Reviewer A Comments

This manuscript presents a novel technique to use a back-projection-like method to image where slip has occurred during a mainshock (or slow slip event), based on the assumption that triggered aftershocks occur in the positive stress lobes of the static Coulomb stress change. Validation on synthetic datasets and real earthquakes with well-constrained slip models (Parkfield and Antelope Valley) shows that the new method can accurately capture the large-scale features of the fault slip. While the slip distributions do not have the level of detail available in some finite-slip models, the ability to rapidly determine a fault slip model from aftershocks makes this method appealing for use in real-time applications. Because this is a new method, I think it could benefit from additional testing, particularly to demonstrate the robustness of the results with respect to differences in the modeling choices (source/receiver orientation, near-source mask, coefficient of friction).

(1) Please cite the earlier work by Seeber & Armbruster (2000) that inverted aftershock locations for slip distribution assuming Coulomb stress change triggering.

(2) I recommend demonstrating the robustness of this method with respect to the modeling choices (mainly the source double-couple and the receiver fault orientation and slip direction, and also the effective coefficient of friction in Equation 1). Please show some results for the synthetic example and the well-constrained real earthquakes using different assumptions about the source and receiver fault orientations and slip directions (e.g. varying the source within the formal uncertainty of the moment tensor, varying the receivers by some reasonable uncertainty/variability in local fault orientation) and different values of coefficient of friction.

(3) The full information about the density of aftershocks is not being used because the earthquake count for a bin is replaced with a 0 or 1. The earthquake count would generally decay with distance from the source, which should provide additional information about the source location. Please show some results using the full earthquake count for comparison.

(4) The smoothing of the mask close to the source needs more justification. The mask itself, where the DCFS kernel is set to 0 to remove those points from the calculations, makes sense. However, the smoothed region around it doesn't perform this function of removing points. Instead, this smoothing reduces the amplitude of DCFS within some distance of the source. It's unclear why this reduction of the amplitude is desirable. Additionally, the distance scale of the smoothing appears arbitrary, and it isn't clear how sensitive the results are to this choice. I recommend dropping this smoothing. If it's retained, please show some results where the smoothing isn't performed and where its length-scale has been changed, so the reader can see the effects of the smoothing and the sensitivity to the chosen length-scale.

(5) Some published work (e.g. DeVries et al., 2018) suggests that metrics such as maximum shear stress change are better correlated with aftershock location than Coulomb stress change. I recommend testing whether a Coulomb stress change kernel or a maximum shear stress change kernel does a better job of capturing the slip distribution.

(6) Please explain why the results would be clipped to the convex hull of the seismicity. The need for this suggests that high correlations can appear outside the area where slip would be reasonably expected, which suggests some problem with the imaging.

Figure 1 & 2 & 3. Please rotate the longitude labels so that they are not upside-down.

Figure 1. Please explain the two larger black dots – the one connected to the arrow appears to be the source location for this example, what is the other dot?

Figure 3. Please explain the black box in the Parkfield profiles that's said to be silent/less active before and after Parkfield, yet seems to contain earthquakes in every time slice.

References:

DeVries, P. M., Viégas, F., Wattenberg, M., & Meade, B. J. (2018). Deep learning of aftershock patterns following large earthquakes. Nature, 560(7720), 632-634.

Seeber, L., & Armbruster, J. G. (2000). Earthquakes as beacons of stress change. Nature, 407(6800), 69-72.

Reviewer C Comments

This manuscript presents a new method (the seismicity-stress mapping) to build a static slip model for major earthquakes using their aftershock distribution and point-source Coulomb failure stress change (ΔCFS) kernels. The method does not require priori information about the fault geometry, surface traces and deformation, and mainshock seismic recordings to build the slip model. Instead, the method maps source-fault slip in 3D by correlation of point-source Coulomb failure stress change (ΔCFS) kernels across the distribution of seismicity around the source. The author utilized a synthetic test and two well-studied magnitude 6 earthquakes to validate the method. The author then applied it to the 2018 Mw 7.1 Anchorage earthquake, which suffered some debates on the hosting fault of the mainshock rupture. The advantages and potential applications of the method were discussed. Overall, I found the manuscript well written and the figures appropriate. The method is built up convincingly. I have several comments and hopefully they could help the author to improve the manuscript before publication:

