Response to Referee #3

We thank the reviewer for their concise and constructive comments on our manuscript. We address the issues raised in sequence in the text below, complete with any explicit changes we have made to our manuscript.

This is a short, straightforward paper addressing an important issue in structural geology. The statistical tests derived here should be of wide interest and applicability.

My maths is too rusty to follow all the details of the statistical treatment. What I can follow looks right and the results are qualitatively sensible.

We thank the reviewer for his belief that this work should be of wide interest and applicability.

My only comments on the paper concern nomenclature of the patterns of poles to faults. I realise that the nomenclature is the published one of Healy et al. (2016) but the comments still need consideration.

The issues arise in lines 43 to 72 and in Figure 1.

a) The term 'bilateral' is used without definition in line 72, presumably to mean a pattern that is bilaterally symmetrical about one or more of the principal planes of the distribution. The issue is that all the illustrated ideal patterns are bilaterally symmetric about all the three principal planes, so 'bilateral' doesn't seem to discriminate any one pattern.

b) The term 'polymodal' is applied to a pattern that is actually bimodal, with each mode dispersed along a small circle about sigma1. Another term, like 'dispersed bimodal' seems more accurate.

c) The term 'orthorhombic' is being equated to the 'polymodal' pattern only? Actually all the illustrated ideal patterns have orthorhombic symmetry, as they must have if they have three orthogonal mirror planes.

I suppose the central point is that the number of modes in a distribution is unrelated to its symmetry. A distribution needs both descriptors to specify it. So distributions could be bimodal and monoclinic, or polymodal and triclinic, or many other variants. Your statistical method (I think) only applies to orthorhombic distributions and maybe this should be stated up front.

This lack of clarity might cause confusion in the future literature?

In line 72, we now make this more explicit to address points a)-c) above. However, note that using 'polymodal' for the type of distribution shown in Figure 1f is correct if (and this is where our work is relevant) there is no central tendency within each of the clusters i.e. it is literally 'many moded', rather than a noisy bimodal pattern. We have added text to the abstract to highlight the restriction to orthorhombic symmetry.

We hope the changes we have made will serve to reduce the chances for confusion.

Response to Referee #4

We thank the reviewer for their concise and constructive comments on our manuscript. We address the issues raised in sequence in the text below, complete with any explicit changes we have made to our manuscript.

231 μ is often used for the friction coefficient: change it to something else

Our paper is not about mechanics or frictional sliding; we doubt there will be any confusion with using mu for this term. Left as is.

235 is gamma intersection angle? If so, point it out.

Explained on line 236.

249 and 252 S and T are commonly used for the principal stresses.

The S notation for the eigenvalues comes from Woodcock (1977); we doubt if there will be confusion with stresses. Left as is.

279 tests of bimodality and quadrimodality appears to be fine and merit the publication of the manuscript.

Good, thanks!

318-324 Good discussion of the Chimney Rock data set, a detail of which is hard to find in any place in the literature. I don't have anything to say about the data from central Italy attributed to Roberts, 2007. Sorry, I am too lazy to dig it out!

Thanks!

372 The word "probably" is unnecessary.

Yes, deleted.

374-376 The main results are worthy in a statistical sense as I mentioned above. Perhaps that is why the authors find a better fit with the synthetic datasets. Given that most map-scale natural faults evolve or grow by interaction, splaying, coalescence, and in some cases, reactivation under progressive material and stress rotation, naturally they are much more complex (Aydin and Zhong; Rock Fracture Knowledgebase). I don't expect the authors discuss all of these mechanisms in one manuscript, but they may just point out why most natural faults are more complicated.

Added new lines in this section. Thanks.