

Review of Schoene et al.

Schoene and colleagues have written a thought-provoking manuscript that builds on discrepant interpretations in the literature of how the rate of eruption of basalt in the Deccan Traps large igneous province varies over time. In two papers published in the same issue of *Science*, Sprain et al. (2019) and Schoene et al. (2019) used geochronologic data to argue, respectively, for an essentially constant eruption rate over time or an eruptive history with distinctive pulses. In the present manuscript, Schoene and colleagues argue that the data behind these arguments – U-Pb in the case of Schoene et al. (2019), $^{40}\text{Ar}/^{39}\text{Ar}$ in the case of Sprain et al. (2019) – are actually consistent, and that the discrepancy between the two conclusions stems largely from assumptions associated with the Bayesian model used by Sprain et al. (2019) to model eruption rates through time. Schoene and colleagues then apply their own Bayesian model with fewer assumptions (also used for the Schoene et al., 2019 paper) to the Sprain et al. (2019) dataset. The result (Figure 4 in the submitted manuscript) is consistent with the Sprain et al. (2019) argument for a constant eruption rate. However, the uncertainty bounds on the model (what I'd call 95% credible intervals, following Gallagher, 2012, but what Schoene et al. call the 95% confidence interval) are very wide, so they don't preclude pulsed increases in eruption rate of the magnitudes inferred by Schoene et al. (2019). They go on to state that their analysis suggests that the $^{40}\text{Ar}/^{39}\text{Ar}$ dataset of Sprain et al. (2019) provides no strong evidence for an increase in eruption rate roughly coeval with most estimates of the age of the Chicxulub impact, a speculation that has appeared in several papers. Finally, the discussion section of the manuscript underscores the importance of extremely precise geochronology in studies of eruption rates (if a single chronometer is used), and both precise and accurate geochronology if multiple chronometers are used.

I think this manuscript is certainly worthy of publication in *Geochronology* with moderate, but straightforward, revision. The authors make excellent points in several parts of this version, but I think there could be some tightening of the focus. In my opinion, the most important contribution here is that the authors have shown that two different, but equally reasonable, Bayesian models of the same dataset with different underlying assumptions can yield different results that can lead to significantly different geologic inferences. A general discussion of this intuitively obvious but frequently underappreciated point would be a great service to the community. A second major

point here is that data uncertainties (and the uncertainties in models derived from them) are fundamentally important when we try to reconstruct rates of geologic processes in general. I think this point is well-enough developed in the current manuscript. I'd encourage the authors to focus almost exclusively on these two points and put only enough of the Sprain et al. (2019) and Schoene et al. (2019) controversy to set up these two discussions. (Pointing out the continuing issues regarding the "age" of the Fish Canyon sanidine standard, issues of $^{40}\text{Ar}/^{39}\text{Ar}$ and U-Pb intercalibration, and disagreements about the age of the Chicxulub impact are important controversies but adding them here seems to diffuse the impact of this manuscript in my opinion.) Such relatively minor changes in emphasis and content would help this contribution rise above something that may seem to some like an extended comment on the Sprain et al. (2019) paper.

Reference

Gallagher, K. (2012), Transdimensional inverse thermal history modeling for quantitative thermochronology, *Journal of Geophysical Research*, *117*, 2156-2202.

Specific comments keyed to lines in the submitted manuscript:

- 28 When discussing models, it is conceptually important to avoid interpreting model results as truth. I have a kneejerk negative reaction to statements to the effect that modeling results allow the authors to "conclude" something. I might suggest a little more circumspection here. The authors could replace "conclude" on this line with "infer from the results that" with no loss of impact.
- 28 I suggest changing "results in" to "implies"
- 29 I suggest adding the word "eruption" after "Deccan Traps"
- 29-30 I'd change "cannot verify or disprove" to "provide no support for, nor evidence against" or something like. By their very nature, models never verify something, and they disprove something only when the model assumptions are demonstrably correct, which is rarely true.
- 32 I'd change "supports an increase" to "supports the notion of"
- 33-36 This sentence makes an excellent point.
- 51 "Kasbohm et al., in press" should be updated when the paper is out

- 60 “On” should be “off”
- 64 Earth-changing
- 77 I’d say that the “Two datasets are consistent in that they provide unambiguous evidence that...” just to reinforce that the disagreements between Sprain et al. and Schoene et al. have less to do with the actual geochronological results in the two papers and more to do with how one infers eruptive rates from the two datasets.
- 103 I’d eliminate correct here, though I understand why it’s attractive to include it. I think it’s important to make it clear that there is nothing wrong with the depiction of data in Figure 4 of Sprain et al. The concern is that Figure 4 is not directly indicative of eruption rate through time.
- 100-104 See comments above on lines 28 and 29-30. I have a similar problem with seeing terms like “we show” and “neither confirm nor refute” in this context. It is more correct to say something like “our modeling of the datasets does not support the conclusions of Sprain et al.” and be done with it. The real issue is not whether or not the conclusions of Sprain et al. are wrong, but whether or not the model upon which those conclusions are based is better or worse than the model presented in this manuscript.
- 108 I’m not sure what is meant by a “more widely used” age for the Fish Canyon sanidine. There is indeed controversy concerning the $^{40}\text{Ar}/^{39}\text{Ar}$ age of this standard, but I’m not sure there is yet a consensus. Maybe it would be better for the authors to say the other age they are referring to, with a reference, rather than calling it more widely used.
- 131 Just to be completely clear, I’d reword this since the term “systematic errors” is sometimes used in different ways by different authors. What you mean is that any errors are common to the calculations done in both papers.
- 158 Similarly, I think the authors should be explicit here about what they mean by “systematic biases”.
- 163 I might call this “an important characteristic of this model” rather than “the main point”.
- 165 Section 3 makes some very good points that underscore both the power of modeling eruption rates and the reasons why different models, in this case both Bayesian, can produce different results. The authors here use a more parsimonious approach when it comes to *a priori* assumptions (“priors” in the Bayesian lexicon), and that may well explain most of the differences in the two models. It’s unsurprising that a model with fewer priors results in greater uncertainty that makes it impossible to discern specific pulses of volcanism (Schoene et al., 2019) or to discern robust evidence for constant rates (Sprain et al., 2019).

- 206 “Our model suggests that” is better than “We show that”.
- 207 “However, the eruption rates are” should be “However, our model and that of Sprain et al. (2019) provide quite different estimates of how eruption rate varied over time...”
- 209 The authors should explicitly state whether these precisions are at 1 or 2σ (or the percentage confidence level, if that is how the precisions are presented).
- 343 “Nailing down” seems a little too colloquial.