1. Though the seismicity-stress mapping does not require priori information, the model built by this method is not a real slip model. Some important parameters for an earthquake, such as the absolute slip amplitudes, seismic moment, and stress drop are lacking. Also, this is only a static model, meaning that the kinematic rupture history and moment rate functions could not be obtained. So in my view, the model built by the method is a "quick but preliminary" one. The quick availability might be an advantage, but it would be better if the author could make an estimation on how quickly the model would be available after an earthquake. To my knowledge, in traditional finite fault modeling approaches, the seismic recordings could also be quickly available. Maybe the GPS and InSAR data need some time to be ready. A comparison on the data available speed and computing time should be added in the discussion.

2. The presentation of comparisons with the previous finite fault models could be improved. For example, in Figures 3 and 5 or other supplementary figures, the author could plot the rupture model of previous studies by contour lines, which could show the detailed slip distribution better than a simple box.

3. I am curious about the influence of rake angles. In real earthquakes, the rake angles usually change and on some fault patches the slip direction deviates from the moment tensor solution. I suggest the author do some tests to show the influence of rake angles of the receiver fault.

4. Lines 163-186: If I understand it correctly, the method assumes that the aftershocks are only induced by ΔCFS, and the relationship between slip and ΔCFS is Okada (1992). However, in natural earthquakes, other factors like dynamical stress, fluid pressurization, and non-elastic processes would also induce aftershocks and affect the results. The influence from these factors should be discussed.

5. Earthquake magnitude. All examples shown in the manuscript are moderate magnitude (M 6-7) events. The method's performance on larger earthquakes (M>=7.5) and more complicated fault systems should be tested.

Minor comments:

1. There is a typo in line 863.
2. Line 242: Please plot a figure to better show the effect of masking. For example, plot a figure showing the curve of Equation 3.
3. For three earthquakes (and possibly all other events), how should the aftershock time window be chosen? Any quantitative criterion?

Dear Ake and Reviewers,

Thank you very much for your time and comments regarding my submission.
My replies to your comments, suggestions and corrections are in this blue text style.
I have also made additional grammar and language corrections, and added some text and references for clarification and completeness, mainly in the context of changes made in response to your comments.

Please also see my "Important note" just below.

Best regards,
Anthony

Important note on finite-fault inversions:

Unfortunately comparing to previous finite-fault models is highly problematic, as finite-faulting maps for a single earthquake produced by different studies are often clearly different (as already mentioned in the introduction), and detailed slip distributions often show opposite results for location of high versus low-slip patches (e.g. compare slip maps for events with multiple slip models at http://equake-rc.info/srcmod). A just-out paper (Wong *et al.* 2024) states "finite-fault inversions are subject to non-uniqueness and uncertainties. The diverse range of published models for the well-recorded 2011 Mw 9.0 Tohoku-Oki earthquake illustrates this challenge, and its rupture process remains under debate". In this paper, the median of 32 models (their Fig. 8b) does not look to provide much information beyond that of a centroid moment-tensor location that is spatially extended with a circular rupture radius.

Thus there is usually no clear benchmark from previous finite-fault fault models with which to compare, beyond, in some cases, one or two patches of main slip that are quasi-stable between models (e.g. see the new analysis in the Supplement of the 2023 Mw 7.8 and Mw 7.5 Kahramanmaraş, Türkiye events). So for Parkfield, I stated "The depth and along-fault ranges of this high potential slip region corresponds well to independent estimates of the geometry of co-seismic slip and energy radiation, especially given the diversity and uncertainty of these estimates", followed by description of principal similarities and differences between the seismicity-stress results and previous finite-fault and high-frequency radiation studies.

[Seismica] Editor Decision
2024-09-10 02:18 AM

Dear Anthony Lomax:

I hope this email finds you well. I have reached a decision regarding your submission to Seismica, "Mapping finite-fault earthquake slip using spatial correlation between seismicity and point-source Coulomb failure stress change". Thank you once again for submitting your work to Seismica.

