Solid Earth Discussions

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Comment on se-2021-100

Anonymous Referee #2

Referee comment on "De-risking the energy transition by quantifying the uncertainties in fault stability" by David Healy and Stephen Paul Hicks, Solid Earth Discuss., https://doi.org/10.5194/se-2021-100-RC2, 2021

First of all, I would say that the authors are top scientists in this field and accordingly, the idea and the methodology reported in this paper seem to be very promising. Moreover, for people like me with a prevalent geological background, the pure statistical part of the paper can be hard to be read just because of the background. However, the geological data seems to be, in my opinion, poorly exposed here and the statistics are sometimes completely detached from the geological data making this paper quite difficult to be read from a Solid Earth reader.

Generally speaking, the paper faces a very interesting problem, and the method is innovative and very exciting. As far as I can see the methodology is new and for this it must be tested and verified yet. The authors attempt to do this by presenting two case studies with the aim to show "how combined RSM/MC approach can be used to estimate the probability of slip on one or more faults". However, the two cases are not very well constrained in terms is of boundary conditions making the probability estimation quite confused. Moreover, the two performed analyses (Porthtowan Fault Zone in Cornwall, UK and Coalfields in South Wales and Greater Manchester, UK) differ in so many aspects and, more importantly the presented results are different in terms of delivered outputs. This make the reading quite confusing and at the end of the paper I got lost about the point that the authors would like to address. In my opinion to test a new methodology we should apply this in areas where data are known as much as possible to see if the model prediction are reliable. In this case since the two areas are poorly constrained, this exercise is difficult to be followed and the results even more difficult to be understood.

The discussion paragraphs more than discuss the results present a list of what we should know to better assess the seismic risk and the main message seems to be that we would need to know a lot of things. I can kind of agree with this but, once again, this makes the main message of the paper more confused.

I strongly suggest the authors to simplify the paper in two ways.

- Try to organize a sort of sensitivity analysis of the involved parameters in a more structured and ordered way in order to facilitate the reader
- Focus in one area and compare the results with something actually observed.

I think that we all agree that there are many topics related to the risk assessment (fault length, roughness, friction, fluids, background seismicity, regional strain rate, and many many others) but in doing this exercise authors must clearly state the assumption and critically analyze the results. In this paper I had the impression that speaking about the many variables we lose the point of the paper, I would say that sometimes less is more.

Minor points:

I am not so convinced about the statistical discussion that is sometimes too focused on the pure statistics and few on the geology behind. For example, can we find a geological meaning to the "asymmetrical or skewed" distribution of some parameters?

I Am not expert on Response Surface Methodology (RSM). However, the paragraph Statistical analysis of geomechanical fault stability start with a discussion on the governing equations for RSM following a quite long description that ends with the definition of Ts by meaning of the very well-known direction cosines (e.g. Ramsay and Lisle 2000). In other word I can't really see why the authors need introducing the RSM theory to infer the Ts definition.

A lot of acronymous BGS, CDF, are used but not defined. Even if they are quite easily understandable, this gives the impression of a lazy writing

The discussion on the relationship between fault length and events magnitude starts with this and ends with discussing the relationship between fault length and number of events. I would say that the two (maximum magnitude and number of events) are surely correlated but they are not the same thing.

Line to line comments:

Line 228 I would say that fluid pressure also influences Ts (e.g. De Paola et al., 2007)

Line 239 is CDF the cumulative distribution function? Authors should state this somewhere.

Line 326 alfa has been not defined

Line 698 Why these may be the ones most likely to slip?

Line 700 Some of this "mismatch" could be explained by the dip of the faults measured at the surface, but not all. What the author mean here?

Line 742 The observational record shows that bigger fault zones. I would say that there are a lot of physical reasons behind this. Moreover, empirical relationships such as those suggested by Wells and Coppersmith 1994, or Leonard 2010 should be cited here.

Suggested references:

De Paola, N., Collettini, C., Trippetta, F., Barchi, M.R., Minelli, G., 2007. A mechanical model for complex fault patterns induced by evaporite dehydration and cyclic changes in fluid pressure. J. Struct. Geol. 29, 1573–1584. https://doi.org/10.1016/j.jsg.2007.07.015

Leonard, M., 2010. Earthquake fault scaling: Self-consistent relating of rupture length, width, average displacement, and moment release. Bull. Seismol. Soc. Am. 100, 1971–1988. https://doi.org/10.1785/0120090189

J.G. Ramsay, R.J. Lisle. The techniques of modern structural geology. Volume 3: Applications of continuum mechanics in structural geology, Academic Press, 2000, pp. 701–1061.

Wells, D.L., Coppersmith, K.J., 1994. New Empirical Relationships among Magnitude, Rupture Length, Rupture Width, Rupture Area, and Surface Displacement. Bull. Seismol. Soc. Am. 84, 974–1002.

Hope this helps