Response to Reviewers - SEISMICA

When you are ready to resubmit the revised version of your manuscript, please upload:

- A 'cleaned' version of the revised manuscript, without any markup/changes highlighted.
- A pdf version of the revised manuscript clearly highlighting changes/markup/edits.
- A 'response-to-reviewers' letter that shows your response to each of the reviewers' points, together with a summary of the resulting changes made to the manuscript.

If you deem it appropriate, please check that the revised version of your manuscript recognises the work of the reviewers in the Acknowledgements section.

Dear Editor,

we revised the manuscript and followed the suggestions of the reviewers. Some comments might need to be reposed if not answered sufficiently.
An acknowledgement of the reviewer's effort is included in the revised manuscript.
The revised manuscript with comments has the changes indicated in blue-colored text.

We are grateful for your efforts and are looking forward to hearing back from you soon.

Kind regards,
Andreas Brotzer & Co-Authors

Reviewer A:

In this manuscript, Brotzer et al. report on the deployment of a rotational rate seismometer at the Piñon Flat Observatory (PFO) in Southern California. It's placement next to a three-component broadband seismometer jointly forms a six degrees-of-freedom station, which itself sits within a large seismic array. The authors use this instrumentation to compute array-derived rotations and validate direct observations made by the rotational sensor, using seismicity in Southern California with a particular focus on two earthquakes of M4.1 and M6.2. They conclude by encouraging further instrumentation development for rotational sensors as they are currently limited by their self-noise level.

Altogether, this was a really enjoyable manuscript to review as it is very informative and well put-together. While I am not an expert in rotational seismology, I can recognise this work will certainly help inform other observatories who are considering deploying rotational sensors, including here in New Zealand. I cannot find any major issues that the authors need to address but I have a small list of items that I have detailed below. Once these have been addressed, I am sure this manuscript will be ready for publication in *Seismica*.

Oliver Lamb

Te Pū Ao | GNS Science

o.lamb@gns.cri.nz

Dear Oliver Lamb,

we appreciate your thoughtful review of our manuscript and your recommendation for publication. Your comments helped to further improve our manuscript.

Andreas Brotzer (and Co-Authors)

Comments

Line 33: Are you missing a word between 'enables' and 'to', perhaps 'us'? (i.e. This enables us to …)
> added: us

Line 37: It seems like you are missing a sentence in your non-technical summary that describes your conclusions?
> added paragraph:
This relation supports future experimental designs towards expected resolvability with a sensor of similar resolution. We conclude that for this first installation of a permanent six degree-of-freedom station in southern California, especially local seismicity is observable. For a sufficient signal-to-noise ratio of seismic events, six degree-of-freedom analysis methods can be successfully applied.

Line 63: Are you missing a word after ‘recent’, perhaps ‘developments’?
> changed to: Until recently,

Line 72: I might suggest you also reference Eibl et al. (2022) as another example of a short-term deployment of a rotational sensor.
> good suggestion. Added the reference

Line 84: At what depth underground is the granite pillar inside the vault (i.e. how deep are the sensors in the vault)?
> details of the pillar and depth below the surface are provided in the text

Line 95: It would be useful to add details on the sensors used in the array (i.e. PY.BPH*), and their sampling rates.
> details are added and a reference provided for further details.

Line 105: Conventionally it is written as ‘STA-LTA’, not the other way around.
> adopted the conventional terminology

Line 106: Can you provide details on the parameters used for the STA-LTA coincidence trigger, particularly the window lengths used and the trigger-on and -off thresholds?
> the STA-LTA trigger settings are specified in the text.

Figure 2: I suggest you use different colormaps for magnitude and depth, to make it clearer you are displaying different things in each panel. The maps in panels c and d are a little hard to read, so I suggest you remove the transparency on the detected events (and add transparency on the grey non-detected events). I’m not sure the red coloured areas for major urban areas are needed.
> changes were adopted to enhance Figure 2

Figure 2 caption: I suggest you used ‘detected’ instead of ‘triggered’, as the latter can be misunderstood to be related to earthquake triggering instead of the STA/LTA coincidence triggering.
> suggestion was adopted

Figure 3: It could be useful for readers to provide an additional supplementary figure showing how these events appeared on the co-located translational sensor.
> an additional plot for translation recordings is provided in the supplements

Line 231: Are you missing ‘is’ between ‘identification’ and ‘difficult’?
> added: is

Line 273: Missing a period at the end of a sentence here.
> added period at the end

Line 336: Typo of ‘epicentral’.
> corrected typo

Conclusion section: I believe some of the text here is better suited in the discussion section, particularly lines 337 to 346. You shouldn’t really use the conclusion section to introduce things that have not already been mentioned previously.