Based on reviews I have received, your manuscript may be suitable for publication after some revisions. Both the reviewers are positive and constructive, and their requests are pretty self-explanatory and should improve the potential impact of the paper. In my own reading I was also concerned with points like the sensitivity to receiver fault orientation and rake (typically an issue with CFS applications, and it seems simplified to take one value for the aftershocks, but does it matter much?) , I agree that aftershock mechanisms should be taken into account, and this is noted for the Antelope Valley example "future seismicity-stress analysis of this event should examine the case of using a point-source ΔCFS kernel with strike-slip receiver faults, and also application separately to normal and strike-slip faulting aftershock sets." But I think such extension and study is beyond the scope of this introductory paper (and the current imaging does work well), and I also suspect that improvements may involve a more creative and general approach than just considering additional receiver fault orientations. So I have only added selected tests with varying the source and receiver fault orientations, effective coefficient of friction, and using only the shear and normal components of ΔCFS.

stress distribution pre-earthquake (you do mention this, and it is difficult to account for) and any effects of fluids (particularly if applied to slow events).

These issues also seems beyond the scope of an introductory paper, and also, like many other aspects of Coulomb analysis, seems quite controversial in the literature. Not to mention the difficulty in identifying, parametrizing and incorporating these phenomena in any procedure and testing.

Overall though, this is an interesting new method with scope for exciting applications,

Thanks – I hope so!

and I hope you find the review comments useful for improving the description and justification of the methodology, demonstrating robustness, and clarifying its potential application and associated limitations.

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.

Once I have read your revised manuscript and rebuttal, I will then decide whether the manuscript either needs to be sent to reviewers again, requires further minor changes, or can be accepted.

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

Please note that Seismica does not have any strict deadlines for submitting revisions, but naturally, it is likely to be in your best interest to submit these fairly promptly, and please let me know of any expected delays.

I wish you the best with working on the revisions. Please don't hesitate to contact me with any questions or comments about your submission, or if you have any feedback about your experience with Seismica.

Kind regards,

*Ake Fagereng
Cardiff University
ake.fagereng@seismica.org*

Reviewer A:

This manuscript presents a novel technique to use a back-projection-like method to image where slip has occurred during a mainshock (or slow slip event), based on the assumption that triggered aftershocks occur in the positive stress lobes of the static Coulomb stress change. Validation on synthetic datasets and real earthquakes with well-constrained slip models (Parkfield and Antelope Valley) shows that the new method can accurately capture the large-scale features of the fault slip. While the slip distributions do not have the level of detail available in some finite-slip models, the ability to rapidly determine a fault slip model from aftershocks makes this method appealing for use in real-time applications. Because this is a new method, I think it could benefit from additional testing, particularly to demonstrate the robustness of the results with respect to differences in the modeling choices (source/receiver orientation, near-source mask, coefficient of friction).

(1) Please cite the earlier work by Seeber & Armbruster (2000) that inverted aftershock locations for slip distribution assuming Coulomb stress change triggering.

Thanks very much for this reference – I did not come across it despite extensive searching over many months! This reference also lead me to two others which also uses aftershock seismicity and ΔCFS to constrain mainshock slip. I have added a paragraph in the introduction citing and summarizing these papers, but noting that they use somewhat ad-hoc, trial-and-error procedures, and all require specification of 2D surfaces for mainshock faulting.

(2) I recommend demonstrating the robustness of this method with respect to the modeling choices (mainly the source double-couple and the receiver fault orientation and slip direction, and also the effective coefficient of friction in Equation 1). Please show some results for the synthetic example and the well-constrained real earthquakes using different assumptions about the source and receiver fault orientations and slip directions (e.g. varying the source within the formal uncertainty of the moment tensor, varying the receivers by some reasonable uncertainty/variability in local fault orientation) and different values of coefficient of friction.

