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Interactive comment on "The impact of earthquake cycle variability on neotectonic and paleoseismic slip rate estimates" *by* Richard Styron

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This paper presents a useful thought experiment on the impact of earthquake cycle variability on measured slip rates, and concludes that the convergence on the expected value is a function of the coefficient of variation. Overall this is a sensible conclusion. Underpinning this analysis are four assumed variants of earthquake recurrence, and a function to express the variability of slip per event. I would like to see the effect of COV isolated from the slip per event distribution (use 1m slip for every event). I would also like to a see a more quantitative comparison of COV and a convergence on the mean to back up the assertion that COV of the distribution is more important than the distribution itself. The paper would be improved by a more quantitative, empirical basis and discussion of physical processes that may drive such recurrence behavior.

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Discussion paper

There is a literature of ideas to draw upon, such as post-seismic fault reloading (Kenner and Simons, 2005), earthquake super cycles (Sieh et al., 2008; Weldon et al., 2004), isolated versus fault-network behavior (Berryman et al., 2012). Some of these ideas are discussed briefly but need more explanation. Likewise one could examine actual earthquake slip distributions (not landform offsets of historic events, which convolve landscape processes with tectonic slip) to develop an empirical basis for the slip function. Some of the scatter in slip distributions is likely due to underreported measurement uncertainty (Gold et al., 2013), and thus the cancellation of this error over multiple earthquakes should let cumulative slip converge more quickly than may be predicted from the author's model.

Line-by-line comments:

Page 1, line 4. The open interval problem is well known and attempts to quantify it do exist on case-by-case basis.

Page 1, line 13. It seems odd to characterize uncertainty due to a random distribution as epistemic. Isn't this unreported aleatory uncertainty?

Page 1, line 20: Why is marker in quotes?

Page 2, line 5. afterslip and creep also contribute.

Page 2, line 10-11. Awkward sentence. Break into two.

Page 2, line 13 and other citations: persistent use of 'e.g' after citing only one or two articles is poor form and makes this reader think that the author has not adequately explored the literature.

Page 3, line 11. This is not the correct definition of an exponential / poisson distribution. There is no prescribed number of events, only a prescribed time-independent probability. It is also worth noting that this is physically unrealistic at short time intervals because it violates elastic rebound.

Page 3, line 24. It would be useful to briefly discuss how shape and scale affect distributions generally. Shape governs the how tailed and is dimensionless; scale determines the spread of the distribution and is dimensioned (in years for this case).

Page 4, line 6. Pareto distribution is another, simpler distribution needing only shape and scale to describe COV > 1

Page 4, line 15. Akciz et al (2012) revised Grant and Sieh (1994) and found much more periodic behavior.

Page 4, line 28. The author should consider non-dimensionalizing the results of this study to facilitate more general use of its results. Instead of mean slip of 1m, one would refer to non-dimensional slip of 1 and multiply by average slip per event to scale the results. This is effectively what the author describes already, though without formal non-dimensionalization.

Page 5, line 15. The statement 'appears to be related to COV' is disappointing. Given that this paper is entirely simulation, the author should be able to make a quantitative comparison of slip-rate variance to COV.

Page 5, line 24. A 'fat' or heavy tailed distribution would not have a defined variance, nor COV. The author should refer to this as a long tail.

Page 5, line 29. This problem has been studied (Weldon et al., 2004; Sieh et al., 2008). The Sumatran subduction zone work is particularly relevant and completely overlooked here.

Page 6, line 1. Zero friction at rupture arrest is very unrealistic, and not a prerequisite for characteristic behavior.

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