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Simulation characteristics of seismic translation 1and rotation under the assumption of nonlinear small deformation
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REVIEW

The paper deals with a generally interesting topic, namely nonlinear effects on both the translational ground motion components
(which are the domain of traditional seismology), and on rotational components. Rotational seismology is currently a rapidly developing field,
hence the subject is highly topical. The authors investigate the numerical solution of the equation of motion with a strain tensor that includes some
nonlinear terms in addition to the linear ones and compare this with the 'classical' solution considering linear strain tensor that is commonly used
because of the small-deformation assumption. For numerical simulations they use a staggered grid finite difference scheme. The authors then
confront the simulations with the records of two selected Taiwan earthquakes.

Unfortunately the paper is not well written and will require significant refinement in a number of respects. The level of English is not good and often
makes it very difficult for the reader to understand the meaning. The quantities and equations should be better described in accordance to
mathematical conventions. The numerical models do not appear to be properly tested, and there is a lack of numerical error estimates.
Comparisons with real data from two selected Taiwan earthquakes are not convincing. The references show deficiencies or errors.
In my opinion, the paper requires MAJOR REVISION.

Comments

1. Introduction

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 !))

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.

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

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.

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

2. Theories

(I don't derive the equations, so I can't comment on whether they are correct.)

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).

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).

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.

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

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.

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.

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

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

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.

3. Wavefield simulations ...

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).

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

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

4. Seismic observations and simulation ...

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

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).

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 wheather 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.

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)

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.

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

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)

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.

Fig. 8a - seems to be incorrect for three reasons: 1) strange dominat translational S-wave amplitude on z-component (I would expect the largest S-wave amplitude on y-componet, as it is almost in transverse direction), 2) Rx much larger that Ry. Acoording 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)

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.

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.

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.

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.

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

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"?

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.

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.

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.

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 structute (isotropic 1D model with homogeneous layers). If we believed that is true, we would have to question all existing seismology!

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.

5. Discussions and 6. Conclusions

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.