This would be 3 source and 3 receiver faulting parameters plus friction parameter for 4 event cases. So if only 4 perturbed values for each parameter were examined there would be about 100 configurations to investigate, plot and discuss. In addition, it is not immediately clear what the character of the results would be and thus what selection, statistical analyses and visualization to apply. All this work seems excessive for an introductory journal article (that is unfunded!), but might be of interest for a well supported thesis or post-doc if the method proves to be useful.

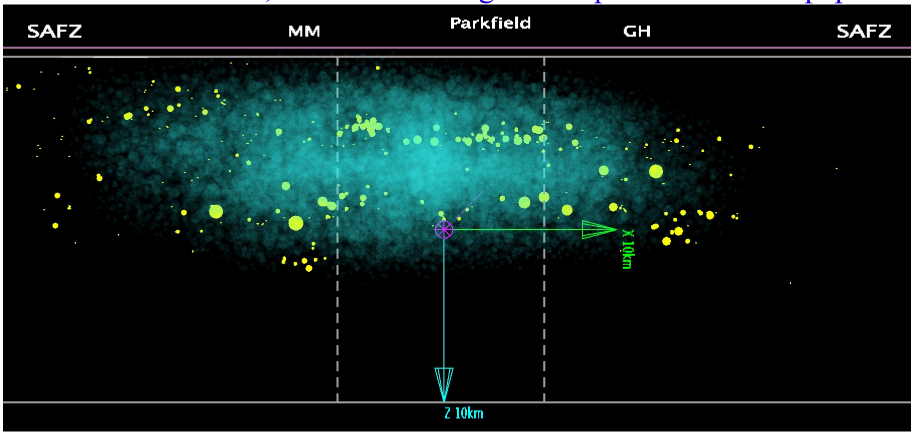
Instead I have added to the Supplement plots showing perturbation of strike, dip and rake for source and receiver faults together, and effective coefficient of friction for the Parkfield case, and added relevant text in the new sub-section “Sensitivity of seismicity-stress imaging to faulting parameters and stress components” in the Discussion. For the Parkfield event the relative simplicity of the faulting and aftershock distribution aids in understanding the effect of varying the mechanism.

(3) The full information about the density of aftershocks is not being used because the earthquake count for a bin is replaced with a 0 or 1. The earthquake count would generally decay with distance from the source, which should provide additional information about the source location. Please show some results using the full earthquake count for comparison.

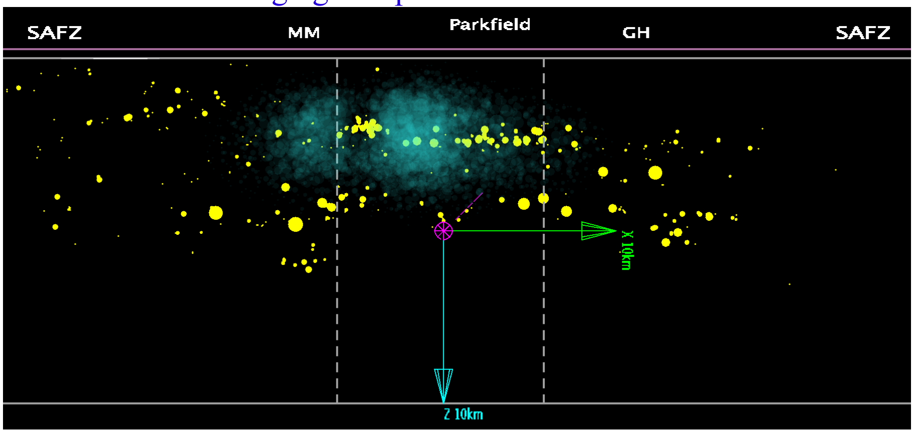
I investigated and tested quite extensively this issue. While the aftershock count should generally decay with distance to the source, there are other factors controlling aftershock density that can easily dominate this decay out to considerable distance. These factors may include: 1) 3D variations in seismogenic potential (e.g. rock properties (brittleness), existing fault and fracture density, previous stress state, distribution of previous slip and slip boundaries, etc); 2) nearby areas of high background seismicity and swarm activity; 3) triggered nearby larger aftershocks and their aftershocks, 4) aftershocks of aftershocks in general, … (I extended the text summarizing this issue in the section "Correlation of ΔCFS kernels over post-event seismicity".)