> Thank you for the suggestion. The conclusions and disscussion section has consequently been modified.

References

Eibl, Eva P. S., Martina Rosskopf, Mariangela Sciotto, Gilda Currenti, Giuseppe Di Grazia, Philippe Jousset, Frank Krüger, and Michael Weber. “Performance of a Rotational Sensor to Decipher Volcano Seismic Signals on Etna, Italy.” Journal of Geophysical Research: Solid Earth, 2022. https://doi.org/10.1029/2021JB023617

Recommendation: Revisions Required

Reviewer B:

review to

On single-station, six degree-of-freedom observations of local to regional seismicity at the Pinon Flat Observatory in Southern California

by A. Brotzer et al., submitted to *Seismica*

The paper presents a data base of local to regional recordings of rotational ground motion. It compares the direct observations to ADR and provides results of a 6DoF analysis including a relation for a rotational magnitude scale.

The presented study is of fundamental importance for the community of rotational seismology because it lays some basic foundations and provides orientation for the design and execution of future studies.

The paper is well written and the methods are clearly described and performed throughoutly. The discussion and conclusion are well thought out. I only have minor comments and therefore **recommend a minor revision**.

In the following I provide some more detailed comments. In case of questions, the authors are welcome to contact me.

Best,

Stefanie Donner

Dear Stefanie Donner,

thank you for carefully evaluating our work and recommending it for publication. We appreciate this detailed review of useful suggestions to further improve our manuscript.

Andreas Brotzer (and Co-Authors)

General comments

For the derivation of the velocities the components have been rotated into the ZRT coordinate system. I am wondering why this is not done for the entire paper. That would support the interpretation of e.g. Fig. 3, 5, and 6.
> Figure 5 and 6 intend to compare direct and derived rotations. Hereby, the rotation of coordinates does not make a difference. This is only important for a comparison of translation and rotation. For Figure 3 merely intends to show a set of events and their characteristics. As no direct comparison with acceleration is performed, that is why we do not use coordinate rotation here.

Please, go through the entire paper again and check the consistency which frequency bands have been applied and which have been mentioned in the text and captions, and labeled in the figures.
> both, the text and captions has been carefully checked to synchronize the specified numbers.

There are two events selected to focus the analysis on. For the Mw 4.1 the location and mechanism is visible in Fig. 1 but not for the Mw 6.2. Any chance to provide better orientation here?
> The Mw6.2 event location is outside the set boundaries of the map. Providing a good overview and detail on the nearby events was prioritized instead of including the remote Mw6.2 event. Also, the shown extent of the map also defines the used geographic constraints used for the catalog subset for which the trigger has been applied. The exact location is not necessary, since later the theoretical backazimuth is provided for reference.

Both, the Mw 4.1 and Mw 6.2 events are arriving more or less from south. As the author states in L206-209, theoretically most of the rotational motion should be visible on the EW component (Rayleigh energy) and on the vertical (Love) with almost no rotation on the NS component. However, Fig. 5 to Fig. 7 show clearly energy on the NS component in both, direct and ADR, in almost all frequency bands. How do the authors explain this discrepancy?
> For the main frequency band the energy on the east component is stronger than on the north component (as expected). The seismic energy on the north component most likely results from scattering of the local subsurface or topography (especially for higher frequency content).

Abstract

L25 – maybe add magnitude range (if yes, then also in L33)
> not considered relevant here

L26-28 – If you could add the term „magnitude“ somewhere in this sentence, it becomes more clear what you are talking about.
> included magnitude to clarify

Dataset

L99 – Sentence refers to Fig 2 a and b but geographic distribution is shown in c and d
> clarified the sub figure content

L109 - It is interesting that whether coincidence 4 or 6 applies seem to not depend on magnitude, event depth, or event-station distance. So what factor influences when/why the rotational components are triggered?!?!? (That is just a comment not part of the review …)
> yes, this is indeed intriguing. Not entirely understood or investigated. Perhaps this is related to the radiation pattern and/or local scattering. Some smaller or distant events are in

amplitude close to the noise level, therefore also random noise interference might diminish amplitudes below the trigger level for one component and not the other.

