment), It is not logical to show P- and S- onsets in iasp91 when the authors consider the CRUST1.0 model in their simulations. Fig. 9b, From where the rotational components come from? In case of the NA01 station, blueSeis rotational sensor was mentioned, but no mention about any rotational sensor in the station that records E2.

Thank you for the comments. We have removed the P-waves and S-waves onsets in revised version. For recording E2, we use blueSeis to record rotational motions.

35: Fig. 9b, As expected, rotational components are more high frequency than translational ones. Nevertheless, it would be worth 'verifying' rotational records by matching waveforms to the relevant acceleration components.

Thank you for the comments. The E2 observation station QS01 is located inside a cave at the Qingyuan Mountain Seismic Station in Quanzhou, Fujian. This location provides a relatively stable and low-noise environment, which is beneficial for the accuracy and reliability of the rotational recordings. In subsequent work, we will consider matching the rotational waveforms with the relevant acceleration components for further verification.

36: Line 408 and Fig. 10, "...Vx and Vy components, with errors up to 10 %...." Fig. 10b shows even much bigger errors: Vx reaching 15% and Vy probably significantly exceeding 15%! I consider this to be unrealistic taking into account the distance (327 km, far from the earthquake focal zone) and simplicity of the structure (isotropic 1D model with homogeneous layers). If we believed that is true, we would have to question all existing seismology!

Thank you for the comments. Considering the distance of the observation site from the epicenter (327 km) and the fact that we used a relatively simple model (1D isotropic homogeneous layered model), this error level was indeed beyond our initial expectations and prompted us to re-examine the situation. We plan to validate and compare the simulation results by analyzing the error and improving the model. If the error remains high, we will investigate possible physical mechanisms or unidentified confounding factors and conduct more in-depth research and validation before reaching a reliable conclusion to ensure our work adds value.

37: Line 417-419, I do not understand the argumentation - speaking about Fig. 10 we speak about relative errors (in %) in the synthetic example, it has nothing to do with an amplitude decay on rotational components.

Thank you for the comments. We want to emphasize that, whether in simulations or actual observations, the rotational component in far-field records of natural earthquakes typically shows a faster amplitude decay trend than the translational component. For this reason, the accumulation of nonlinear errors in the rotational component may be relatively small. When discussing the relative errors in Figure 10, we did not directly address the issue of amplitude decay of the rotational component. We will modify relevant expressions in the manuscript to ensure that they accurately convey our research content and findings.

5. Discussions and 6. Conclusions

38: I do not consider the claims made here to be relevant until the analyses, tests and corrections suggested above have been carried out. General comment: the influence of much more significant factors such as lateral structure inhomogeneity, anisotropy, attenuation, etc. should be investigated before any consideration of nonlinear effects in real seismograms based on simple simulations in 1D models composed of homogeneous layers filled with isotropic and perfectly elastic material.

Thank you for your thorough review and valuable comments on our research manuscript. We fully understand that studying the nonlinear effects of other key factors in depth is necessary. These tasks are indeed necessary and important for our understanding of nonlinear wave propagation. After consideration, we will rewrite the conclusion and discussion of the manuscript to point out the directions for further research that we need to do in the future, especially your suggestion to analyze and consider more significant factors. We are well aware that there are still shortcomings in the manuscript, but your valuable opinions have pointed us in the direction of improvement. We sincerely thank the reviewers for their attention and contribution to our work, and we will revise according to the above suggestions.

We look forward to presenting more accurate and complete research results in subsequent version. For language issues, we will seek professional language services to improve the English expression to ensure the readability and academic standards of the manuscript.