So between these considerations and testing, I settled on the 0/1 aftershock count convention.

Here is the seismicity-stress slip imaging for Parkfield for 1 month of aftershocks with 0/1 aftershock bin count, otherwise configured as presented in the paper:



Here is the same imaging except with unlimited aftershock bin count:



The resolved slip potential is highly concentrated next to the areas of highest aftershock density. This slip potential does not match previous results which generally place the 2004 M6 rupture over an ~20km length to the NW of the hypocenter and between the two main aftershock streaks, as is the case the seismicity-stress slip imaging with 0/1 aftershock bin count.

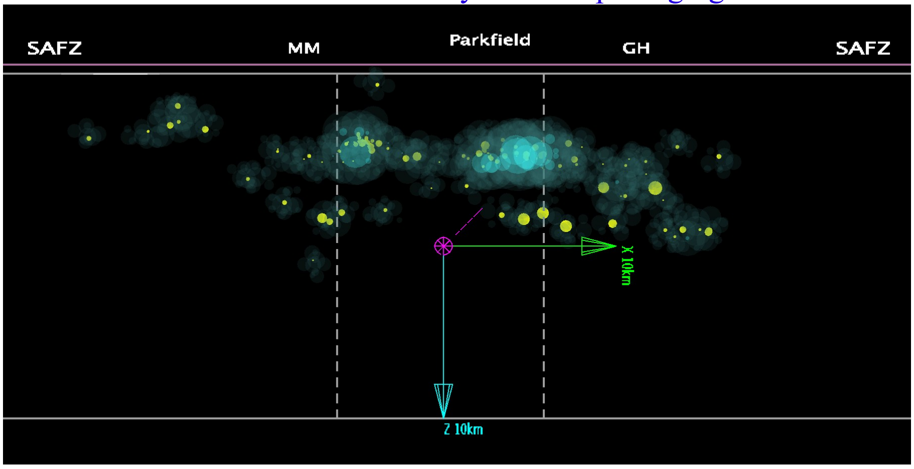
(4) The smoothing of the mask close to the source needs more justification. The mask itself, where the DCFS kernel is set to 0 to remove those points from the calculations, makes sense. However, the smoothed region around it doesn't perform this function of removing points. Instead, this smoothing reduces the amplitude of DCFS within some distance of the source. It's unclear why this reduction of the amplitude is desirable.

This was addressed section "The seismicity-stress imaging procedure", I have extended this text.

Additionally, the distance scale of the smoothing appears arbitrary, and it isn't clear how sensitive the results are to this choice. I recommend dropping this smoothing. If it's retained, please show some results where the smoothing isn't performed and where its length-scale has been changed, so the reader can see the effects of the smoothing and the sensitivity to the chosen length-scale.

These issues are addressed in the new Supplementary Figure S1 and further discussed in additions in the main text in section "The seismicity-stress imaging procedure".

Without the smoothing, which targets the seismicity-stress imaging to the length scale of source rupture and typical distance between rupture and post-event seismicity, the procedure is highly biased to imaging slip only in the halo region around each aftershock and just recovers a blurry map of aftershocks. Here is the seismicity-stress slip imaging for Parkfield with L_m smoothing not used:



(5) Some published work (e.g. DeVries et al., 2018) suggests that metrics such as maximum shear stress change are better correlated with aftershock location than Coulomb stress change. I recommend testing whether a Coulomb stress change kernel or a maximum shear stress change kernel does a better job of capturing the slip distribution.

Maximum shear stress change is a quantity that is always positive, so it is difficult to asses its performance in correlating with aftershocks. And an always positive quantity may not give usable results for imaging methods such as seismicity-stress where the spatial resolution of the method depends on cancellation between positive and negative spatial measures. Moreover, it may be that 1) a well constrained rupture geometry and 2) excluding nearby aftershocks are primary requirements for the successful explanation of aftershocks with ΔCFS (Steacy *et al.* 2004) – the seismicity-stress procedure addresses both of these issues (it finds an optimal rupture geometry, and the masking can be used to effectively exclude nearby aftershocks).

