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Interactive comment on "An evaluation of Deccan Traps eruption rates using geochronologic data" *by* Blair Schoene et al.

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Note the formatting below: due to the necessity of uploading plaintext into the textbox (and the first author's impatience with Latex formatting), the original reviewer comments have an R: at the beginning of text written by the reviewer and an A: at the beginning of a response section. Some responses will read in the past tense for things that have already been changed because they were easy, and others will read as the future tense, to be made following the AE's response/recommendation.

R: 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), 40Ar/39Ar 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 40Ar/39Ar 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.

A: This is a good summary of our paper, and we thank Dr. Hodges for this comments and clarifications. We'd emphasize a couple points maybe not stressed in the summary above (which we therefore need to highlight in revising). The main difference that arises from the Bayesian models (Dr. Hodges is correct to call them credible intervals, we will change) is that we think the placement of the KPB in Sprain2019 is overprecisely placed "near" the Bushe-Poladpur boundary, whereas our model quantifies the possible placement of the boundary. In the end, the Ar-Ar and U/Pb datasets don't agree on this point regardless of what model one uses. Secondly, while the first author of Sprain2019 confirms in her review that they did not mean to imply an increase in eruption rates at the KPB, we perceive that this is what the community is taking from the paper because of the mislabeling of their Fig. 4. We will also revise to include the intent of Dr. Sprain in our manuscript, which in the end will help strengthen our paper.

R: 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 40Ar/39Ar 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.

A: We appreciate the recommendation to remove the FCs discussion, but in the end a) the other three reviewers seem to like this section and b) in our reading with students, they seem to have gotten a lot from this section, as they were unaware that systematic uncertainties between Ar-Ar and U/Pb could generate such large differences. We will emphasize the latter point in revising and make it clearer why we think this section is

important for the discussion.

R: 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.

A: fair enough. Changed.

R: 28 I suggest changing "results in" to "implies"

A: We do believe that you can have results from a model, just like you can have results from any experiment, that may not be Truth.

R: 29 I suggest adding the word "eruption" after "Deccan Traps"

A: changed.

R: 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.

A: can't disagree with this. Changed.

R: 32 I'd change "supports an increase" to "supports the notion of" 33-36 This sentence makes an excellent point.

A: Thanks, changed.

R: 51 "Kasbohm et al., in press" should be updated when the paper is out

A: wouldn't that be nice if it was out yet? Added ISBN and title of book it's in.

R: 60 "On" should be "off" 64 Earth-changing

A: changes made

R: 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.

A: Good point, change made.

R: 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.

A: We disagree that there is nothing wrong with the depiction of their data in Fig.4 from Sprain2019, in that it is stated to plot eruptive flux, but it doesn't. It is very difficult to put into words what one should extract from Fig. 4 of Sprain2019. For example, as an analogy, it's like if you want to depict some information about a journey made in a car. You might plot velocity as a function of time (such that integrating that curve gives you total distance). You might plot distance as a function of time, such that the slope is velocity. But you would never plot total distance covered in a period of time versus time, because the slower you cover that distance, the larger the area under that curve is for that time interval, which is totally misleading. This is analogous to their figure if you put volume in place of distance.

R: 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 that the model presented in this manuscript.

A: We have reworded along the lines suggested

R: 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 40Ar/39Ar 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.

A: It's been reworded to not make any unfounded claims about which age for FCs is more widely used.

R: 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".

A: These have both been changed to be clearer in the meaning.

R: 163 I might call this "an important characteristic of this model" rather than "the main point".

A: reworded as follows: "The main point here is that the model results from neither dataset show any evidence for an increase in eruption rate associated with the Chicxulub impact (Fig. 4, and see discussion below)." The point being that these results are not actually dependent on the model you use.

R: 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 us 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 vulcanism (Schoene et al., 2019) or to discern robust evidence for constant rates (Sprain et al., 2019).

A: agreed

R: 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..."

A: those sentences are rewritten as follows: We use the modeling exercise above to argue that neither the 40Ar/39Ar nor the U-Pb data support an increase in eruption rate in the Deccan Traps at the time of the Chicxulub impact. Whlie the average eruption rates through time are equivalent for both datasets, the model result for the 40Ar/39Ar dataset shows constant eruptions at ca. 1-2 km3/a and that for the U-Pb dataset shows pulses reaching > 10 km3/a (Fig. 4).

R: 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).

A: They are 2-sigma and that is now written.

R: 343 "Nailing down" seems a little too colloquial.

A: Trying to avoid the word constrain. . .:)

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