Sec 3.2 – It is not needed to discuss all possible factors influencing ADR. However, adding a short sentence that the number of stations used for ADR is also important might be useful (i.e. the more stations, the better influencing factors average out). This aspect could be interesting with respect to Figs 5+6. On the other hand, you would open up an entire new discussion point, especialy with respect to L138-140. Well, your decision …
> geometric constraints and assumptions are briefly mentioned in L125 - L 129. A more detailed discussion would indeed open up a new topic… since we cannot clearly distinguish or assign different contributions to ADR effects we would not continue this here.

L143 – „and“ twice in one sentence. Please rephrase. Suggestion: „as well as“
> adopted

L144 – Why 15 sec time window? According your Fig 3 that is too short to capture the maximum amplitude for all events farther than … ~80(?!, wild guess) km.
> this is relative to the trigger time. Hence, 15s after the trigger time should include the peak amplitude as well. This was set based on manual selected events.

L155/156 – Very important point here, you and I are quite familar with. Rotational newbies probably not. Maybe add examples, e.g. the SNR for the Mw 4.1 event you analysed in more detail, in a more appropriate (higher) f-limit and add a sentence here.
> good point. I included Figure 7 for this purpose. This demonstrates the frequency content towards frequencies (>5Hz). Since it is later introduced, I didn’t want to cross-reference the figure here.

6 DoF analysis

L161 – It would be great to see the location of this event in the map as well.
> As stated above, this would require a new map since the purpose of the map in Figure 2 is to also constrain the geographic data area of activity. This event is an outlier. I specified this in the text.

L165 – That means, you had only 3 sensors available for i-ADR? So, the low cc values for the 1-5 Hz band can have two reasons: a crappy ADR result due to lack of data and the high magnitude not exciting enough high-f energy. Maybe you want to precise your sentence in L168 accordingly.
> good suggestion. I added a sentence to specify how this partially explains the low waveform match.

L181/182 – citation needed
> specified in the text. Citation of ADR Spudich et al. (2009) added.

L189-202 – explanations needed for the choice of component combinations shown in the two top subpanels of Fig 8 and 9
> combination have been adopted to surface waves.

L191 – „overlapping [time] windows along the event. The [time window with] maximum …“
> this has been adopted and specified further in the text

L195 – same as L191
> has been adopted

L211/212 – Please, explain the choice of component combination in the two top panels of Fig 8 and 9 in more depth.
> the combination of components has been changed to the standard.

L229 – Please, cite Kurrle et al. 2010 here as well.
> added reference

L230/231 – Equations 1 and 2 have been derived and applied mainly to continental/teleseismic distances and surface wave energy so far. You are working in a rather local distance range, low magnitudes, and in rather high frequencies which are untypical for surface waves (btw: have you filtered here?). So, I guess, this is where your „phase identification difficulties“ come from.
I am afraid you need to justify that your approach is still valid. The resulting velocities makes me confident it is. To support your argumentation, how about preparing some supplement plots similar to Fig. 11, either picking random events from your data set or taking the exact same events as in Fig. 3 (for more consistency)?
> We focus on the exemplary events because they have a good S/N ratio for lower frequencies. Most local events only provide energy for higher frequencies (> 5Hz), which is why we only use peak amplitudes here for rough analysis (as a demonstration). The assumptions of plane waves and fundamental modes are not sufficiently justified for the high frequencies. Therefore, we cannot expand the analysis to these events.

L231 – „difficult[ies]?“
> correct

L235 – check grammar (Maybe only delete the „the“?)
> adjusted

L236 and following – Are there independent information on phase velocities available for the area from literature? Fig 12 shows quite a large scatter and a comparison with independent information would be helpful for the interpretation of this figure.
> Interpretation is generally difficult since we analyze dispersive surface waves and would need to infer dispersion curves (ideally of many event with good S/N) in order to invert for local phase velocity profiles. This merely intends to provide a rough estimate on local, average phase velocities using the more reliable peak amplitues, which seem to be in an acceptable range.

L242 – Fig. 11 states a time window of 10 sec instead of the here mentioned 2 sec.
> This resulted from different frequency bands (hence different time windows). In the revised plot we compare same frequency bands and windows.

L251 – Please, be consistent in your writing: Either give velocities in units of m/s or km/s.
> changed all to m/s

Empirical scaling relation

L263 – The Mw 4.1 event is already included within the 118 events?
> yes, this is already included. M62 not, since it is out of the geographical boundaries of Fig1c

L271+Fig 13 – Well, to be really precise, you would need to grey-shade the area larger than 150 km as well. But this is just a side note. The distribution of coloured/grey dots is showing the same information.
> indeed, but as you note the grey dots are providing this information as well.