The results of (Meade *et al.* 2017) show that Coulomb failure criteria total shear and some other Coulomb failure criteria components are not much different from maximum shear stress change. Given these results and as I cannot calculate maximum shear stress with my current algorithms and underlying code, I have added to the Supplement plots showing the slip imaging obtained from only the shear and only normal stress change components of ΔCFS for the Parkfield and Antelope Valley cases, and added relevant text in the new sub-section "Sensitivity of seismicity-stress imaging to faulting parameters and stress components" in the Discussion. For these events the shear stress contribution greatly dominates over normal stress to form the slip map obtained with the full Coulomb stress.

(6) Please explain why the results would be clipped to the convex hull of the seismicity. The need for this suggests that high correlations can appear outside the area where slip would be reasonably expected, which suggests some problem with the imaging.

As with any finite inversion or imaging procedure, there are limits to the available data distribution and model extent; outside these limits the method may produce noise or artefacts – it has no or low resolution. As the ΔCFS kernel has strong decay with distance, high-correlations do not seem to occur with the seismicity-stress imaging beyond roughly the decay-length smoothing distance. Thus the clipping using the seismicity convex hull is mainly for clear visualization of the limits of the model resolution. I have added in the sub-section "The seismicity-stress imaging procedure":

"This clipping is useful for visualization and interpretation since, in general, potential slip in volumes not surrounded by seismicity will not be well constrained by the procedure. Similar masking is used in many inversion and imaging methods, for example, in seismic tomography for areas with little or no ray coverage. There may be cases where seismicity-stress clipping is undesirable, for example in attempting to image strain due to tremor or ductile slip below an active seismogenic zone."

Figure 1 & 2 & 3. Please rotate the longitude labels so that they are not upside-down.

The labels are plotted in a standard, bottom-to-the-east geographic orientation. However, the plot is rotated 49° counter-clockwise and this also rotates the labels. These label rotations seem to me to be informative to the viewer, emphasizing the plot rotation. In any case, the plot is a snapshot from 3D interactive viewing software that does not currently support changing the geographic orientation of the latitude and longitude labels.

Figure 1. Please explain the two larger black dots – the one connected to the arrow appears to be the source location for this example, what is the other dot?

I have modified the image and explained the two dots in the caption.

Figure 3. Please explain the black box in the Parkfield profiles that's said to be silent/less active before and after Parkfield, yet seems to contain earthquakes in every time slice.

To my eye, the seismicity in the box is higher 20-10 years before the 2004 mainshock (Fig S1, 1984-1994) than 10-20 years after the mainshock (Fig S1, 2014-2023). But this pattern would best be examined with regards to magnitude of completeness and other statistics, and is a side issue for the current paper, so I have removed the box from the fugues and text mentioning this possible change of activity.

References:

DeVries, P. M., Viégas, F., Wattenberg, M., & Meade, B. J. (2018). Deep learning of aftershock patterns following large earthquakes. Nature, 560(7720), 632-634.

Seeber, L., & Armbruster, J. G. (2000). Earthquakes as beacons of stress change. Nature, 407(6800), 69-72.

Recommendation: Revisions Required

Reviewer C:

This manuscript presents a new method (the seismicity-stress mapping) to build a static slip model for major earthquakes using their aftershock distribution and point-source Coulomb failure stress change (ΔCFS) kernels. The method does not require priori information about the fault geometry, surface traces and deformation, and mainshock seismic recordings to build the slip model. Instead, the method maps source-fault slip in 3D by correlation of point-source Coulomb failure stress change (ΔCFS) kernels across the distribution of seismicity around the source. The author utilized a synthetic test and two well-studied magnitude 6 earthquakes to validate the method. The author then applied it to the 2018 Mw 7.1 Anchorage earthquake, which suffered some debates on the hosting fault of the mainshock rupture. The advantages and potential applications of the method were discussed. Overall, I found the manuscript well written and the figures appropriate. The method is built up convincingly. I have several comments and hopefully they could help the author to improve the manuscript before publication:

1. Though the seismicity-stress mapping does not require priori information, the model built by this method is not a real slip model. Some important parameters for an earthquake, such as the absolute slip amplitudes, seismic moment, and stress drop are lacking.

