Dear Editor,

I am grateful to you and the reviewers for looking at my manuscript. The comments have helped to improve the paper significantly. Below, I give my individual responses to each comment from the reviewers. Here I will just briefly summarize the main changes.

There were numerous questions raised about uncertainties in the measurements, and so to respond to these valid concerns, I implemented a bootstrap procedure that provides simplified visual and quantitative uncertainty estimates of the dip. I've added these results to each figure where the dip estimates are made.

I added an extensive section in the methods section describing the parameter selection process and effective resolution of the technique. I added extra figures to help communicate the resolution as a function of the parameters.

Numerous other textual and figure changes were made to improve clarity and provide additional information when appropriate.

I hope that you will find this revision suitable for publication.

Sincerely,

Zachary Ross

Reviewer A:

The study aims to resolve fault geometry from seismicity using a new statistical method. The method is based on theories in anisotropic point processes and can identify geometric features in seismicity. The author tested the method using synthetic data and then applied the approach to 11 seismically active regions in southern California. The results agree with existing models. The study is interesting, and the method appears to have potential for being broadly used to study other fault systems. I enjoyed reading the manuscript. Here are a few comments to help improve the clarity of the study:

Thank you for your feedback and review of this paper.

1. The current manuscript didn't address the uncertainties in the estimated fault geometry. From the stereographic figures, the contours seem to suggest a range of values for fault dips. A clear quantification and discussion of the uncertainties would be appreciated.

Indeed, this is a good question. I have added a new section to the methodology called "Dip Uncertainty Estimates", which adds a bootstrap procedure to the workflow and results to quantify the uncertainty in the dip. I have added bootstrap histograms for each region to

Figures 4, 5, 6, 7. In summary: the conclusions are unchanged from the previous submission, but these new figures are quite helpful to communicate this properly.

2. The selection of parameters is not explained. There are multiple sets of different parameters used in the study to test the synthetics and examine the seismicity in southern California. The values span a broad range, and it is unclear why these values are used. Justifications are needed, and differences in the values require explanations.

I have added three paragraphs under a new section in Methods called "Parameter Selection and Resolution". This section lays out in detail some guidance on selecting the parameters and how to interpret the results from them. I am hopeful this will address your concerns adequately.

3. The synthetic tests are very idealized. To investigate the method's effectiveness, I would encourage the author to include a few more realistic examples, e.g., different fault strands with varying orientations and seismicity with limited spatial extents.

I have added a new synthetic test consisting of conjugate dipping faults with slightly randomized angles. This test shows the ability of the method to resolve multiple persistent fault orientations when present.

4. Some of the values are confusing. For example, the seismogenic thickness is listed as 10km in Line 119, while the figure seems to suggest it is 50 km.

These values have all been corrected. My apologies for this confusion. The maximum depth for all of the tests is given as 20 km now, and the figures have been updated.

5. The listric geometry at the SJFZ Trifurcation Area is interesting, and perhaps, the author can consider including some seismicity depth profiles to compare with the obtained values.

There are excellent cross sections of the seismicity of this area already in several papers. I have added text pointing the readers to them directly

"For cross sections of the seismicity in this area, the reader is recommended to see Figure 7 of Schulte-Pelkum et al. (2020) or Figure 2 of Ross et al. (2017a)"

Reviewer B:

This paper proposes a new way to characterize the strike and dip of faults from seismicity, by using the statistics of event pairs within a disc-shaped cylinder. This makes sense intuitively that there would be more event pairs within a disc aligned with the orientation of a fault, and this is demonstrated in a few synthetic examples. This new technique is then applied to selected faults in southern California. Because this method is so general and easy to implement, it has the potential to become a widely-used tool in seismicity analysis. I have a few questions about how to interpret some of the results, however, and I think the paper would benefit from a few more synthetic examples that illustrate what the results look like when there are multiple fault orientations present. I also think that it would be useful to discuss how to characterize the uncertainty in the inferred fault orientations.