L273 – Dot at the end of sentence missing.
> added period

L272-278 – I appreciate this study, the resulting magnitude relation and the very helpful figure 13. However, I am wondering how the rotational magnitudes compare to the translational magnitudes of the analysed events. Because, you have already determined the rotational magnitudes for the subset of 118 events, maybe you could add a histogram for comparison.
> I am not entirely sure, what you mean by “histogram for comparison”. We focus on the rotations, since relations for the acceleration exists, and there will be no new information from just the one station at PFO colocated to the rotation sensor. If I misunderstood, please specify further.

Conclusion

L344 – You could cite Hadziioannou et al., 2012 here.
> added reference

Figures

Fig 2 – The colour-coding of events is hard to see on the coloured topography. How about adding a white transparent layer between the topography and the seismicity? Thus, you would still be able to plot some seismicity in gray but the topography colour would be dulled to make coloured seismicity more clearly visible. ; What is the radius of the map in d? ; What is the shallow and strong event at the southern tip of the lake? It seems larger than the Mw 4.1 which was analysed in more detail? ; Where on the map is the analysed Mw 6.2 located?
> The M6.2 is not included due to reasons explained before. The events south of the Salton Sea are indeed stronger than Mw4.1, however, more distant and therefore not as suited for the analysis as the Mw4.1.
The radius in degree is set as 1 for the azimuthal projection using GMT: *Optionally, the horizon, i.e., the number of degrees from the center to the edge (<=180) [default is 180].*
I adjusted the colormap for the depth of the events and changed transparency of colored events with grey events. This was a suggestion by the second reviewer. I think this increases the visibility sufficiently.

Fig 3 – Only shows waveforms of earthquakes until M 3.6 but section 4 works on Mw 4.1 and Mw 6.1. I would like to see these waveforms as well here (or in a separate Fig to sec 4); especially because Fig 5 and 6 show the waveforms of the larger events only with very limited freq range.
> I am not convinced there is a benefit in showing these event waveforms here as well. We decided on showing additional events to provide and impression as well as characteristic values of smaller magnitudes with regard to their appearance on different components and the noise level. The exemplary events are shown with more detail in Figure 5 and 6.

Fig 4 – „all events“ mean 400 or 118?
> This has been checked and specified in the caption. It is 398 (400 - 2 events which produced an nan value and have been rejected.)

Fig 5 – frequencies mentioned in the caption are not consistent with frequencies mentioned on the plot; also check cc values again.
> well spotted! Thanks. The final update of the figures was apparently not adjusted for in the caption. This has been revised.

Fig 6 – Same as Fig5: check consistency of f and cc within plot and caption (and maybe also text).
> this has been revised.

Fig 7 – maybe show this plot for the Mw 6.1 event as well.
> added the plot to the appendix

Fig 8+9 – explain the combinations of acc components in the two top subpanels ; Is there any relation intended between the top 3 subpanels and the bottom 3 subpanels? If yes, make it more clear; maybe work with (a) to (f) here.
> updated the figure to include the common component combinations (was still based on trial runs). Also added subplot labels and adopted caption.

Fig 9 – why is the total time window for analysis so long (unnecessary bias in histogram)? Maybe cut it at e.g. 100sec?
> was arbitrary to show variations in code. Adjusted it to a shorter time window.

Fig 8+9 – Better use p/m 10° as grey bar (would be consistent to Fig 10)
> good idea. I adjusted it to pm10 degrees

Fig 11 – I like the identification of the time windows as error bars. Nice idea! Velocity results would be better comparable when using the same rage for the y-achsis for the same wavetype for both events (it is a property of the ground and should be the same, independent of the event, right?). Also, plotting the dots a little larger would improve the visibilty of the cc value colour.
> I increased the dot size and set a common maximum for the velocity axis. Different directions probably see a different subsurface with the sensitivity kernel (for lower frequencies at least).

Tables

Tab 2 – „eathquake events“ is a term doublet, „earthquakes“ is enough
> adopted proposed change

Tab 2 – Baz according catalog location or ADR?!?!
> it is the theoretical backazimuth. The waveforms (noise) does not allow for proper estimation. I specified that in the caption.

Tab 3 – I would like to see a figure of the parameter fitting as well.
> We are not entirely sure which parameter fit figure is expected. Here we plot the differences of predicted and observed max. amplitude for the events.