Seismic moment estimates for large earthquakes are now available within minutes after the event occurs (Lomax & Michelini 2013; Goldberg *et al.* 2023) and, given a rupture area, moment can be used to determine mean values for slip amplitude and an approximate stress drop (Aki 1972). Thus slip and stress drop estimates can be obtained from seismicity-stress imaging since it provides information on rupture area, e.g. from the limit of high-potential faulting patches. Furthermore, the spatial variation of relative seismicity-stress slip may be effective for determining heterogeneous slip maps and, in consequence, heterogeneous stress drop (Madariaga 1979).

I have added the above paragraph in the Discussion, following an existing sentence that already alluded to quantification of slip and stress drop.

Also, this is only a static model, meaning that the kinematic rupture history and moment rate functions could not be obtained.

Yes, correct. That the method produces “potential, relative, static finite-fault slip”, as is mentioned throughout the manuscript.

So in my view, the model built by the method is a “quick but preliminary” one.

Yes, if judged only by the measures nominally produced by standard, kinematic finite-fault approaches (and often very non-uniquely produced, see “Important note on finite-fault inversions” at the top of this reply).

However, there are many additional advantages, measures, results and uses provided by seismicity-stress imaging, which I tried to emphasize and describe in the manuscript, e.g.

- The seismicity-stress procedure maps potential slip in 3D space without need for defining a priori the position and geometry of 2D faulting surfaces.
- The procedure does it require oa hypocentral location or other initiation point for target slip, or specification of parameters like rupture speed and rise-time.

- The procedure uses different data than other methods (e.g. aftershocks instead of seismic waveforms or geodetic displacements) and so can provide independent information to resolve scientific questions. This use of the procedure is illustrated through identification of the likely mainshock faulting plane in the Anchorage application.
- The procedure can be used to map migration of areas of slip, after-slip and creep over longer time periods.
- The procedure may be applicable to map stress sources like volcanic dyke intrusion that do not involve a mainshock or other abrupt, dislocation, as long as the source causes surrounding seismicity.

The quick availability might be an advantage, but it would be better if the author could make an estimation on how quickly the model would be available after an earthquake. To my knowledge, in traditional finite fault modeling approaches, the seismic recordings could also be quickly available. Maybe the GPS and InSAR data need some time to be ready. A comparison on the data available speed and computing time should be added in the discussion.

The computing time is small to negligible, as already discussed in the section "The seismicity-stress imaging procedure". And this is with rapidly assembled, research code, not optimized, production code.

For rapid seismicity-stress analysis after a large earthquake, the main delay is waiting for the occurrence of and location of aftershock seismicity. The Parkfield application in this study uses 4 hours of aftershock seismicity, at which point a moment tensor would have been long available, so the seismicity-stress results could also be obtained a few hours after the mainshock. In the near future, high-performance, automated processing using machine-learning seismic phase picking, which can detect several times more events than current automatic picking algorithms (Liu *et al.* 2020; Cianetti *et al.* 2021), should allow effective application of the seismicity-stress procedure in reduced time after a mainshock.

I have added a paragraph similar the above in the Discussion.

2. The presentation of comparisons with the previous finite fault models could be improved. For example, in Figures 3 and 5 or other supplementary figures, the author could plot the rupture model of previous studies by contour lines, which could show the detailed slip distribution better than a simple box.

First, please recall my assessment in "Important note on finite-fault inversions" at the top of this reply. Due to this assessment I do not think it meaningful to make detailed, geometrical or quantitative comparisons with details of previous rupture models.

The simple, boxy representation in Fig. 5 of the (Wang *et al.* 2023) slip is already a near-exact representation (after shifting to compensate for differences in hypocenter location) of a "contour" of their rectangular gridded, source fault parametrization. Any smooth contouring of their results might be aesthetically clearer, but would mis-represent their results.