Thanks for your review of my manuscript.

(1) Are the cylinders actually disc-shaped as described? A disc should have radius (r) > height (t), while the synthetic catalogs are analyzed with r=0.1 km, t=1 km, and the real data uses (r, t) = (50 m, 500 m), (100 m, 1000 m), and (200 m, 2000 m). I hope that these are typos, or that the definitions of r and t on line 103 are backwards.

These values were unfortunately a set of typos that resulted from a misused find/replace. You are correct, r > t for this study and has been corrected throughout the paper. Thanks so much for pointing this out.

(2) The interpretations assume that the cylinders characterize fault orientation on the length-scale of the size of the cylinders. This isn't demonstrated, and the examples in Figure 2 appear to contradict this assumption. The faults in the examples have length and width on the order of 50 km, and they are visible with cylinders with length scales of only 0.1-1 km. I think that as long as the cylinders are larger than the thickness of the fault strands, the fault orientation should be detectable. Some more synthetic examples could make it clearer how the size of the cylinders relates to the dimensions of the faults they are sensitive to, including fault thickness.

I added a new Figure (now called 4) which is a schematic illustrating how the value of *r* relates to the resolution. I also added a new section called "Parameter Selection and Resolution" to the methods with several paragraphs explaining this in detail.

(3) Changes in apparent dip with the size of the cylinders is interpreted as the presence of multiple fault orientations, with different size faults having different orientation. It would be helpful to see analogous synthetic examples of regions with multiple fault populations.

I have added a new Figure (now called 5) with a catalog containing faults 3km and 1km, having dips of 90 and 0 degrees respectively. This shows exactly what you have asked about.

(4) The paper could use more discussion of uncertainty. While the paper reports the orientations with the highest Kcyl value, many of the plots show a large area of high Kcyl (yellow/orange). For example, while the "best" dip of the Southern San Andreas increases with increasing cylinder size, the full range of dips (50°-90°) have high Kcyl (yellow/orange) for all cylinder sizes. It's unclear what is the uncertainty of the "best" dips, and therefore how significant the differences are.

Repeated from above: I have added a new section to the methodology called “Dip Uncertainty Estimates”, which adds a bootstrap procedure to the workflow and results to quantify the uncertainty in the dip. I have added bootstrap histograms for each region to Figures 4, 5, 6, 7. In summary: the conclusions are unchanged from the previous submission, but these new figures are quite helpful to communicate this properly.

(5) Similarly, the large areas of yellow/orange might imply large uncertainty, or they might imply that the study areas encompass a variety of fault orientations. It might be helpful to show some synthetics to try to understand to what extent noise (uncertainty) can be distinguished from a true variety of plane orientations.

In addition to the bootstrap uncertainties added in the revision, I have added a new synthetic catalog to Figure 2 that has multiple fault orientations, and that the method can successfully recover them.

(6) It might also be useful to consider a larger range of choices for r and t, and different ratios r/t, to give some guidance about how to best select these parameters.

Copied from my response to reviewer 1: I have added three paragraphs under a new section in Methods called “Parameter Selection and Resolution”. This section lays out in detail some guidance on selecting the parameters and how to interpret the results from them. I am hopeful this will address your concerns adequately.

(7) Please explain more about the edge correction factor, eij, and/or give an equation.

I added the equation (now eq 6) for this.

Figures:

The stereonet plots of Kcyl need a legend for the color scale. It appears that they are all scaled so that the maximum value always has the same color. So it would work to show one legend (% of max value) and then report the maximum value on each plot.

I have remade these figures so that they are now all on the same scale and added a color bar.

Some of the stereonet plots are cut off for dips less than 50°, or perhaps the projection is changed to compact the region for dips <60°. Please explain what is going on with these projections.

I have extended the dip angular range to [0, 90] degrees.

In Figure 2, the seismogenic thickness appears to be 30-50 km, not the 10 km given in the text.

I've fixed these issues in the text and figures. I've also modified the depth now to be a maximum of 20 km.