I have removed the perhaps misleading box in Fig. 3 and related figures which highlighted particular aftershocks and was not a simple representation of a rupture model.

3. I am curious about the influence of rake angles. In real earthquakes, the rake angles usually change and on some fault patches the slip direction deviates from the moment tensor solution. I suggest the author do some tests to show the influence of rake angles of the receiver fault.

I have added to the Supplement plots showing perturbation of strike, dip and rake for source and receiver faults for the Parkfield case. For this event the relative simplicity of the faulting and aftershock distribution aids in understanding the effects of varying the mechanism.

4. Lines 163-186: If I understand it correctly, the method assumes that the aftershocks are only induced by ΔCFS, and the relationship between slip and ΔCFS is Okada (1992). However, in natural earthquakes, other factors like dynamical stress, fluid pressurization, and non-elastic processes would also induce aftershocks and affect the results. The influence from these factors should be discussed.

There is very much literature, discussion and controversy on these topics, and, as with most studies involving ΔCFS based analysis, it is not feasible or practical to include them, let alone investigate and discuss them. I have added a sentence in the Discussion section "Sensitivity of seismicity-stress imaging to faulting parameters and stress components" noting these issues in the context of the seismicity-stress procedure and emphasizing that it only addresses quasi-static stress change due to dislocation or crack opening in an elastic medium.

5. Earthquake magnitude. All examples shown in the manuscript are moderate magnitude (M 6-7) events. The method's performance on larger earthquakes (M>=7.5) and more complicated fault systems should be tested.

I had applied and extensively tested application of the method to a number of larger sequences. The results are very promising, but differences in application, interpretation and potential impact of such results necessitates that the large earthquake case be treated in a separate paper. However, I take advantage of you comment to present in the supplementary material a simplified, preliminary investigation of such application for the 2023 Mw 7.8 and Mw 7.5 Kahramanmaraş, Türkiye, earthquake sequence, and I introduce and summarize these results in a paragraph in the Discussion in the main text.

Minor comments:

1. There is a typo in line 863.

Corrected.

2. Line 242: Please plot a figure to better show the effect of masking. For example, plot a figure showing the curve of Equation 3.

These is done in the new Supplementary Figure S1 and further discussed in additions in the main text in section "The seismicity-stress imaging procedure".

3. For three earthquakes (and possibly all other events), how should the aftershock time window be chosen? Any quantitative criterion?

The aftershock time window after the main event should be as short as possible, otherwise the seismicity-stress slip imaging may capture after-slip. However, the imaging will in general increase in accuracy as the number of aftershocks increases. A higher rate of aftershock detection with machine learning should aid in shortening the time window.

I have added text on this topic in the Discussion.

Recommendation: Revisions Required

References for author’s replies

Aki, K., 1972. Earthquake mechanism. *Tectonophysics*, **13**, 423–446. doi:10.1016/0040-1951(72)90032-7

Cianetti, S., Bruni, R., Gaviano, S., Keir, D., Piccinini, D., Saccorotti, G. & Giunchi, C., 2021. Comparison of Deep Learning Techniques for the Investigation of a Seismic Sequence: An Application to the 2019, Mw 4.5 Mugello (Italy) Earthquake. *Journal of Geophysical Research: Solid Earth*, **126**, e2021JB023405, John Wiley & Sons, Ltd. doi:10.1029/2021JB023405

Goldberg, D.E., Taymaz, T., Reitman, N.G., Hatem, A.E., Yolsal-Çevikbilen, S., Barnhart, W.D., Irmak, T.S., *et al.*, 2023. Rapid Characterization of the February 2023 Kahramanmaraş, Türkiye, Earthquake Sequence. *The Seismic Record*, **3**, 156–167. doi:10.1785/0320230009

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Lomax, A. & Michelini, A., 2013. Tsunami Early Warning Within Five Minutes. *Pure Appl. Geophys.*, **170**, 1385–1395. doi:10.1007/s00024-012-0512-6

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